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Article

The Legacy Debt Associated with Past Social Security Transfers

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A number of studies have used estimates of historical and projected lifetime net transfers (benefits less taxes) by birth cohort under the Old-Age and Survivors Insurance program to calculate and compare the aggregate present-value sum of such transfers for selected birth-cohort groups. Those calculations indicate that, from a program accounting perspective, the earliest generations of program participants received large transfers from later generations of participants. Some recent studies have referred to this cumulative transfer to the earliest generations as a "legacy debt" and characterized it as a burden borne by the later generations. This article clarifies the legacy debt concept and discusses the conditions required for a legacy debt to exist in a meaningful economic sense.

Introduction

A number of studies have used estimates of historical and projected lifetime net transfers (benefits less taxes accumulated or discounted using market-based interest rates) by birth cohort under the Old-Age and Survivors Insurance (OASI) program to calculate and compare the aggregate present-value sum of those transfers for selected birth-cohort groups. Using historical and projected OASI Trust Fund interest rates, such calculations confirm that, from a program accounting perspective, the earliest generations of program participants received positive lifetime net transfers, while later generations are projected to experience negative lifetime net transfers. Some recent studies have referred to the cumulative net transfer to the earliest cohorts as a "legacy debt" and characterized it as a burden borne by later program participants. Based on that perspective, some of those studies have suggested that a portion of the legacy debt be repaid to distribute the burden more fairly across cohorts. This article clarifies various aspects of the legacy debt concept, in particular by distinguishing between "actuarial" and "real" legacy debt concepts and by identifying the conditions required for a real legacy debt to exist in a meaningful economic sense.

The next section of the article discusses the actuarial legacy debt concept and provides some estimates of its size. The subsequent two sections discuss the extent to which those actuarial estimates are meaningful indicators of any real legacy debt associated with past program transfers, either in terms of any effect of the program on private saving or in terms of the relationship between rates of return under the program and market interest rates for present and future program participants. The final section summarizes the discussion.

Selected Abbreviations

OASDI	Old-Age, Survivors, and Disability Insurance
OASI	Old-Age and Survivors Insurance
OCACT	Office of the Chief Actuary
PAYGO	pay-as-you-go

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Actuarial Measures of the OASI Legacy Debt

The notion that OASI has created a legacy debt arises because the program historically has been financed primarily on a pay-as-you-go (PAYGO) basis rather than a fully funded basis. In a fully funded program, payroll taxes paid by workers are invested in government securities or other market assets and accumulated to fund the benefits that those workers and their eligible dependents receive when they retire or become other types of beneficiaries—each birth cohort or generation of workers effectively pays for its own benefits through the accumulation of those prior investments. In OASI, by contrast, payroll taxes paid by current workers have been used largely to finance the benefits of current beneficiaries. In particular, OASI taxes collected during the early years of the program were used to finance benefits to earlier-born beneficiaries who had not paid program taxes over their entire working lives. The OASI PAYGO approach allowed relatively generous benefit payments to those earlier-born beneficiaries.[1] Diamond and Orszag (2004, 69–70) argue that these relatively generous benefits were "a humane response to the suffering imposed by World War I, the Great Depression, and World War II on Americans who came of age during those years, and it helped to reduce unacceptably high rates of poverty among them in old age" and "not only helped the recipients themselves but also relieved part of the burden on their families and friends, and on the taxpayers of that era, who would otherwise have contributed more to their support." Although they received relatively generous Social Security benefits, many of these early beneficiaries bore the burden of supporting aged parents, a burden that the program substantially lessened for later generations. In addition, the burden of supporting aged parents fell unevenly across workers in those earliest generations, while later generations benefited from the program's provision of collective insurance against many such risks faced by individuals and families.[2]

Regardless of the motivation behind or the social merits of those generous early benefits, the PAYGO financing approach had the potential to create what has been called the legacy debt, a measure of the relative generosity of the Social Security program to the earliest participant cohorts.[3] An actuarial estimate of the OASI legacy debt as of a given valuation date for a specific group of the earliest cohorts affected by the program is typically calculated as the present-value sum of aggregate accumulated historical and discounted projected lifetime net transfers (benefits less taxes) under the program for all birth cohorts in that group. The historical or projected net transfers are usually accumulated or discounted to the valuation date using the OASI Trust Fund interest rate. The present-value sum of the lifetime net transfers across all cohorts in that cohort group represents an estimate of the cumulative effect of the net transfers to those cohorts on the size of the OASI Trust Fund as of the valuation date. The expected present-value sum of lifetime net transfers for those cohorts would have been zero had the program been fully funded from the start.[4]

In mathematical terms, an actuarial valuation of the legacy debt as of the end of year T (L_T) might be expressed as

$$L_T = \sum_{c=x}^{y} \sum_{a=0}^{M} N_{c,a} \left(\frac{f_T}{f_{c+a}} \right)$$

$$\text{with} \quad f_i = \prod_{t=1937}^{i} (1 + r_t),$$

where c is the birth year of an included cohort, x is the birth year of the earliest cohort affected by the program, y is the birth year of the last cohort included in the legacy debt valuation, a is a given age from birth (0) through the maximum attainable age (M), $N_{c,a}$ is aggregate OASI net transfers across all members of cohort c at age a, r_t is the OASI Trust Fund interest rate in year t, and f_i is the accumulated OASI Trust Fund interest rate factor from the program's first year (1937) through year i. For simplicity, all net transfers are assumed to occur at year-end.

Historical program data on annual net transfers by cohort can be used for part of the actuarial legacy debt calculation, but annual net transfers by cohort beyond the historical period are typically based on projections consistent with a recent *Annual Report of the Board of Trustees of the Federal Old-Age and Survivors Insurance and the Federal Disability Insurance Trust Funds* (hereafter, the *Trustees Report*). The *Trustees Report* presents an official actuarial projection of the financial status of the Social Security program based on annually updated economic, demographic, and program assumptions. When the current *Trustees Report* projects the OASI Trust Fund to be out of long-run financial balance, projected outcomes by cohort under alternative policies designed to restore the program's projected long-run financial balance[5] could also be used in actuarial legacy debt estimates—the estimates would then be conditional on the adoption of those particular policies, to the extent that they affect the cohorts included in the estimates. Most typically, however, actuarial legacy debt estimates in the literature have included only those cohorts unlikely to be significantly affected by such future program changes.

Diamond (2004) provides a rough estimate of $11.5 trillion for the 2002 present value of the actuarial legacy debt. That estimate represents the accumulated and projected present-value sum of lifetime OASI net transfers for all cohorts born through 1949 (attaining age 55 in 2004) based on the assumption that those cohorts are unlikely to experience significant effects from future legislation to bring the program into long-run financial balance. Diamond derives this legacy debt valuation from estimates developed in Leimer (1994) of aggregate lifetime OASI net transfers under present law for individual birth cohorts, evaluated as of 1989 using the OASI Trust Fund effective interest rate.[6] Using the more recent estimates in Leimer (2007) of aggregate lifetime OASI net transfers under present law for the same birth cohorts produces an actuarial legacy debt estimate of about $11.2 trillion when evaluated as of year-end 2001.[7] The top panel of Table 1 shows that this year-end 2001 legacy debt valuation equals about 5.7 percent of the present value of future Old-Age, Survivors, and Disability Insurance (OASDI) taxable payroll[8] projected by the Social Security Administration's Office of the Chief Actuary (OCACT) over the *Trustees Report* 75-year projection period as of that valuation date.[9] The $11.2 trillion year-end 2001 present value, accumulated to year-end 2014 using the OASI Trust Fund effective interest rates between 2001 and 2014,[10] implies an actuarial legacy debt present value as of year-end 2014 of about $20.9 trillion for cohorts born through 1949. This year-end 2014 valuation represents about 5.0 percent of the corresponding present value of future OASDI taxable payroll over the 75-year projection period and about 3.2 percent of future taxable payroll over the infinite projection period.

Table 1.
OASI actuarial legacy debt estimates for two cohort groups and two year-end valuation dates

Date	Valuation (trillions of dollars)	Valuation as a percentage of the present value of future taxable payroll: 75-year projection period	Infinite projection period
		Cohorts born through 1949	
2001	11.2	5.7	--
2014	20.9	5.0	3.2
		Cohorts born through 1931	
2001	13.0	6.7	--
2014	24.4	5.8	3.7

SOURCE: Author's calculations based on Leimer (2007) estimates of OASI lifetime net transfers for individual birth-year cohorts.

NOTE: -- = not available.

The legacy debt has also been defined to include only the earliest birth cohorts that are projected to receive positive lifetime net transfers from the program when calculated using the trust fund interest rate.[11] That definition leads to a higher actuarial legacy debt estimate because the cohorts born after 1931 included in the Diamond (2004) measure are projected in Leimer (2007, Appendix Table A-1) to experience negative lifetime OASI net transfers when calculated using the OASI Trust Fund effective interest rate. The Leimer (2007) lifetime net transfer data suggest a year-end 2001 actuarial legacy debt valuation of about $13.0 trillion for the cohorts born through 1931, representing about 6.7 percent of the present value of future OASDI taxable payroll over the 75-year projection period as of the beginning of 2002 (bottom panel of Table 1). Accumulating that year-end 2001 legacy debt estimate forward to year-end 2014 using the OASI Trust Fund effective interest rates between those dates produces an estimate of about $24.4 trillion for those cohorts, or about 5.8 percent of the corresponding present value of future OASDI taxable payroll over the 75-year projection period and about 3.7 percent of future taxable payroll over the infinite projection period.

Conceptually, these actuarial legacy debt measures are related to a PAYGO program's closed group unfunded liability. That liability is typically defined as the aggregate present value of projected future program costs (including administrative expenses) less taxes over the remaining lifetimes of *all current* program participants (the closed group) as of a given valuation date, less the value of any trust fund associated with the program as of that date. As such, a closed group unfunded liability estimate represents the amount by which the program's trust fund would have to be increased to attain full funding for *current* program participants. Historical program costs and taxes are represented in the unfunded liability measure through their effect on the size of the program's trust fund as of the valuation date. OCACT publishes such estimates, referred to as the "closed group transition cost" or "unfunded obligation for past and current participants" (for example, Schultz and Nickerson 2015). However, the OCACT estimates are for the combined OASDI program rather than for the OASI program alone, and those estimates include many more recent birth cohorts than are typically included in legacy debt measures. In addition, the actuarial legacy debt is generally defined as the cumulative lifetime net transfers to a fixed group of cohorts, so that the valuation of that debt will grow over time at a rate equal to the interest rate used in the valuations—usually, the OASI Trust Fund effective interest rate. By contrast, closed group unfunded liability measures for successive years reflect a cohort group (current program participants) that changes over time. One effect of this difference is that the growth rate over time in a PAYGO program's closed group unfunded liability is more closely related to the growth rate of the program's tax base than to the trust fund interest rate.[12] As such, the OCACT closed group unfunded obligation measure, while conceptually related, has important differences from the actuarial legacy debt measure.

When considering actuarial legacy debt measures in the context of program reform, it is important to remember that the legacy debt measure is generally defined as excluding any birth cohorts that are likely to be significantly affected by future policies enacted to bring the program into long-run financial balance. By that definition, then, restoring long-run financial balance will *not* significantly change the estimated actuarial legacy debt. Any further policy adjustments designed to repay a portion of the actuarial legacy debt under these assumptions (in effect, reducing the unfunded liability of the program beyond that required for long-run solvency) would place an *additional* burden on later generations. In other words, repaying a portion of such an actuarial legacy debt as an element of program reform, as some suggest, would require larger financing adjustments than are needed simply to restore long-run financial balance. Those larger financing adjustments would place an additional burden on the cohorts putatively harmed by the legacy debt creation and consequently require justification over and above restoring long-run financial balance. Stated more generally, the actuarial legacy debt is not an indicator of the long-run financial balance of the OASI program, despite the frequent linkage of the two in the literature.[13] Even if the program were projected at present to be in long-run financial balance, a legacy debt defined over some group of the earliest cohorts would still exist and could be carried forward indefinitely without affecting the program's long-run financial status.[14]

Studies focusing on the actuarial legacy debt concept have emphasized that the legacy debt can be viewed from two different perspectives *for a PAYGO program that is in long-run financial balance*. The first perspective considers the lifetime effects of the program on selected earlier program participant cohorts (where "selected" refers to the cutoff cohort used in the legacy debt calculation); this is the predominantly backward-looking cohort perspective defined and discussed above. The second perspective considers the lifetime effects of the program on the subsequent present and future program participant cohorts, a predominantly forward-looking cohort perspective. Some studies have illustrated the equivalence of these two perspectives using the example of a PAYGO retirement program that is in long-run financial balance with tax revenues and benefit expenditures that are temporally constant proportions of economic output in a simplified theoretical economy. In the simplified theoretical economy, (1) economic and demographic growth rates and the market interest rate are constant and known with certainty and (2) the market interest rate (generally interpreted as the rate of return to capital) exceeds the economic growth rate.[15] Because the market interest rate exceeds the growth rate in economic output (and the program's tax base), the present-value sum of *all* past and future net transfers (expenditures less taxes accumulated or discounted using the market interest rate) under the PAYGO program into the indefinite future is zero under these assumptions. As such, granting positive lifetime net transfers to the earliest cohorts necessarily results in negative lifetime net transfers to later cohorts.[16]

From the perspective of the later (present and future) cohorts participating in the OASI program, the actuarial legacy debt is sometimes characterized as the present-value cost of the below-market lifetime returns (negative lifetime net transfers) that they can expect to receive from the program under such assumptions (for example, Geanakoplos, Mitchell, and Zeldes 1999; Diamond and Orszag 2004, 2005). Alternatively, from the perspective of the program or economy as a whole, the OASI legacy debt is sometimes likened to the difference between the portion of the OASI Trust Fund attributable to the cohorts included in the legacy debt calculations and the (much larger) portion that would be attributable to those cohorts if the program had been fully funded from the start (for example, Diamond 2004; Diamond and Orszag 2004, 2005).

Although illustrating a PAYGO program's potential for the relatively generous treatment of early beneficiaries, such depictions may be misleading indicators of the program's effect on later cohorts in the context of real-world business cycles, behavioral responses, and uncertain economic and demographic outcomes. From a program accounting perspective, the relatively generous benefits granted to early OASI participants clearly led to a smaller OASI Trust Fund than would have resulted if the program had been fully funded. The real economic consequences of forgoing the buildup of a larger OASI Trust Fund are not as clear—whether, for example, the smaller trust fund reflects an associated real reduction in national saving and the capital stock, as some allege. The next two sections of this article discuss how both the backward-looking and forward-looking legacy debt perspectives pose complex empirical questions that have yet to yield definitive answers. Such answers are required to determine whether the Social Security program has created a *real* legacy debt in an economically meaningful sense, rather than simply an *actuarial* legacy debt. These answers depend in part on the economic circumstances under which the program was created, the behavioral responses of program participants and their families, and the risk-and-return tradeoffs associated with the returns to market assets and the PAYGO program.

The Legacy Debt and the Capital Stock

Because it is not a fully funded program, OASI may have led to associated reductions in national saving and the capital stock. For example, if OASI participants viewed their contributions to the program as retirement saving, they may have reduced their market-based retirement saving. If so, aggregate saving could have been reduced in the absence of offsetting monetary or fiscal policies because OASI taxes were used largely to finance benefits instead of being invested in government or other market assets. That is, only a relatively small amount of OASI Trust Fund assets were created over the historical period to replace the potential reduction in privately held market assets.

That possibility does not by itself imply that the relatively generous benefits provided to early OASI participants represented bad policy. The extent to which any public retirement program builds up a partial or full trust fund represents an intergenerational equity and fiscal policy decision that must be made in the context of the social and economic conditions prevailing at the time that the program begins or, more generally, over the entire course of the program. OASI began at a low point in the business cycle with an aged population in special need of financial assistance. Under such business-cycle and social conditions, the establishment of a PAYGO program that distributes early program taxes to early beneficiaries instead of saving those taxes in a trust fund might both (1) have desired intergenerational equity effects and (2) stimulate consumption and other economic activity, eventually resulting in more, not less, income and capital in subsequent periods. Such increases in private and total societal income and wealth would not be fully captured by the program and reflected in the trust fund under these conditions, of course, but could exist nonetheless.

There are other reasons why PAYGO retirement programs such as OASI do not necessarily create a real legacy debt in the form of a lower capital stock, even though the trust fund assets of such a program will be lower in an accounting sense than those in a fully funded program.[17] As a result, the existence or nonexistence of a full reserve fund in a retirement program does not by itself indicate its historical effect on national saving. For example, a program with no trust fund may have increased national saving by stimulating economic activity at a low point in the business cycle. Alternatively, a program with a full reserve fund may have had no effect or only an attenuated effect on national saving, depending on economic conditions, the behavioral responses of consumers, and the associated monetary and fiscal policy at the time of the creation of the fund. The same observations would apply, of course, to large private pension programs or other private or public economic activities that have broad macroeconomic effects.

Consequently, empirically determining the effects of public retirement programs (or other large-scale economic activities) on national saving can be very difficult, whether or not the programs or activities were fully funded and whether or not they created explicit or implicit debt from an accounting perspective. Although many studies have examined this issue in the case of the present Social Security program, the historical effect of the program on national saving still remains an open empirical question. In short, OASI did not necessarily create a real legacy debt in the form of a lower capital stock. Diamond (2004, especially 15–17) provides a succinct and excellent discussion of these issues in the context of the OASI legacy debt.

However, even if the Social Security program is believed to have created a real legacy debt in the form of a lower capital stock, other considerations may suggest that any policies designed to increase the capital stock should be implemented outside the program. Those other considerations include desirable characteristics of the program, possibly even aspects of the program's PAYGO financing itself, as discussed in the next section.

The Legacy Debt and the Rate of Return to Social Security

From the perspective of its participants, the PAYGO financing of a public retirement program effectively creates a new retirement saving "asset" associated with the benefit rights generated by program tax payments.[18] The average rate of return to this asset (that is, the average lifetime rate of return for program participants) in a mature PAYGO retirement program that is in long-run financial balance is often lower than the average rate of return to many financial assets, including the projected interest rates typically used in legacy debt calculations.[19] As a result, discussions in the popular press and many technical papers often treat the PAYGO financing of mature public retirement programs as a poor "investment" choice from the perspective of the later-born participants. In this view, a legacy debt arises from the forced participation of later birth cohorts in a PAYGO program that tends to pay below-market rates of return. As indicated earlier, such a legacy debt can be measured actuarially as the expected present-value cost of the below-market returns over the lifetimes of the later birth cohorts.

Interestingly, that view, on which notions of the existence of a legacy debt are often based, is generally inconsistent with the historical record. The relationship between (1) the growth rates of economic aggregates that are tax-base candidates for a PAYGO retirement program and (2) analogous market-based interest rates, such as the rates of return to trust fund assets or to intermediate- or long-term government bonds, can be complex. That complexity arises from a variety of factors, including monetary and fiscal policy overlaid on changes over time in economic and demographic conditions. To illustrate using historical data, let aggregate wages and salaries represent a possible tax base for a PAYGO public retirement program.[20] As discussed earlier, the rate of growth over time in a mature PAYGO program's tax base is a prime determinant of the typical lifetime rate of return for later cohorts that have participated in the program over their entire lifetimes, assuming the program is kept in long-run financial balance. Over the period for which OASI Trust Fund interest rate data were available as of this writing (1940–2014), the average annual real rate of growth in aggregate wages and salaries was 3.2 percent, compared with a much lower average annual real effective rate of return of 1.6 percent to OASI Trust Fund assets (Table 2).[21] The annual real growth rate in aggregate wages and salaries exceeded or equaled the annual real OASI Trust Fund effective interest rate in 56 percent of the years during that period. Geometric mean real annual rates over that period were similar, 3.1 percent for the aggregate wages and salaries growth rate and 1.5 percent for the OASI Trust Fund rate of return. Although the disparity was not as large, the average annual real rate of growth in aggregate wages and salaries was also frequently larger than average annual real total rates of return to intermediate- and long-term government bonds over the longer historical period for which those data were available (1930–2014). The real growth rates in aggregate wages and salaries exceeded or equaled the intermediate- and long-term government bond rates of return in more than half of the years during that period. As depicted in Table 2, the average annual real rate of growth in aggregate wages and salaries over the period 1930–2014 was 3.0 percent, while the average annual real total rates of return to intermediate- and long-term government bonds were 2.4 percent and 3.1 percent, respectively. The geometric mean real annual rate of growth in aggregate wages and salaries over that period was 2.9 percent, while the corresponding geometric mean real annual intermediate- and long-term government bond rates were 2.0 percent and 2.4 percent, respectively. Employee compensation is another possible wage-related tax base for a PAYGO public retirement program; Table 2 shows similar but somewhat larger differentials for that economic aggregate.[22]

The historical outcomes depicted in Table 2 might be a poor guide to future outcomes, of course—opinions on the probable nature of these relationships in future years differ considerably. Nevertheless, it is interesting to note the general inconsistency of the assumptions used in the actuarial legacy debt calculations—assumptions that give rise to the very notion of a legacy debt—with the historical record as illustrated in Table 2.

More broadly, the question of the existence of an economically meaningful legacy debt persists even if one assumes that future PAYGO program rates of return will generally fall below analogous

Table 2.
Real annual growth rates for selected economic aggregates compared with selected market-based real annual interest rates, by historical period (in percent)

Type of economic aggregate or interest rate	Arithmetic mean	Geometric mean	Percentage of years in which the interest rate is exceeded by or equals the real annual growth rate of— Wages and salaries	Employee compensation
			OASI Trust Fund (1940–2014)	
Economic aggregate				
Wages and salaries	3.2	3.1	. . .	. . .
Employee compensation	3.4	3.3	. . .	. . .
Interest rate				
OASI Trust Fund effective rate	1.6	1.5	56.0	58.7
			Government bonds (1930–2014)	
Economic aggregate				
Wages and salaries	3.0	2.9	. . .	. . .
Employee compensation	3.3	3.2	. . .	. . .
Interest rate				
Government bonds				
Intermediate-term	2.4	2.0	57.6	57.6
Long-term	3.1	2.4	54.1	56.5

SOURCE: Author's calculations.

NOTES: The historical periods reflect the years for which corresponding interest-rate data were available as of the analysis date.

. . . = not applicable.

market-based interest rates or if one compares PAYGO program rates of return to the much higher rates of return to capital (rates which also exhibit higher intertemporal variability). Standard financial analysis indicates that portfolio diversification can lead to optimum portfolios that include certain low-return assets. Diversifying a nation's total retirement asset portfolio through the addition of a PAYGO program, for example, might actually increase the expected rate of return of that portfolio over a broad range of risk, even if the PAYGO program's rate of return generally falls below market-based rates of return. Depending on the interrelationships among market asset and implicit PAYGO program rates of return, a PAYGO program with a relatively low rate of return might still be an attractive "asset" comprising a significant share of a nation's total retirement portfolio. The recent turmoil in the financial and housing markets emphasizes the potential advantages of including a retirement income asset that is less volatile than many market-based assets.

A number of studies have suggested that the potential attractiveness of including a PAYGO program in a nation's retirement portfolio is consistent with historical data for the United States. For example, Leimer and Pattison (1998) present a standard mean-variance analysis of historical annual real rates of return over the period 1930–1997 for six broad financial asset classes[23] with and without a PAYGO retirement program asset (represented by the real annual growth rate in aggregate employee compensation). They find that the PAYGO program asset comprised a dominant share (that is, the largest asset share, as high as 70 percent) of the highest-return asset portfolio across nearly half of the historical range of standard deviations. They find similar results, some with even higher PAYGO program asset shares, for the post–World War II period (1947–1997) and the more recent subperiod 1974–1997.

The appendix of this article adopts the general approach of Leimer and Pattison (1998) but uses data for the period 1930–2009. Those data include annual real total rates of return to the same six broad financial asset classes with and without a PAYGO program asset represented by the real annual growth rate in aggregate wages and salaries.[24] When asset shares are constrained to be nonnegative, the (standard deviation, mean rate of return) coordinates for the PAYGO program asset fall slightly outside the portfolio efficiency frontier that is attainable without that asset (Appendix

Chart A-1).[25] Based on this metric, the PAYGO program asset by itself is superior to *any* combination of the included financial asset types at that level of portfolio risk—from an alternative perspective, the PAYGO program asset cannot be replicated by any combination of these market assets. More generally, the inclusion of the PAYGO program asset in the retirement asset portfolio shifts the portfolio efficiency frontier outward in this analysis across essentially the entire range of standard deviations. The PAYGO program asset constitutes a dominant share of the optimum portfolio over much of that standard deviation range, reaching a maximum optimum share of over 55 percent (Appendix Chart A-2).[26]

Again, such results illustrate how a PAYGO program might represent an important, even dominant, share of a nation's optimum retirement portfolio over a substantial portion of the portfolio risk range. This "portfolio-enhancing" potential may be particularly important over a broad lower portion of that range, which is especially relevant for a public program such as Social Security that is intended to provide a modest but predictable base of retirement income, consistent with a relatively conservative investment strategy.[27] These results are only illustrative, of course, given the limitations of the mean-variance approach,[28] the limited extent of the historical record, uncertainty concerning the long-run relationships between market rates of return and the growth rates in potential PAYGO program tax bases, and the effect of economic policy on those relationships.

Studies using other approaches have also suggested that potential welfare gains might be associated with PAYGO retirement programs in the context of real-world stochastic asset returns. In their estimates of consumer expenditure functions based on cross-sectional data, Leimer and Richardson (1992) find that consumers may associate a negative risk premium with the implicit Social Security "asset," a result consistent with the premise that Social Security reduces overall portfolio risk. Using various parameterizations of a mean-variance model for several countries in the context of stochastic asset returns, Dutta, Kapur, and Orszag (2000) illustrate the potential risk-diversification advantages of unfunded pension systems. Some analyses based on overlapping generations (OLG) models find that PAYGO programs can be portfolio enhancing in the context of economic uncertainty (for example, Enders and Lapan 1982; Gordon and Varian 1988; Barbie, Hagedorn, and Kaul 2000; Krueger and Kubler 2002, 2006; Matsen and Thøgersen 2004; de Ménil, Murtin, and Sheshinski 2006; and Gottardi and Kubler 2011).[29] Other analyses based on OLG models but using different assumptions and welfare criteria do not support a conclusion that PAYGO social security programs improve welfare, often because of potential reductions in capital accumulation. More generally, Leimer (2011) notes that the potential risk-sharing advantages of a PAYGO retirement program can be obtained without the potential crowding-out effect on capital accumulation, through program design or offsetting economic policy.

Several studies have used derivative pricing techniques drawn from finance theory to estimate various market valuations related to the PAYGO asset implicit in the present Social Security program. The empirical results of this approach for dealing with stochastic asset returns, however, have yielded conclusions that are remarkably inconsistent across and even within some of the analyses, reflecting an extreme sensitivity to different assumptions and approaches. Blocker, Kotlikoff, and Ross (2008) conclude that a market valuation of Social Security's net retirement liability for a sample of working-age Americans exceeds the standard actuarial valuation by almost one-quarter. One might interpret that result as indicating that Social Security is portfolio enhancing in that an estimated market valuation of discounted prospective program benefits less taxes for participants is larger than that implied by a standard actuarial valuation. That outcome is analogous to using a rate that is lower than the actuarial interest rate in the valuation of prospective Social Security net transfers. Geanakoplos and Zeldes (2010) find that their market valuation of accrued benefits for a sample of workers and beneficiaries is only about four-fifths of that implied by standard actuarial valuation, suggesting that prospective Social Security benefits reflect greater risk in a market valuation than in a standard actuarial valuation.[30] However, the authors also note that preliminary results of an extended analysis that estimates market valuations of open group transition cost measures[31] incorporating future Social Security contributions as well as future benefit accruals show a larger deficit than that implied by a standard actuarial valuation. One might interpret that result as consistent with the view that the Social Security program is portfolio enhancing (again, analogous to using a lower-than-actuarial interest rate in the valuation). Koehler and Kotlikoff (2009) estimate a market valuation of the infinite-horizon open group liability for the OASDI program, treating the growth rates of OASDI aggregate benefits and taxes as implicit securities that are spanned by the returns on marketed securities. Based on their preliminary results, the authors conclude that a market valuation of Social Security's open group liability may be many times larger than the standard actuarial valuation. Again, one might interpret such a result as supporting the view that the program is portfolio enhancing. However, Koehler and Kotlikoff's estimates are extremely sensitive to the alternative assumptions and methods applied as well as to the set of included market assets, resulting in radically different valuations ranging from the infinitely negative to the infinitely positive and underscoring the preliminary and difficult nature of these analyses.

The discussion thus far has focused largely on the portfolio-enhancing potential of a PAYGO program as seen from a purely financial perspective. In addition, Social Security provides social insurance that is unavailable or imperfectly available in private markets. These "market-improving" provisions of Social Security include the automatic inflation adjustment of benefits after entitlement without the default risk that would be associated with analogous insurance offered by private insurance firms, the effective provision of fair annuities without the inefficiencies of adverse selection,[32] and insurance against various types of human capital and earnings risks deriving from the redistribution of lifetime resources based on lifetime earnings outcomes. Other Social Security provisions are market improving in the sense that they address societal adequacy and equity concerns arising from undesirable market outcomes.[33] Diamond (2004) and Aaron (2011) provide excellent and more thorough discussions of Social Security's insurance features.[34]

The portfolio-enhancing potential and market-improving provisions of the primarily PAYGO OASI program may support a different interpretation of the legacy debt. OASI might actually represent an attractive "investment" option for present and future program participants based on its broader portfolio-enhancing and market-improving effects, despite offering a lower rate of return than some market assets. The extent of the potential portfolio-enhancing effect remains an open question that requires further empirical refinement.

Conclusion

This analysis aims to clarify aspects of the legacy debt concept, which arises from the PAYGO funding of the OASI program. Although the program may have created a real legacy debt borne by later program participants in the form of a lower capital stock or below-market lifetime rates of return, it is also possible that no real legacy debt was created, or that it is substantially smaller than is often suggested. Actuarial legacy debt estimates might considerably overstate the extent to which the program has affected national saving and the capital stock when the economic circumstances existing at the program's inception, the effect of alternative policies that might have been adopted at that time to address societal equity concerns, and the possible behavioral responses of consumers are considered. The PAYGO program asset and the rates of return that the program generates for current and future participants might represent a desirable addition to a nation's retirement asset portfolio, rather than a burden to be borne. In addition, programs such as Social Security can ameliorate a variety of risks by providing social insurance that is unavailable or imperfectly available in private markets. Together, these portfolio-enhancing and market-improving program effects may offset or exceed actuarial measures of the legacy debt, so that any real legacy debt associated with the program is substantially lower or nonexistent.

Empirical research has thus far failed to provide definitive evidence concerning the size and nature of these potential opposing effects and their implications for the existence of a real, rather than simply an actuarial, legacy debt. Evidence can be found to support virtually any interpretation of the size and even the direction of a real legacy debt. Some analyses suggest that standard actuarial valuations of the legacy debt may be substantial overestimates or even be of the wrong sign while other analyses suggest that actuarial valuations may substantially underestimate the size of the real legacy debt.

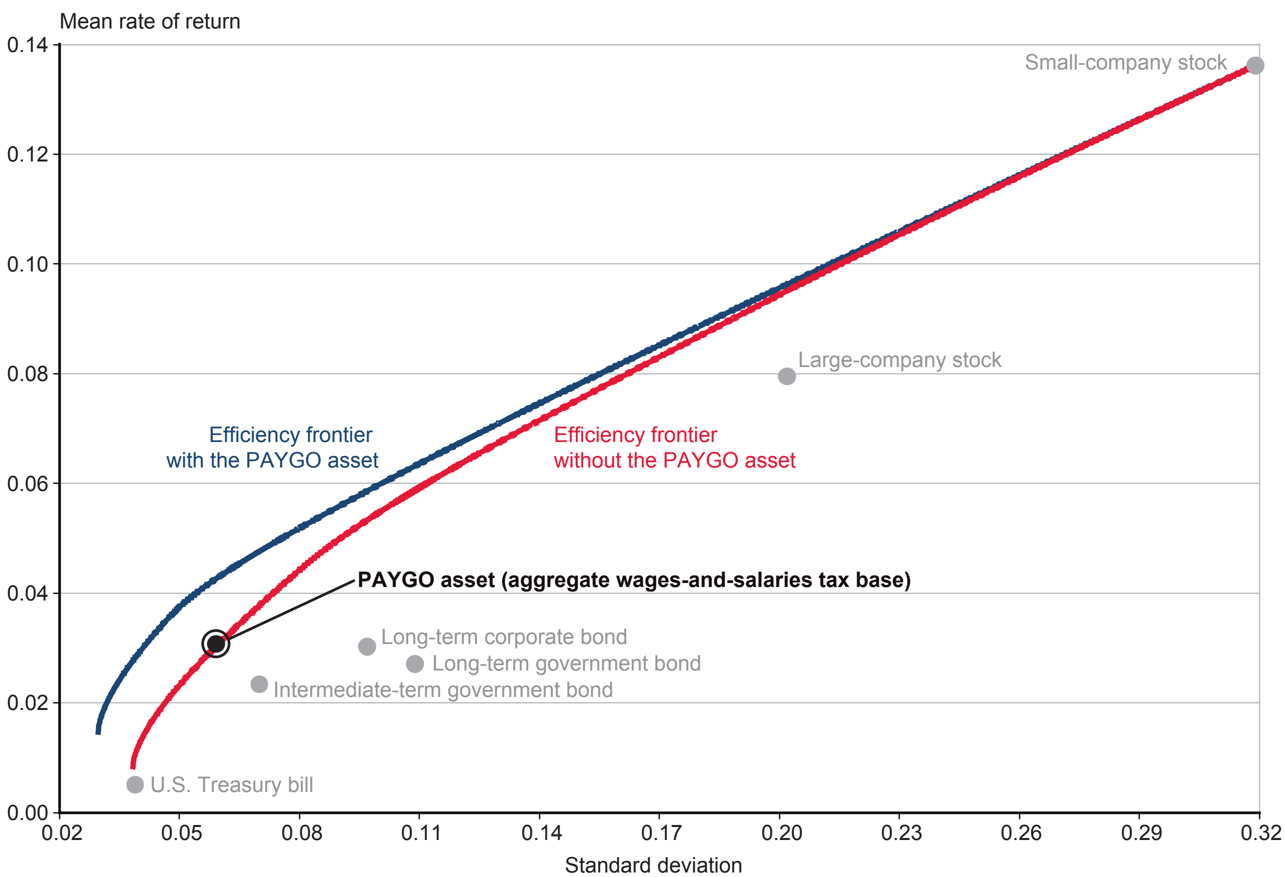
Depending on one's interpretation of the available evidence, this analysis may provide insight into the debate about how to restore long-run financial balance to the OASI program, even though the actuarial legacy debt is not an indicator of that balance. Simply restoring long-run financial balance would not have a significant effect on the actuarial legacy debt as generally defined. However, future benefit reductions designed to restore long-run program solvency may be more palatable if one believes that the program has created a real legacy debt that should not be expanded. Alternatively, future tax increases designed to restore long-run program solvency may be more palatable if one believes that the program's potential portfolio-enhancing and market-improving effects are real and worth preserving or perhaps expanding; in that case, any policies designed to increase national saving might best be implemented outside the program, even if one believes that the program has contributed to a reduction in the capital stock.

Appendix: Optimum Mean-Variance Retirement Portfolios With and Without a PAYGO Program Asset

The analysis presented here adopts the general approach used in Leimer and Pattison (1998) to calculate optimum retirement asset portfolios using standard mean-variance analysis of annual real total rates of return to six broad financial asset classes and a PAYGO program asset. The present analysis updates the 1998 analysis using data for the period 1930–2009 (instead of 1930–1997) and uses annual real growth rates in aggregate wages and salaries (instead of employee compensation) to represent rates of return to the PAYGO program asset.[35] The included financial assets in both analyses are U.S. Treasury bills, intermediate-term government bonds, long-term government bonds, long-term corporate bonds, small-company stocks, and large-company stocks.[36] Charts A-1 and A-2 display the results of the mean-variance portfolio analysis (restricted to non-negative asset shares).

Chart A-1 displays the portfolio efficiency frontiers with and without the PAYGO program asset. The efficiency frontier represents the portfolio mix that provides the highest mean return for a given standard deviation or, from a different perspective, the portfolio mix that provides the lowest standard deviation for a given mean return.[37] The gray circular data points in Chart A-1 represent the (standard deviation, mean rate of return) coordinates for each of the six financial asset types over the period 1930–2009, and the black data point represents the coordinates for the PAYGO program asset over that period. For these data, the (standard deviation, mean rate of return) coordinates for the PAYGO program with a wages-and-salaries tax base lie slightly beyond the portfolio efficiency frontier that is attainable without the PAYGO program asset. Based on this metric, the PAYGO program asset by itself is

Chart A-1.
Portfolio efficiency frontiers with and without a wages-and-salaries PAYGO asset based on annual real rates of return to six broad financial asset classes, 1930–2009



SOURCE: Author's calculations.

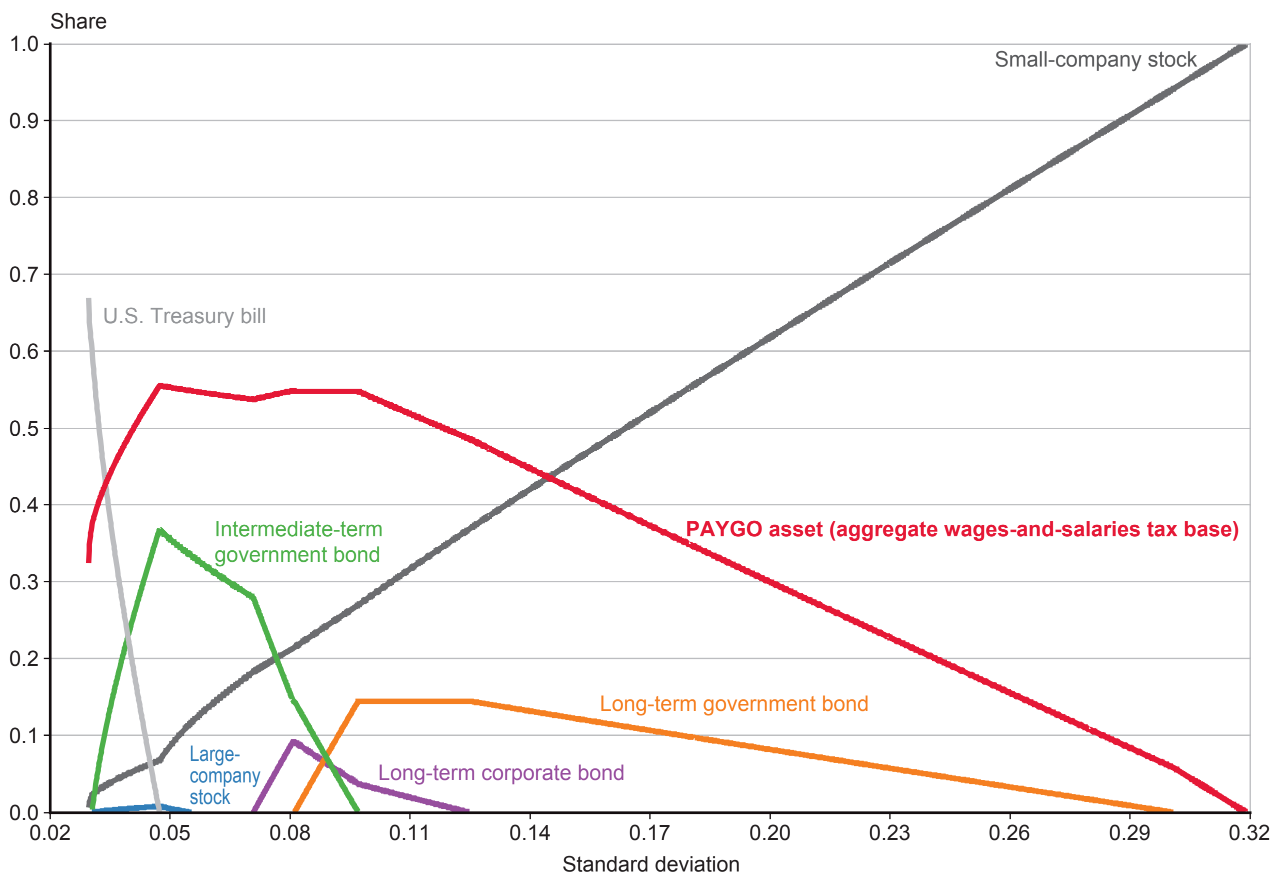
NOTE: The mean-variance analysis is constrained to exclude negative asset shares.

superior to *any* combination of the included financial asset types at that standard deviation. These results also imply that the PAYGO program asset could not be replicated by any combination of these market assets. More generally, including the PAYGO program asset shifts the portfolio efficiency frontier outward in Chart A-1 over essentially all of the relevant standard deviation range. That is, for any given level of risk (as represented by the standard deviation of portfolio returns for this historical period), including a PAYGO program with a wages-and-salaries tax base increases the attainable historical mean portfolio rate of return.

Chart A-2 shows the portfolio share of each asset on the efficiency frontier across the standard deviation range when the PAYGO program asset is included in the portfolio. The PAYGO program asset comprises a dominant share (that is, the largest asset share, reaching over 55 percent) of the optimum portfolio over much of the standard deviation range. These results suggest that a PAYGO program might represent a substantial, even dominant, share of a nation's optimum retirement portfolio mix, particularly over a broad lower portion of the standard deviation range. That lower portion is especially relevant for a public program such as Social Security that is intended to provide a modest but more predictable base of retirement income for workers, consistent with a relatively conservative investment strategy.

The analysis underlying Charts A-1 and A-2 imposes nonnegative asset share constraints, but the case for including a PAYGO program asset in the optimum portfolio in this example persists when those constraints are relaxed. When negative asset shares are allowed,[38] including a PAYGO program with a wages-and-salaries tax base shifts the attainable portfolio efficiency frontier outward as in the nonnegative share case, but the outward shift continues and increases over the entire tested standard deviation range. In

Chart A-2.
Asset shares on the portfolio efficiency frontier with a wages-and-salaries PAYGO asset and six broad financial asset classes based on annual real rates of return, 1930–2009



SOURCE: Author's calculations.

NOTE: The mean-variance analysis is constrained to exclude negative asset shares.

addition, the PAYGO program asset is the only asset with a uniformly positive and increasing asset share over the entire tested standard deviation range.

Analogous analyses using nonoverlapping historical investment periods of as long as 10 years were also examined. Although subject to decreasing confidence as the number of usable data points declined, all of those analyses supported the same qualitative conclusion as that suggested by Charts A-1 and A-2.

In addition to the limitations of mean-variance analysis, however, the limited historical record makes it difficult to determine the likelihood that analogous results would hold up over investment periods longer than 10 years—which may be relevant to retirement saving early in workers' life cycles. Moreover, historical rates of return might not be a good guide to future outcomes. There is considerable controversy, for example, over whether future equity returns are likely to be lower and riskier than in the past, and there is similar uncertainty concerning the interrelationships between future asset returns and growth rates in labor income. Nevertheless, this type of analysis does illustrate how a relatively low-return PAYGO program asset might be an attractive component of a nation's retirement portfolio, bringing the notion of a real legacy debt into question.

Notes

Acknowledgments: The author thanks Benjamin Bridges, Jr., Michael V. Leonesio, David Pattison, and Jason Schultz for commenting on the paper or discussing various aspects of the analysis.

[1] Characterizing the benefits to earlier-born cohorts as "relatively generous" is not intended to imply anything about the adequacy of those benefits relative to the needs of the early beneficiaries, only that those benefits relative to prior tax payments were generally larger than they would have been under a fully funded program.

[2] Waldron (2015, Appendix B) discusses this "familial risk" factor as part of the motivation behind the design of the Old-Age Insurance program.

[3] The term "legacy debt" appears to have been used first by Diamond and Orszag (2004)—see Aaron (2011, 397)—but the concept considerably predated that usage of the term. Leimer (1994), for example, refers to the same general concept as a PAYGO program's "start-up dividend" (33) and discusses alternative distributions of this dividend across cohorts under alternative notions of intergenerational fairness. Geanakoplos, Mitchell, and Zeldes (1999) also discuss similar concepts of lifetime redistribution under Social Security between earlier and later cohorts based on accumulated lifetime net transfers.

[4] This statement and legacy debt calculations in the literature abstract from trust fund components other than benefit expenditures and tax receipts because the data available for legacy debt calculations exclude the other trust fund components. The other trust fund components, such as administrative expenses, could be included but would require additional assumptions about how to allocate those components by cohort. However, OASI administrative expenses have become relatively small as the program has matured and likely would not have a substantial effect on the calculations if included. In 2013, for example, OASI administrative expenses were about 0.5 percent of OASI benefit payments (Social Security Administration 2015, Table 4.A1), and OASI administrative expenses accumulated over the 1940–2013 period using the OASI Trust Fund effective annual interest rates were about 1 percent of OASI benefits accumulated over that period.

[5] For examples of such estimates, see Leimer (1994, 2007).

[6] The Leimer (1994) lifetime net transfer estimates are based on historical program data and projections using a simulation model calibrated for rough consistency with the *1991 Trustees Report* intermediate assumptions. Diamond aggregates the Leimer estimates across cohorts and updates them "to present value 2002 dollars."

[7] The Leimer (2007) lifetime net transfer estimates are based on historical program data and projections consistent with the *2002 Trustees Report* intermediate assumptions.

[8] Taxable payroll is the same for the OASDI and OASI programs but is generally referred to as OASDI taxable payroll.

[9] A rough estimate of the present value of future OASDI taxable payroll over the *Trustees Report* 75-year projection period as of the beginning of 2002 can be derived from data in Schultz and Nickerson (2015, Table 1). A footnote to Table VI.F1 of the *2015 Trustees Report* provides more precise estimates of the present value of future OASDI taxable payroll over the *Trustees Report* 75-year projection period and the infinite projection period with a valuation date at the beginning of 2015. The legacy debt year-end present values discussed here are compared to the projected taxable payroll estimates as of the beginning of the subsequent year.

[10] The OASI Trust Fund effective interest rates are available by calendar year at https://www.socialsecurity.gov/oact/ProgData/effectiveRates.html.

[11] This definition is mentioned in Aaron (2011, 397).

[12] This well-known result is easily demonstrated using an overlapping generations simulation of a PAYGO program assuming constant interest rates and constant growth rates in relevant economic aggregates.

[13] This linkage may sometimes be primarily expositional or illustrative. Diamond and Orszag (2004, 38), for example, recognize that "Social Security reforms, unless they reduce benefits for current retirees (which no one today is

seriously proposing), will have only modest effects on the size of the legacy debt."

[14] Aaron (2011, 398) also makes this point.

[15] The second condition indicates that the saving rate in this theoretical economy is not so high that the rate of return to capital falls below the economic growth rate, an economically inefficient outcome. Under these assumptions, the rate of return for cohorts who have participated in the PAYGO retirement program over their entire lifetimes is equal to the growth rate in economic output, which serves as the program's tax base in these models. Samuelson (1958) and Aaron (1966) provide early analytical derivations of this well-known result. Under these theoretical assumptions, the rate of return for full lifetime participants in a PAYGO program is necessarily less than the market interest rate.

[16] Geanakoplos, Mitchell, and Zeldes (1999) provide an example to illustrate this result and conclude that: "In an unfunded PAYGO system every generation after the initial few *must* lose money in present value terms under social security. Because rates of return were high for the first generations, rates of return must be low for later generations" (86).

[17] For example, Congressional Budget Office (1998) discusses the variety of ways in which Social Security might affect personal saving. That study also provides a summary (and one interpretation) of the empirical evidence.

[18] This discussion would not hold, of course, for a fully funded public retirement program and would hold to a lesser extent for a partially funded program. For simplicity, the discussion assumes strict PAYGO financing.

[19] A PAYGO program becomes "mature" in this sense when retirees have participated in the program over their entire lifetimes. As noted earlier, the rate of return for participants in a mature PAYGO program that is in long-run financial balance tends to equal the growth rate in the program's tax base, assuming that the program has temporally constant tax-rate and benefit-rate structures in an environment of relatively constant economic and demographic growth rates. The assumption of relatively constant economic and demographic growth rates is generally consistent with the long-run assumptions in the annual *Trustees Reports,* which have served as the basis for projected outcomes in legacy debt calculations.

[20] OASI's historical tax base is not as useful for this discussion because of the multiple changes in coverage and tax rates over the program's history.

[21] The average annual real rates of return to intermediate-term and long-term government bonds over the period 1940–2014 were 1.8 percent and 2.5 percent, respectively.

[22] The nominal aggregate wage-and-salary and employee-compensation data used in these comparisons are from the Bureau of Economic Analysis National Income and Products Accounts (Table 2.1), current as of November 24, 2015. Nominal OASI Trust Fund effective interest rates are from the OCACT website cited in note 10. The nominal intermediate- and long-term government bond rate data are consistent with the year-end total return indices reported in Ibbotson Associates (2015). Real growth rates for the economic aggregates and real interest rates were derived using annual Consumer Price Index for Urban Wage Earners and Clerical Workers (CPI-W) data from the Bureau of Labor Statistics, current as of November 12, 2015. The "real growth rate" and "real interest rate" terminology used throughout this article refers to the corresponding nominal rates adjusted for price inflation.

[23] The financial asset classes in that analysis correspond to the annual total rates of return to U.S. Treasury bills, intermediate-term government bonds, long-term government bonds, long-term corporate bonds, small-company stocks, and large-company stocks.

[24] The appendix uses the real annual growth rate in aggregate wages and salaries to represent the PAYGO program asset in part because Table 2 suggests that it might show less favorable outcomes than the growth rate in aggregate employee compensation. An update and extension of the Leimer and Pattison analysis using data for the period 1930–2014 is in progress.

[25] Leimer and Pattison (1998) find a corresponding but more pronounced relationship using aggregate employee compensation as the PAYGO program tax base. The efficiency frontier identifies the portfolio mix that provides the highest mean rate of return for a given standard deviation or, from a different perspective, identifies the portfolio mix that provides the lowest standard deviation for a given mean rate of return.

[26] When negative asset shares are allowed in the analysis, inclusion of the PAYGO program asset shifts the attainable efficiency frontier outward, as in the nonnegative share case. However, (1) the outward shift continues and increases over the entire range of tested standard deviations and (2) the PAYGO program asset is the only included asset with a uniformly positive and increasing asset share over that range.

[27] From its inception, Social Security was intended to provide a retirement income foundation that workers would supplement with private pensions and personal saving (see, for example, DeWitt 1996).

[28] See, for example, Hanoch and Levy (1969) and Tesfatsion (1976).

[29] Although Krueger and Kubler (2006) find that an unfunded social security system could provide welfare-improving intergenerational risk-sharing opportunities in their model, they also conclude that the welfare improvement is likely to be more than offset by the unfunded program's potential crowding-out effect on capital accumulation. However, Gottardi and Kubler (2011) argue that that result depends on model restrictions and the particular welfare criterion applied by Krueger and Kubler. In the

Gottardi and Kubler model, intergenerational risk sharing provides a normative justification for a PAYGO social security system when markets are complete, even if one accounts for its effects on the capital stock.

[30] This result should be interpreted in the context of the authors' assumption of a close long-run correlation between average labor earnings and market assets, which, while possibly correct, limits the portfolio-enhancing potential of a PAYGO program asset based on the interrelationships among asset returns.

[31] For definitions of various closed and open group measures of the financial status of the Social Security program used by OCACT, see Schultz and Nickerson (2015).

[32] Adverse selection is the tendency of voluntary insurance programs to attract those most likely to benefit from the insurance, resulting in a higher-cost pool of program participants and effectively excluding those at lower risk.

[33] Many of Social Security's market-improving effects derive from its tax and benefit provisions and mandatory participation rather than from its financing approach. As such, those effects might also apply to an analogous fully funded public program.

[34] Other contributors include Thompson (1983), who discusses alternative insurance-model interpretations of the Social Security program and associated implications for various policy proposals. In addition to the "familial risk" factor noted above, Waldron (2015) discusses other private market failures and risks motivating the design of the Old-Age Insurance program. Leimer and Richardson (1992); Geanakoplos, Mitchell, and Zeldes (1999); Mariger (1999); and Diamond and Orszag (2005) also discuss these issues in varying detail.

[35] The nominal aggregate wage-and-salary data are from the Bureau of Economic Analysis National Income and Products Accounts (Table 2.1), current as of August 27, 2010.

[36] The annual total rates of return to the financial asset types and the price index series used to convert nominal indices to real indices are consistent with those reported in Ibbotson (2010).

[37] The efficiency frontiers and associated optimum portfolio shares in Charts A-1 and A-2 were identified by a frontier traversal method. Given a nonnegative constraint on asset shares, the asset with the highest mean return and standard deviation represents a known endpoint on the relevant portion of the efficiency frontier. Given that point, the remaining points on the frontier can be identified with arbitrary accuracy by traversing the frontier in correspondingly small portfolio share increments. These charts use a share increment of 10^{-7}. This frontier-traversal method was checked on test problems using standard Markowitz-Sharpe techniques for nonnegative portfolios.

[38] In the analysis with negative asset shares allowed, the minimum-variance portfolio mix was identified using the approach given in Campbell, Lo, and MacKinlay (1997).

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State Medicaid Eligibility and Enrollment Policies and Rates of Medicaid Participation among Disabled Supplemental Security Income Recipients

by Kalman Rupp and Gerald F. Riley*

In addition to providing income-maintenance payments to eligible participants, the Supplemental Security Income (SSI) program provides automatic Medicaid enrollment for applicants upon SSI award in most states. Other states require applicants to file a separate Medicaid application. Some use the SSI eligibility criteria for both programs; others use Medicaid eligibility rules that are more restrictive. We use matched monthly longitudinal administrative records to test whether automatic enrollment has a positive effect on Medicaid coverage. Using logistic regression with a combination of repeated cross-section and regression discontinuity approaches, we find positive effects of automatic enrollment on Medicaid coverage relative to other policies. The differences are attributable to a discontinuous increase in Medicaid coverage shortly after the final disability determination decision. The time lag arising from the often-lengthy disability determination process reduces the effectiveness of automatic enrollment, which depends critically on timeliness of the final award decision.

Introduction

The Supplemental Security Income (SSI) program is a crucial component of the social safety net for low-income adults with severe disabilities. In addition to providing federal cash payments (with optional state supplements), SSI often serves as a gateway to health insurance under Medicaid. Although SSI and Medicaid are both means-tested programs, financial eligibility for SSI is determined using standard national criteria, whereas Medicaid is administered by the states, which have considerable leeway in developing Medicaid eligibility policies. In 40 states and the District of Columbia (41 jurisdictions), SSI awardees are categorically eligible for Medicaid. In 34 of those jurisdictions, the Social Security Administration (SSA) promptly notifies the state Medicaid agency of an individual's categorical eligibility for Medicaid upon award of SSI payments using an electronic transmission process—in other words, Medicaid enrollment is automatic.[1] However, in seven of the states where SSI eligibility confers Medicaid eligibility, SSI awardees must file a separate Medicaid application.[2] Ten other states also require a separate Medicaid application and employ Medicaid income or asset limits that are more restrictive than those for SSI, with the result that some SSI recipients do not qualify for Medicaid coverage in those states.[3] Thus,

Selected Abbreviations

ACA	Affordable Care Act
CMS	Centers for Medicare and Medicaid Services
DI	Disability Insurance
SSA	Social Security Administration
SSI	Supplemental Security Income

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we observe three distinct state Medicaid enrollment policy regimes for SSI awardees:

1. automatic Medicaid enrollment of SSI awardees, initiated by SSA, and categorical Medicaid eligibility using uniform national standards to establish SSI eligibility for federal benefits (hereafter, *automatic enrollment*);
2. the requirement of a separate Medicaid application and the reliance on SSA's determination of SSI eligibility to establish categorical Medicaid eligibility (hereafter, *separate-application/nonrestrictive*); and
3. the requirement of a separate Medicaid application with Medicaid eligibility criteria that are more restrictive than those for SSI (hereafter, *separate-application/restrictive*).

Box 1 summarizes the policy regimes and lists the states that have adopted them.

Requiring a separate Medicaid application may limit Medicaid enrollment among SSI recipients for several reasons. First, it imposes a burden of additional time and effort on the applicant. Recent evidence from behavioral economics suggests that default automatic enrollment substantially increases participation (Knoll 2010). Second, requiring a separate application increases administrative complexity, which may result in processing delays and an extended period of uncertainty about ultimate eligibility for Medicaid coverage. Further, the Medicaid enrollment rates of SSI recipients in separate application/restrictive states are expected by design to be lower than those in other states, at least initially. However, automatic enrollment does not necessarily guarantee swift access to Medicaid coverage because it does not take place until after SSA determines that an SSI applicant meets the program's definition of disability, which may require more than one level of adjudication; in some cases, that process may take more than 2 years. For these and other reasons, we are particularly interested in both temporary and long-term effects of automatic enrollment on Medicaid coverage, as compared with the separate-application policy regimes.

This study examines month-to-month longitudinal dynamics of Medicaid coverage among disabled adult first-time SSI awardees who do not receive any Social Security Disability Insurance (DI) benefits. We track Medicaid coverage not only for 72 months starting with the month of SSI award but also for the 12 months

Box 1.
State Medicaid enrollment policies for SSI recipients

Policy regime	Enrollment process	SSI eligibility—[a]	States
Automatic enrollment	SSA automatically notifies state Medicaid office upon determining that an SSI applicant is eligible for SSI.	Confers categorical eligibility for Medicaid.	Alabama, Arizona, Arkansas, California, Colorado, Delaware, District of Columbia, Florida, Georgia, Iowa, Kentucky, Louisiana, Maine, Maryland, Massachusetts, Michigan, Mississippi, Montana, New Jersey, New Mexico, New York, North Carolina, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Vermont, Washington, West Virginia, Wisconsin, and Wyoming. (Sometimes called the "1634 states.")
Separate-application/ nonrestrictive	SSI applicant must file a separate Medicaid application.	Confers categorical eligibility for Medicaid.	Alaska, Idaho, Kansas, Nebraska, Nevada, Oregon, and Utah. (Sometimes called the "SSI criteria states.")
Separate-application/ restrictive	SSI applicant must file a separate Medicaid application.	Does not confer categorical eligibility for Medicaid. State uses at least one eligibility criterion that is more restrictive than those of SSI.	Connecticut, Hawaii, Illinois, Indiana,[b] Minnesota, Missouri, New Hampshire, North Dakota, Ohio, Oklahoma, and Virginia. (Sometimes called the "209b states.")

SOURCE: SSA (2014).

a. Regardless of policy regime, SSI applicants who are not categorically eligible for SSI may yet be Medicaid-eligible depending on state eligibility rules.

b. Indiana converted to automatic enrollment in 2014, after the period studied in this analysis (2001–2006).

prior to eligibility onset. Throughout the article, "Medicaid coverage" applies to individuals who are eligible for full Medicaid benefits and excludes those who receive only partial benefits under Medicare Savings Programs (such as the Qualified Medicare Beneficiary and the Specified Low Income Beneficiary programs).

In this article, we test the following four hypotheses:

- That states requiring a separate application are associated with lower Medicaid coverage than automatic enrollment states are, at least initially.
- That using restrictive eligibility rules further decreases Medicaid coverage among SSI awardees, at least initially.
- That the final determination that a working-age SSI applicant is categorically disabled produces a sharply discontinuous increase in Medicaid coverage, regardless of the enrollment policy regime.
- That the positive Medicaid coverage effect of automatic enrollment is conditional on the timeliness of the final disability determination and subsequent receipt of the first SSI payment, such that extended lags in the receipt of the first SSI payment reduce the effectiveness of automatic enrollment.

Although we are unaware of any studies that directly test these hypotheses, we note two observational studies that develop and test closely related hypotheses using cross-sectional regression methods. Ungaro and Federman (2009) hypothesize that restrictive eligibility criteria should be associated with lower probability of Medicaid enrollment among the elderly. Their point estimate is consistent with their hypothesis, but is not statistically significant. Burns and others (2012) hypothesize that lack of insurance among adults with disabilities should be positively associated with separate-application policies relative to automatic enrollment. Their regression-adjusted estimates show statistically significant differences in the expected direction. Finally, we note that there is a broader body of literature looking at the effect of Medicaid expansion on various outcomes. Some studies use data from social experiments (Baicker and Finkelstein 2011; Finkelstein and others 2012; Baicker and others 2013; Taubman and others 2014; Weathers and Stegman 2012). Some focus on Medicaid expansion under the Affordable Care Act (ACA) (Dorn and others 2013; Flowers 2010; Musumeci 2012; Rudowitz and others 2014; Sheedy and Witgert 2013; Sommers, Baicker, and Epstein 2012). Swartz and others (2015) present simulation results showing that administrative innovation might reduce Medicaid "churning" (program exits and reentries because of frequent income- or asset-related eligibility changes) and thereby increase coverage. Although our study covers a pre-ACA period, we find relevant implications for Medicaid enrollment under the ACA. In particular, because Medicaid expansion under the ACA effectively decouples SSI and Medicaid eligibility criteria in the affected "expansion states," we will discuss a simple procedural change to the current Medicaid automatic enrollment process that could dramatically increase its scope and improve its timeliness.

Data

We use administrative records from SSA's Disability Analysis File (DAF) linked to Medicaid records from the Centers for Medicare and Medicaid Services (CMS). The DAF combines program enrollment and benefit data from several SSA files on disabled beneficiaries of the SSI or DI programs (or both). From the DAF, we select a 10 percent sample of first-time SSI recipients aged 18–64 at program entry whose first month of payment eligibility was in 2000. For each sample member, we extract data on monthly benefit eligibility and actual payment status for SSI and DI from birth through 2006, state of residence at the month of first SSI eligibility in 2000, monthly survivor status for the 2000 to 2007 period, and selected demographic and diagnostic characteristics.

In this analysis, the key longitudinal data elements involve SSI payment status: specifically, whether the individual is eligible for SSI payment and whether the benefit is actually paid during a given month. Payment eligibility refers to having a payment due according to legislative design, while benefit payment refers to actual delivery by check or electronic transfer. The two may differ for a variety of reasons, most often because of the time lag involved in the initial disability determination.[4] Unlike DI benefits, SSI payments cannot be granted for months prior to application, even if the person was disabled in those months. Nevertheless, retroactivity arising from operational lags in the disability determination process applies to both programs. For practical purposes, the first month of SSI payment eligibility is the month immediately after application, and benefits are first paid shortly after the disability allowance decision. Thus, the lag between application and allowance is roughly equal to the lag between the first month of SSI eligibility and the first month of payment receipt. For that reason and others, we focus on those dates: that is, the first month of payment eligibility and the first month of actual receipt.

The sample excludes adults who received SSI disability payments as children. We also exclude recipients with any DI benefit eligibility from 2000 through 2006 because DI eligibility can affect eligibility for SSI and (indirectly) Medicaid benefits (Rupp and Riley 2011, 2012). Applicants for and recipients of concurrent SSI/DI benefits are subject to more complex eligibility rules and administrative procedures; in addition, working-age individuals at risk of DI and concurrent SSI/DI participation tend to have stronger labor force attachment and access to a wider array of health insurance options than those at risk of SSI-only participation. Moreover, because the dominant source of health insurance for SSI-only participants is clearly Medicaid, those individuals would stand to be the ones most affected by the state Medicaid policy regime (Rupp, Davies, and Strand 2008).

We link our sample file to annual Medicaid Analytic Extract (MAX) personal summary files in the CMS Chronic Conditions Data Warehouse (CMS 2016) using Social Security number, sex, and date of birth. The MAX personal summary files include data on monthly Medicaid coverage, Medicaid payments, and demographics from 1999 (the year prior to first-ever month of SSI eligibility) through 2007. Additional details on the data set and sample selection are available in Riley and Rupp (2014b).

Our analysis file includes demographic and diagnosis data for the first month of SSI eligibility in 2000. It tracks 84 months of Medicaid coverage, SSI eligibility, and SSI payment status keyed to the first month of SSI eligibility (from the 12 months preceding the first month of eligibility through a period of 72 months starting with the month of award), as well as survival status and age for each sample member at any month in that span.

Methods

First, we test the relationship between automatic enrollment and the two alternative policies using repeated cross-section modeling. We use the following equation:

$$Y_{it} = \alpha_t + X_i\beta_t + \text{POLICY}\,\gamma_t + \varepsilon_{it}, \qquad (1)$$

where Y_{it} is Medicaid coverage of individual *i* at month *t* (−12 to 72), *X* is a vector of individual characteristics, POLICY is the state-policy vector, and ε_{it} is the error term. In this formulation, $t = 1$ represents the month of first SSI payment eligibility.

Our basic model structure is similar to that of the cross-sectional models used in Ungaro and Federman (2009) and Burns and others (2012). Repeating the cross-sections provides two improvements. First, with the monthly cross-sections prior to the first month of SSI eligibility, we can test the hypothesis of separate-application effects. Any significant positive or negative coefficient for 4 to 12 months before eligibility should represent unobserved differences among the three policy regimes that cannot be explained by automatic enrollment, which is triggered by a positive disability determination that can only occur after the first month of payment eligibility. However, the results for 1 to 3 months before eligibility may be somewhat affected by the few cases in which SSI disability determination is almost instantaneous, because Medicaid coverage can be granted for up to 3 months prior to SSI eligibility. Second, our data allow 72 repeated cross-sections starting with the month of first SSI award, enabling us to analyze the temporal pattern of estimated policy-regime effects.

Next, we address how automatic enrollment in practice may affect the results from equation 1. As noted earlier, SSI payments cannot be granted for months of disablement prior to application, but eligibility can start as early as the month after application. Here we face the inconvenient but operationally inevitable fact that the complex SSI disability determination process is typically far from instantaneous. In fact, the SSI final award decision lags substantially behind the application date in many cases. Initially rejected applicants might go through multiple layers of appeal; in some cases, the final determination can take more than 2 years. Once disability has been determined, two events occur within a short span, if not simultaneously. First, the approved applicant starts to receive monthly payments (along with a retroactive lump-sum payment). Second, automatic enrollment occurs: SSA informs the state Medicaid office that the person is entitled to SSI payment and is therefore categorically eligible for Medicaid. This second step of course does not affect people living in states with separate-application policies.

The disability determination process may create a sharp discontinuity in Medicaid coverage rates that can be identified through regression analysis. From the applicant and Medicaid program perspectives, the ultimate eligibility of individuals awaiting an initial disability determination (or appealing a rejected initial determination) is uncertain. Under all three of the policy regimes, individuals can apply for Medicaid regardless of SSI application status; but costs, information gaps, and barriers to access reduce the likelihood that they will do so. Thus, we hypothesize a sharp increase in Medicaid coverage in automatic enrollment states for SSI recipients around the time of the first payment. Although coverage may also increase in the other two groups of states around the time of the first SSI payment (as SSA apprises awardees of potential Medicaid eligibility), we hypothesize a discontinuous differential increase in Medicaid coverage arising specifically from automatic enrollment. To test this hypothesis, we face two distinct challenges. The first is to demonstrate whether a sharp increase in Medicaid coverage is associated with the first SSI payment regardless of policy regime. The second is to detect any effect of automatic enrollment on Medicaid coverage conditional on the receipt of the first SSI payment. We use the following equation to address the first of these issues:

$$Y_{it} = \alpha_t + X_i\beta_t + Z_{itk}\delta_{tk} + S\zeta_t + \varepsilon_{it}, \qquad (2)$$

where Z_{itk} is a vector that equals 1 if the first SSI payment for individual i was made during month $k \leq t$, and 0 if otherwise; and S is a vector of states. Our basic hypothesis is that there is a discontinuous jump around the month of the first SSI payment ($t = k$). The state dummies factor out time-invariant fixed effects, including policy regime. Again, because we follow up for 72 months, we have ample detail on the temporal patterns associated with the combination of t and k.

The next two equations are identical to equation 1 except that the estimates are conditioned on the receipt of the first SSI payment:

$$Y_{it} \mid (\text{PAID}_{it} = 1) = \alpha_t + X_i\beta_{it} + \text{POLICY}\,\gamma_t + \varepsilon_{it}, \quad (3)$$

and

$$Y_{it} \mid (\text{PAID}_{it} = 0) = \alpha_t + X_i\beta_{it} + \text{POLICY}\,\gamma_t + \varepsilon_{it}, \quad (4)$$

where PAID_{it} is an indicator that equals 1 if a first SSI payment was made in month $k \leq t$, and 0 if otherwise.

Equation 3 is a direct test of the hypothesis of a positive effect for a subsample that is directly affected by automatic enrollment compared with two counterfactual policy-regime scenarios using the same sample-selection rule. Here, we expect positive automatic-enrollment effects of greater absolute magnitude than those estimated by equation 1.

Conversely, equation 4 tests effects on applicants whose Medicaid eligibility should not be affected by automatic enrollment. Although in this case we expect no positive automatic-enrollment effect, our expectations are otherwise somewhat ambiguous because the results are also influenced by the effectiveness of separate-application policy regimes. For example, separate-application/nonrestrictive states may use other Medicaid eligibility categories to establish disability status for Medicaid applicants whose cases SSA has not yet adjudicated and thus whose final SSI eligibility status is yet uncertain. If such cases are frequent, we should expect a negative automatic enrollment estimate from equation 4.[5]

We use logistic regression and express the results in terms of relative odds. Although our key interest is in the coverage effect of automatic enrollment, we discuss our results from the perspective of the two alternative policies and cast automatic enrollment as the counterfactual reference-case scenario. Using this approach, we can easily estimate the effect of the absence of automatic enrollment—alone and in combination with stricter Medicaid financial eligibility rules. The estimated effects of automatic enrollment relative to the separate-application policy regimes can be obtained by either changing the sign of the estimate or calculating the inverse of the odds ratios presented, depending on the specification.

In addition to Medicaid coverage, we look at two factors that may also be associated with the policy regimes: Medicaid expenditures and participant demographic characteristics. Because Medicaid expenditures vary widely, we analyze both average amounts and distributional patterns. We calculate average expenditures per awardee for the full awardee cohort, including those who did not enroll in Medicaid. For SSI recipients without Medicaid coverage and thus no record of Medicaid expenditures, we assign an expenditure amount of $0; those observations principally represent true zeros, not missing values. We examine annual and cumulative expenditures for the period 2001–2006. We expect variations in Medicaid expenditures per SSI awardee to reflect in part the financial impact of the different state Medicaid policy regimes. In the analysis of policy-regime differences in demographic characteristics, we use a difference-in-differences framework first to explore mortality then to look at associations between Medicaid policy regimes and enrollee demographic characteristics conditional on survival.

The strength of the empirical analysis depends on the data available. The internal validity of observational data such as ours may not be equal to that of data derived from social experiments. In particular, Medicaid coverage may be affected by unmeasured variables associated with the timing of disability determinations, the policy regime, or both. We attempt to address this concern by using repeated preeligibility and posteligibility cross-sections, regression discontinuity, and difference-in-differences techniques to test multiple implications of our core hypotheses. Although survey data might have a richer array of information on applicant characteristics, administrative records provide tremendous advantages for this analysis, such as greater sample size, precise monthly measurements of SSI and Medicaid participation and mortality status over several years, exact timing of key program events, and precise data on Medicaid program expenditures. Some SSA and CMS records may have failed to match (because Social Security number data are occasionally inaccurate or missing from some Medicaid records), but such problems appear to be fairly minor in the data set we use (Riley and Rupp 2014b). Again, although we cannot completely eliminate validity concerns arising from using these data, our analytic techniques have been selected in part to optimize identification.

Results

Table 1 compares awardee characteristics by state policy regime. Percentage distributions by sex and primary diagnosis are fairly similar for the three regimes. The separate-application/restrictive states had a slightly younger SSI population and slightly higher proportions of recipients diagnosed with mental and intellectual disorders than the other states.

Table 1.
Percentage distributions of 2000 SSI awardees aged 18–64, by selected characteristics and state Medicaid policy regime

Characteristic	Medicaid policy regime: Automatic enrollment	Medicaid policy regime: Separate-application/ nonrestrictive	Medicaid policy regime: Separate-application/ restrictive	Overall
Total number	8,639	394	1,730	10,763
Percentage distribution	80.3	3.7	16.1	100.0
	Percentage distributions			
Age group				
18–30	21.5	24.6	28.3*	22.7
31–45	28.6	29.7	30.1	28.9
46–64	49.9	45.7	41.6*	48.4
Total	100.0	100.0	100.0	100.0
Sex				
Women	56.1	55.6	55.4	55.9
Men	42.3	43.1	43.2	42.5
Data missing	1.6	1.3	1.4	1.6
Total	100.0	100.0	100.0	100.0
SSA primary diagnosis				
Mental impairments				
Intellectual disability	8.1	8.4	11.2*	8.6
Other mental impairments	30.5	30.5	34.5*	31.2
Nonmental impairments				
Neoplasms	9.1	7.6	10.1	9.2
Congenital anomalies	0.4	0.8	0.3	0.4
Endocrine, nutritional, and metabolic diseases	3.1	3.6	2.1	2.9
Infectious and parasitic diseases	3.1	0.8*	2.1	2.9
Injuries	3.3	2.3	3.2	3.3
Diseases of the—				
Circulatory system	9.5	7.6	7.3	9.1
Digestive system	2.2	2.5	1.9	2.2
Genitourinary system	2.1	1.3	1.8	2.0
Musculoskeletal system	13.3	13.7	11.6	13.0
Nervous system	6.2	10.7*	5.8	6.3
Respiratory system	3.6	4.6	3.3	3.6
Other nonmental impairments	0.6	1.3	0.5	0.6
Unknown	5.0	4.6	4.4	4.8
Total	100.0	100.0	100.0	100.0

(Continued)

Table 1.
Percentage distributions of 2000 SSI awardees aged 18–64, by selected characteristics and state Medicaid policy regime—*Continued*

Characteristic	Medicaid policy regime: Automatic enrollment	Separate-application/ nonrestrictive	Separate-application/ restrictive	Overall
	Standard errors			
Age group				
18–30	0.4	2.2	1.1	0.4
31–45	0.5	2.3	1.1	0.4
46–64	0.5	2.5	1.2	0.5
Sex				
Women	0.5	2.5	1.2	0.5
Men	0.5	2.5	1.2	0.5
Data missing	0.1	0.6	0.3	0.1
SSA primary diagnosis				
Mental impairments				
Intellectual disability	0.3	1.4	0.8	0.3
Other mental impairments	0.5	2.3	1.1	0.4
Nonmental impairments				
Neoplasms	0.3	1.3	0.7	0.3
Congenital anomalies	0.1	0.4	0.1	0.1
Endocrine, nutritional, and metabolic diseases	0.2	0.9	0.3	0.2
Infectious and parasitic diseases	0.2	0.4	0.3	0.2
Injuries	0.2	0.8	0.4	0.2
Diseases of the—				
Circulatory system	0.3	1.3	0.6	0.3
Digestive system	0.2	0.8	0.3	0.1
Genitourinary system	0.2	0.6	0.3	0.1
Musculoskeletal system	0.4	1.7	0.8	0.3
Nervous system	0.3	1.6	0.6	0.2
Respiratory system	0.2	1.1	0.4	0.2
Other nonmental impairments	0.1	0.6	0.2	0.1
Unknown	0.2	1.1	0.5	0.2

SOURCE: Authors' calculations based on SSA and CMS administrative records.

NOTES: Sample comprises SSI awardees who were aged 18–64 at award in 2000. Sample is limited to first-time awardees who were not DI beneficiaries before or within 72 months of SSI award. SSI awardees who were first entitled to SSI payments prior to their 18th birthdays are not included in the sample cohort.

Rounded components of percentage distributions do not necessarily sum to 100.0.

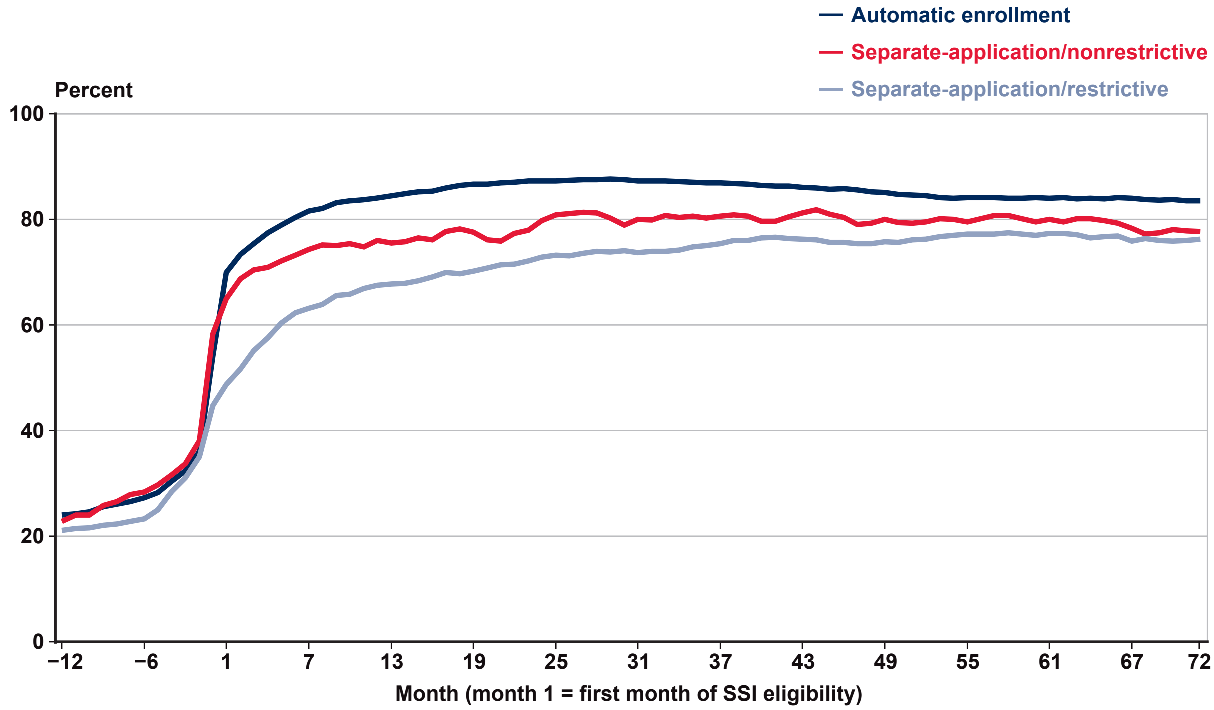
* = difference from the value for automatic enrollment states is statistically significant at the 0.05 level (two-tailed test).

Medicaid Coverage

Chart 1 shows Medicaid coverage rates by month and policy regime. Residents of separate-application/restrictive states always have the lowest levels of Medicaid coverage, while residents of automatic enrollment states usually have the highest coverage rates. Nevertheless, the temporal patterns are worth noting. The lower Medicaid coverage rates in the separate-application/restrictive states 4–12 months prior to SSI eligibility suggest some selection effect. However, the differences between separate-application/restrictive policy and the other regimes are much smaller in that period than are the posteligibility differences. This difference in differences suggests that posteligibility variation in Medicaid coverage rates may be partly attributable to different state Medicaid enrollment policies and are not spurious reflections of unrelated factors. We also observe that posteligibility differences tend to diminish through time. Thus, whereas part of the policy-regime effect seems relatively permanent, the rest might be temporary.

Next, we estimate equation 1. Table 2 presents regression-adjusted odds-ratio estimates at eight cross-sections from 12 months before SSI eligibility to the

Chart 1.
Trends in Medicaid coverage for 2000 SSI awardees aged 18–64 and alive during given month, by state Medicaid policy regime



SOURCE: Authors' calculations based on SSA and CMS administrative records.

NOTE: Sample comprises recipients of SSI payments who were aged 18–64 at award in 2000. Sample is limited to first-time awardees who were not DI beneficiaries before or within 72 months of SSI award. SSI awardees who first received SSI payments prior to their 18th birthdays are not included in the sample cohort.

end of the 72-month period starting with the month of SSI award showing the effect on Medicaid coverage of the two separate-application policies relative to automatic enrollment. We hypothesize that the separate-application/nonrestrictive policy regime should have (a) no effect on Medicaid coverage at 12 and 6 months before eligibility, with some ambiguity for 3 months before eligibility because Medicaid coverage can be retroactive at SSI award; and (b) a negative effect on Medicaid coverage relative to automatic enrollment once SSI eligibility has been established. The empirical results are consistent with both hypotheses. From month 6 to month 72, the estimated odds ratios for separate-application/nonrestrictive states are highly significant and remarkably stable, ranging from 0.61 to 0.72, suggesting a 28–39 percent permanent drop in the relative odds of Medicaid coverage relative to automatic enrollment.

For separate-application/restrictive states, we find relatively low and statistically significant odds ratios of 0.82 and 0.80 for 12 and 6 months before eligibility, respectively. These pre-SSI results suggest that, to some extent, restrictive-eligibility states may have relatively low coverage rates for Medicaid applicants for reasons unrelated to the absence of automatic enrollment. However, we do find a sharp decline in the posteligibility odds ratios, which range from 0.38 to 0.62, consistent with the hypothesis of a negative Medicaid coverage effect arising from separate-application/restrictive policy. In sum, we find evidence supporting the hypothesis that automatic enrollment increases Medicaid coverage, while the relative tightness of Medicaid eligibility rules in separate-application/restrictive states has the opposite effect.

Thus, we find that the absence of automatic enrollment depresses Medicaid coverage patterns under both of the separate-application policy regimes. Under those two policy regimes, the absence of automatic enrollment is the principal causal factor affecting difference in coverage rates. However, in separate-application/restrictive states, marginal effects also arise from the relative restrictions of Medicaid enrollment policies. In these states, the two effects are additive.

These findings are to be expected, given the implementation practices that each of the policy regimes entail. The effect of automatic enrollment becomes even clearer when we consider what it means in an operational context. Automatic Medicaid enrollment takes effect only after an applicant is *approved* for SSI disability payments; until then, the applicant does not know the eventual award outcome.[6] Therefore, automatic enrollment should have no positive effect on Medicaid coverage during the period prior to award, during which SSI eligibility remains uncertain.[7]

Table 2.
Estimated regression-adjusted odds of Medicaid coverage, by separate-application policy regime relative to automatic enrollment: Selected time points before and after date of first SSI eligibility for 2000 awardees aged 18–64

Cross-section (relative to SSI eligibility onset)	Separate-application/ nonrestrictive	Separate-application/ restrictive
12 months prior		
Odds ratio	1.01	0.82*
P>z	0.92	0.00
6 months prior		
Odds ratio	1.17	0.80*
P>z	0.20	0.00
3 months prior		
Odds ratio	1.14	0.94
P>z	0.23	0.32
3rd month		
Odds ratio	0.85	0.39*
P>z	0.15	0.00
6th month		
Odds ratio	0.72*	0.39*
P>z	0.01	0.00
12th month		
Odds ratio	0.64*	0.38*
P>z	0.00	0.00
24th month		
Odds ratio	0.61*	0.39*
P>z	0.00	0.00
72nd month		
Odds ratio	0.68*	0.62*
P>z	0.01	0.00

SOURCE: Authors' calculations based on SSA and CMS administrative records.

NOTES: Sample comprises SSI awardees who were aged 18–64 at award in 2000. Sample is limited to first-time awardees who were not DI beneficiaries before or within 72 months after SSI award; for a given cross-section, the sample is further limited to survivors aged younger than 65. SSI awardees who were first entitled to SSI payments prior to their 18th birthdays are not included in the sample cohort.

The dependent variable for each cross-section is defined as a 0–1 variable, with Medicaid coverage for a given month coded as 1 and the lack of Medicaid coverage coded as 0.

Statistics are estimated from logistic regressions. Models include controls for demographic and diagnostic variables.

* = difference from odds of Medicaid enrollment in automatic enrollment states is statistically significant at the 0.05 level (two-tailed test).

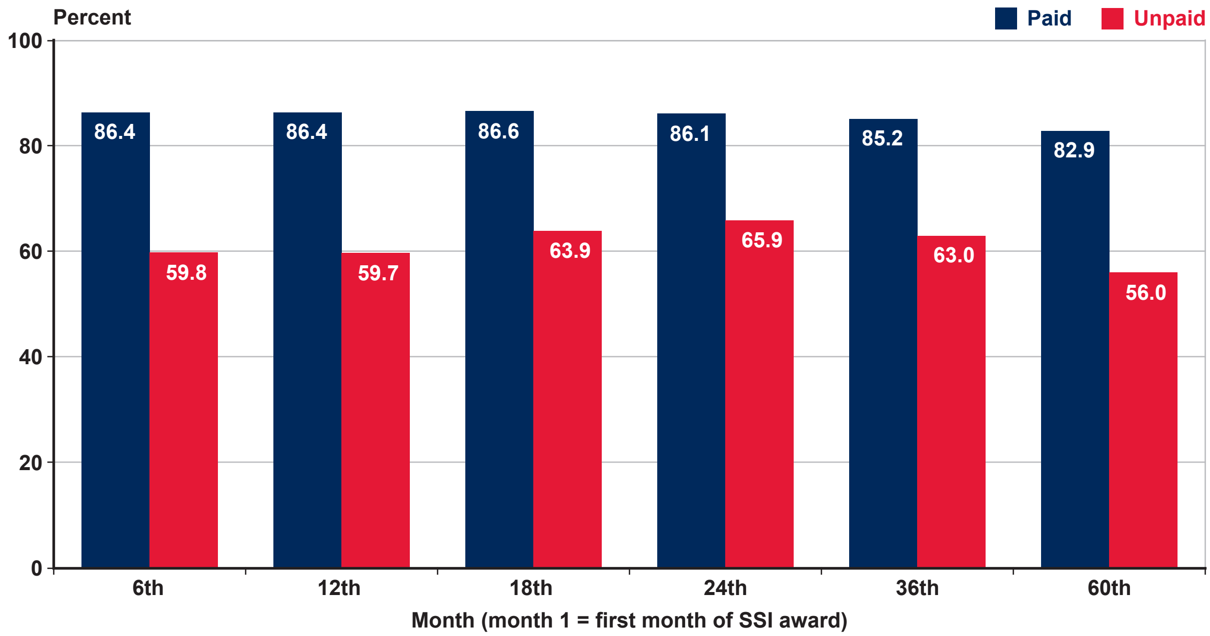
The degree to which lags in receiving the first SSI payment mediate the automatic enrollment effect is not trivial, because the SSI disability determination process is typically long. The lag between application and allowance provides an opportunity to test the effect of the timing of the first SSI payment on Medicaid coverage by disaggregating the results according to SSI payment status. Doing so explicitly addresses the effect of the lag on Medicaid coverage. We test the hypothesis that the uncertainty arising from the lag in the SSI disability determination process has a negative effect on Medicaid coverage. An eventual SSI awardee does not know the results of the final disability determination until fairly close to the first actual SSI payment. In addition, SSI applicants who were initially denied retain that status until the denial is reversed at a higher level of adjudication. Although denied SSI applicants could possibly obtain Medicaid coverage (depending on Medicaid rules and eligibility determination practices in the given state), uncertainty about categorical SSI eligibility would presumably reduce the probability of that outcome.

To assess how delays in the disability determination affect Medicaid coverage by policy regime, we continue with a two-step analysis. First, we assess whether delays in receiving the first SSI payment affect Medicaid coverage overall. Second, we address the combined effect of automatic enrollment and delays in receiving the first SSI payment. Chart 2 and Table 3 address the first step.

Chart 2 shows Medicaid coverage rates at selected cross-sections separately for those who have been paid their first benefit by the given month and those who are not. Because of sample size constraints, month 60 is the last observation we present here. As expected, across time points, coverage rates for those in paid status exceed the rates for those who are not, with differences ranging from 20 to 27 percentage points.

To further explore whether these patterns reflect causal effects, we test the hypothesized negative effect of prolonged lags between SSI application and final award decision—primarily because of the multistep disability determination process—with a regression discontinuity design. We assume a discontinuity in Medicaid coverage patterns associated with the first month of SSI payment after the final award decision

Chart 2.
Medicaid coverage during selected months after month of first SSI award: 2000 SSI awardees aged 18–64 at award, by whether first SSI benefit has been paid as of the given month



SOURCE: Authors' calculations based on SSA and CMS administrative records.

NOTE: Sample comprises SSI awardees who were aged 18–64 at award in 2000. Sample is limited to first-time awardees who were not DI beneficiaries before or within 72 months after SSI award; for a given cross-section, the sample is further limited to survivors aged younger than 65. SSI awardees who were first entitled to SSI payments prior to their 18th birthdays are not included in the sample cohort.

(equation 2). This approach provides relatively strong identification because there is no reason to assume a direct relationship between the exact timing of the first payment and state Medicaid policies or unobserved applicant characteristics. Of course, marginally qualified applicants tend to be denied initially and gain a reversal only in later stages of the disability determination process, but there is no reason to anticipate an abrupt change associated with the timing of the first SSI payment.

Table 3 presents the relevant estimates from equation 2 for nine cross-sections using logistic regression. Our dependent variable is the relative odds of Medicaid coverage at selected time points before and after eligibility by the month of first payment relative to SSI award. The reference category is first SSI payment during months 1–6. This corresponds to a lag period of 0–5 months because if first payment occurs during month 1 (month of award), the lag period is 0. In general, if first payment occurs for month z, the payment lag is $z - 1$. The paired cells of hypothesized discontinuity are outlined and the periods of actual SSI payments ($k \leq t$) are shaded. Thus, in each row, the progression from months with no payment yet to months with payment reads from right to left.[8]

For the 6th month to the 60th month cross-sections, we can observe situations when the awardee is SSI-eligible but has not received SSI payment, as well as situations when the eligible awardee has already been paid for 1 or more months. For the two preaward cross-sections, there can be no SSI eligibility or payment, by cohort definition. Therefore, the corresponding regressions serve a useful control function: For 12 months before eligibility, we should expect no effect of payment delay on Medicaid coverage; but for 3 months before eligibility, the situation is ambiguous because Medicaid can be awarded retroactively for up to 3 months prior to the first month of SSI eligibility.

The empirical results are overwhelmingly consistent with our hypothesis of a positive relationship between Medicaid coverage during month t and SSI cash payment during month $k \leq t$. The pattern of coefficients clearly shows the discontinuity we hypothesized: A substantial drop in the odds ratios appears in the transition from payment to nonpayment status periods (from shaded to unshaded cell to the right

Table 3.
Estimated regression-adjusted odds of Medicaid coverage at selected time points before and after SSI eligibility onset, by time period of first SSI payment relative to month of first-ever award: 2000 SSI awardees aged 18–64

Cross-section (relative to SSI eligibility onset)	First payment during month (month 1 = first month of SSI eligibility)					
	7–12	13–18	19–24	25–36	37–60	61 or more
12 months prior						
Odds ratio	0.98	1.08	1.32*	1.29*	1.03	1.15
P>z	0.80	0.46	0.01	0.03	0.89	0.80
3 months prior						
Odds ratio	0.84*	0.92	1.06	1.07	0.87	1.39
P>z	0.01	0.41	0.54	0.54	0.49	0.50
6th month						
Odds ratio	0.35*	0.18*	0.14*	0.12*	0.11*	0.22*
P>z	0.00	0.00	0.00	0.00	0.00	0.00
12th month						
Odds ratio	0.93	0.31*	0.17*	0.14*	0.10*	0.29*
P>z	0.40	0.00	0.00	0.00	0.00	0.01
18th month						
Odds ratio	1.01	1.06	0.32*	0.20*	0.12*	0.34*
P>z	0.91	0.64	0.00	0.00	0.00	0.02
24th month						
Odds ratio	1.05	1.55*	1.28	0.35*	0.18*	0.19*
P>z	0.59	0.00	0.07	0.00	0.00	0.00
36th month						
Odds ratio	0.91	1.19	1.28	1.53*	0.36*	0.20*
P>z	0.26	0.20	0.06	0.01	0.00	0.00
60th month						
Odds ratio	0.89	1.34*	1.37*	1.71*	1.71*	0.49
P>z	0.18	0.03	0.02	0.00	0.04	0.14
72nd month						
Odds ratio	0.96	1.32*	1.75*	1.96*	2.54*	0.90
P>z	0.60	0.03	0.00	0.00	0.00	0.84

SOURCE: Authors' calculations based on SSA and CMS administrative records.

NOTES: Sample comprises recipients of SSI payments who were aged 18–64 at award in 2000. Sample is limited to first-time awardees who were not DI beneficiaries before or within 72 months of SSI award; for a given cross-section, the sample is further limited to survivors aged younger than 65. SSI awardees who first received SSI payments prior to their 18th birthdays are not included in the sample cohort.

The dependent variable for each cross-section is defined as a 0–1 variable, with Medicaid coverage for a given month coded as 1 and the lack of Medicaid coverage coded as 0.

Statistics are estimated from logistic regressions. Models include demographic, diagnostic, and state dummy variables.

* = difference from odds of Medicaid enrollment for SSI awardees who experienced a 0–5 month lag in the SSI disability determination process is statistically significant at the 0.05 level (two-tailed test).

in each outlined pair). Values in those shaded SSI-payment cells range from 0.93 to 1.71; in the adjoining no-payment-yet cells, they drop to values ranging from 0.31 to 0.49. In effect, actual SSI payment is associated with at least a threefold increase in relative odds of Medicaid coverage, strongly supporting the hypothesis of a negative association of lags in the adjudication of SSI claims with Medicaid coverage.

For the two preeligibility time points, the adjusted relative odds are generally close to 1, differing significantly only in two lag periods for the 12-month cross-section. This finding is largely consistent with our hypothesis that the lag between eligibility onset and first payment should have no effect on Medicaid coverage 1 year prior to the first month of SSI eligibility. There is, however, a relatively small but statistically significant drop in the relative odds of Medicaid coverage at 3 months before eligibility associated with a 6–11 month lag. This finding is consistent with the Medicaid program rule that allows eligibility to be granted retroactively for 3 months prior to first month of SSI eligibility.[9]

Table 3 provides strong evidence for a negative association of delays in the receipt of the first SSI payment with Medicaid coverage overall. Next, we explore how those lags in the SSA disability determination process interact with the Medicaid policy regime.

Table 4 shows the estimated effect of state Medicaid policy regime on Medicaid coverage disaggregated by whether the first SSI monthly benefit has been paid as of a given time point. By definition, the proportion of new SSI awardees in paid status is zero during the preeligibility months, and increases as the eligibility period increases. Conversely, the proportion that has not been paid decreases over time. The null hypothesis is that automatic enrollment should positively affect Medicaid coverage (relative to both separate-application policy regimes) for SSI awardees who have been paid (equation 3). For SSI awardees who have not been paid by the given observation period (equation 4), automatic enrollment should have no effect relative to the separate-application/nonrestrictive policy regime, and some effect relative to the separate-application/restrictive policy regime.

Consistent with expectations, the empirical results show clear automatic-enrollment effects among those whose first benefit has already been paid. For SSI recipients who have been paid, the low odds ratios

Table 4.
Estimated regression-adjusted odds of Medicaid coverage, by separate-application policy regime relative to automatic enrollment and SSI payment status as of selected time points before and after SSI eligibility onset for 2000 awardees aged 18–64

Cross-section (relative to SSI eligibility onset)	First SSI payment (status as of cross-section date)			
	Separate-application/nonrestrictive		Separate-application/restrictive	
	Paid	Unpaid	Paid	Unpaid
12 months prior				
Odds ratio	. . .	1.01	. . .	0.82*
P>z	. . .	0.92	. . .	0.00
6 months prior				
Odds ratio	. . .	1.17	. . .	0.80*
P>z	. . .	0.20	. . .	0.00
3 months prior				
Odds ratio	. . .	1.14	. . .	0.94
P>z	. . .	0.23	. . .	0.32
3rd month				
Odds ratio	0.44*	0.96	0.17*	0.46*
P>z	0.00	0.76	0.00	0.00
6th month				
Odds ratio	0.45*	0.98	0.18*	0.71*
P>z	0.00	0.93	0.00	0.00
12th month				
Odds ratio	0.41*	1.42	0.25*	0.86
P>z	0.00	0.17	0.00	0.23
24th month				
Odds ratio	0.54*	1.41	0.36*	0.51*
P>z	0.00	0.47	0.00	0.00
72nd month				
Odds ratio	0.68*	. . .	0.62*	. . .
P>z	0.01	. . .	0.00	. . .

SOURCE: Authors' calculations based on SSA and CMS administrative records.

NOTES: Sample comprises SSI awardees who were aged 18–64 at award in 2000. Sample is limited to first-time awardees who were not DI beneficiaries before or within 72 months after SSI award; for a given cross-section, the sample is further limited to survivors aged younger than 65. SSI awardees who were first entitled to SSI payments prior to their 18th birthdays are not included in the sample cohort.

The dependent variable for each cross-section is defined as a 0–1 variable, with Medicaid coverage for a given month coded as 1 and the lack of Medicaid coverage coded as 0.

Statistics are estimated from logistic regressions. Models include controls for demographic and diagnostic variables.

. . . = not applicable.

* = difference from odds of Medicaid enrollment in automatic enrollment states is statistically significant at the 0.05 level (two-tailed test).

for separate-application/nonrestrictive states (ranging from 0.41 to 0.68) indicate automatic enrollment's consistently large and comparatively positive effect. As expected, the contrast between automatic enrollment and separate-application/restrictive policy is even stronger, indicated by still lower odds ratios (ranging from 0.17 to 0.62). The patterns are consistent with the hypothesis that both the lack of automatic enrollment and restrictive state eligibility policies contribute to lower Medicaid coverage in the separate-application/restrictive states.

Results for SSI applicants whose first benefit has not been paid are also consistent with our null hypotheses. The separate-application/nonrestrictive regime shows no statistically significant results relative to automatic enrollment. If anything, odds ratios tend to exceed 1.00, suggesting that automatic enrollment may actually be counterproductive for those whose SSI eligibility status is still undecided. In automatic enrollment states, SSA apprises applicants that the SSI award notice will notify them of Medicaid eligibility. Under those circumstances, SSI applicants may simply wait to receive the SSI award notice, and initiate no contact with the state Medicaid agency. It is also possible that if an SSI applicant contacts the state Medicaid office, he or she will be told that further action must await the SSI initial award decision. If an SSI applicant is initially denied, SSA refers the ineligible applicant to the state Medicaid agency, which may either find the applicant ineligible for Medicaid or grant coverage on state-level eligibility criteria. Both outcomes take time. By contrast, in separate-application states, SSA refers all SSI claimants (including those who concurrently apply for DI) to the state Medicaid office at the outset of the process. Thus, Medicaid coverage is not granted to those SSI applicants unless they actively apply; and because there is no forthcoming SSA action for which to wait, applicants have strong incentive to apply for Medicaid right away. Medicaid coverage is granted to some applicants even before categorical eligibility for SSI as disabled has been determined.

Not surprisingly, SSI awardees whose first SSI benefit has not been paid in separate-application/restricted states have lower odds ratios for Medicaid coverage relative to residents of automatic enrollment states 6 and 12 months prior to SSI award. The patterns suggest that the statistically significant coefficients for those who have not been paid SSI benefits may primarily or exclusively reflect the restrictiveness of Medicaid eligibility rules. Moreover, the pairwise comparison of the point estimates in the "paid" and "unpaid" subgroups is consistent with the expectation of differential Medicaid coverage effect by payment status.[10]

Other Potential Consequences of State Medicaid Policy

Although this article focuses on the effect of state Medicaid policy regime on Medicaid coverage, related factors are also of potential interest. The evidence presented so far strongly supports the notion that requiring a separate Medicaid application—and, in some states, also imposing restrictive state Medicaid eligibility criteria—has substantial negative effects on Medicaid coverage. We next address whether these policies also affect average Medicaid expenditures and whether there is distributional evidence of selective coverage effects for certain demographic or diagnostic characteristics. Perhaps many people brought into Medicaid by automatic enrollment have access to private health insurance or health conditions that are less severe than those of other Medicaid beneficiaries, resulting in no or very little effect on average program expenditures. At the other extreme, perhaps automatic enrollment removes barriers to coverage that affect people with severe health conditions and high expected costs. To assess the tradeoffs implied by restricting Medicaid coverage, we present empirical evidence on expenditure patterns and participant demographic characteristics by policy regime.

Expenditure patterns. Table 5 shows average Medicaid expenditures per SSI awardee for single years (2001–2006) along with the annual average for the entire period, with regression-adjusted differences by policy regime. We focus on the 2001–2006 period because 2001 was the first year in which all awardees could have had a full year of SSI-related Medicaid expenditures. Neither the raw averages nor the regression-adjusted differences for the two separate-application policy regimes are statistically significant compared with the counterfactual of automatic enrollment for any single year or for the 6-year period combined. Yet, given the substantial variance of the expenditure data, one possible reason for rejecting the null hypothesis (no difference) might be Type 2 error. After all, even for the two combined-period regression-adjusted differences, large standard errors of $624 and $322 suggest possible failure to detect relatively meaningful magnitudes of difference. Alternatively, the average expenditures may mask important distributional differences, a consideration that also calls for more detailed analysis.

Table 5.
Unadjusted and ordinary least square regression-adjusted mean Medicaid expenditures per SSI awardee by state Medicaid policy regime for 2000 SSI awardees aged 18–64, 2001–2006 (in 2012 dollars)

Year	Unadjusted value			Regression-adjusted difference from automatic enrollment states	
	Automatic enrollment	Separate-application/nonrestrictive	Separate-application/restrictive	Separate-application/nonrestrictive	Separate-application/restrictive
2001					
Mean	11,296	11,659	11,089	837	0
Standard error	300	1,003	693	1,387	719
2002					
Mean	12,248	12,691	12,413	651	253
Standard error	329	1,186	732	1,512	780
2003					
Mean	13,083	11,653	12,455	-1,376	-443
Standard error	338	911	661	1,519	785
2004					
Mean	13,578	13,024	12,640	-358	-764
Standard error	352	1,415	628	1,584	815
2005					
Mean	13,814	13,865	13,624	58	-133
Standard error	351	1,649	694	1,613	825
2006					
Mean	13,759	11,128	13,833	-2,765	382
Standard error	338	936	812	1,569	807
2001–2006 (annual average) [a]					
Mean	12,876	12,322	12,605	-393	-104
Standard error	136	489	288	624	322

SOURCE: Authors' calculations based on SSA and CMS administrative records.

NOTES: Sample comprises SSI awardees who were aged 18–64 at award in 2000. Sample is limited to first-time awardees who were not DI beneficiaries before or within 72 months after SSI award; for a given cross-section, the sample is further limited to survivors aged younger than 65. SSI awardees who were first entitled to SSI payments prior to their 18th birthdays are not included in the sample cohort.

A value of zero Medicaid expenditures is imputed for individuals who did not have Medicaid coverage in all 12 months of a given year.

Annual averages are calculated for survivors aged younger than 65 at the end of the calendar year.

a. Calculated as the sum of the annual Medicaid expenditures for each awardee divided by six. For individuals who exited the sample during a given year, a value of zero Medicaid expenditures is imputed for subsequent years.

Table 6 provides logistic regression-adjusted relative odds of Medicaid expenditures exceeding selected percentile levels ranging from the 25th to the 99th for separate-application/nonrestrictive and separate-application/restrictive states relative to automatic enrollment states. In this instance, we first examine the separate-application/restrictive states in the table's lower panel. The results are fairly straightforward: Restrictive eligibility is associated with statistically significant negative effects in each single year and for the period as a whole at the 25th and 50th percentiles. All of the estimated odds that Medicaid expenditures exceed the 90th percentile are relatively high for the separate-application/restrictive policy regime, but the 2004 odds ratio is not statistically significant. In no single year are the estimated odds that expenditures exceed the 99th percentile statistically significant, and the point estimates show no clear pattern. The same is true for the entire period. Thus, there is some uncertainty about the presence of a positive relationship at the upper tail; the results are clearly inconsistent with policy concerns about Medicaid access among the severely disabled in restrictive-eligibility states that would imply the exact opposite of what we find.

Table 6.
Estimated regression-adjusted odds that Medicaid expenditures will exceed selected percentile levels, by separate-application policy regime relative to automatic enrollment, for 2000 SSI awardees aged 18–64, 2001–2006

Year	25th percentile		50th percentile		75th percentile		90th percentile		99th percentile	
	Odds ratio	Standard error	Odds ratio	Standard error	Odds ratio	Standard error	Odds ratio	Standard error	Odds ratio	Standard error
					Separate-application/nonrestrictive					
2001	0.96	0.12	1.28*	0.14	1.29*	0.16	1.52*	0.26	0.32	0.33
2002	0.97	0.13	1.08	0.12	1.14	0.15	1.33	0.24	1.01	0.61
2003	0.90	0.12	1.12	0.13	0.95	0.13	0.81	0.17	--	--
2004	1.01	0.14	1.24	0.15	1.19	0.16	0.69	0.16	1.00	0.60
2005	0.93	0.13	1.20	0.15	1.03	0.15	0.87	0.19	0.74	0.54
2006	0.67*	0.09	0.92	0.12	0.87	0.13	0.88	0.20	--	--
2001–2006 (annual average) [a]	0.91	0.05	1.11*	0.05	1.13*	0.06	0.95	0.08	0.44*	0.16
					Separate-application/restrictive					
2001	0.51*	0.03	0.61*	0.04	0.94	0.06	1.27*	0.12	1.22	0.33
2002	0.54*	0.03	0.69*	0.04	0.98	0.07	1.38*	0.13	1.07	0.31
2003	0.58*	0.04	0.76*	0.05	0.87*	0.06	1.22*	0.12	1.15	0.33
2004	0.67*	0.05	0.74*	0.05	0.94	0.07	1.08	0.11	0.97	0.30
2005	0.67*	0.05	0.80*	0.05	0.99	0.07	1.26*	0.13	0.88	0.29
2006	0.63*	0.04	0.77*	0.05	1.02	0.08	1.26*	0.13	1.27	0.39
2001–2006 (annual average) [a]	0.60*	0.02	0.73*	0.02	0.97	0.03	1.24*	0.05	1.09	0.13

SOURCE: Authors' calculations based on SSA and CMS administrative records.

NOTES: Sample comprises SSI awardees who were aged 18–64 at award in 2000. Sample is limited to first-time awardees who were not DI beneficiaries before or within 72 months after SSI award; for a given cross-section, the sample is further limited to survivors aged younger than 65. SSI awardees who were first entitled to SSI payments prior to their 18th birthdays are not included in the sample cohort.

A value of zero Medicaid expenditures is imputed for individuals who did not have Medicaid coverage in all 12 months of a given year.

Statistics are estimated from logistic regressions.

-- = not available (insufficient sample size).

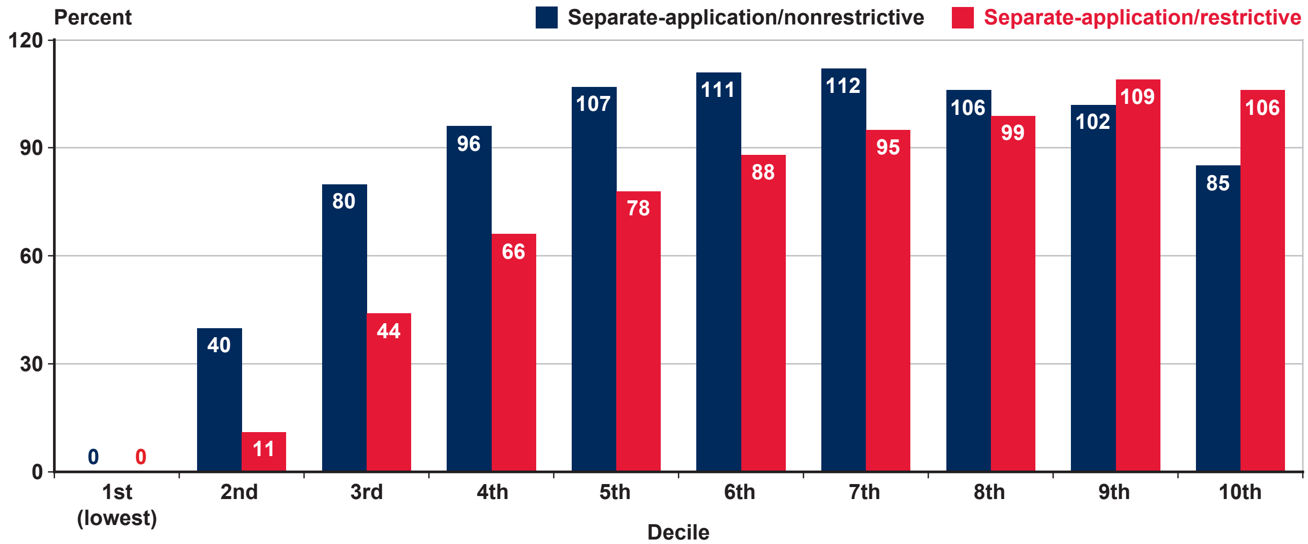
* = difference from odds of Medicaid expenditures in automatic enrollment states is statistically significant at the 0.05 level (two-tailed test).

a. Calculated for each component year in constant dollars, for all persons in the sample regardless of exit status. For individuals who exited the sample during a given year, a value of zero Medicaid expenditures is imputed for subsequent years.

In the upper panel of Table 6, we find no clear evidence of major distributional effects for separate-application/nonrestrictive states relative to automatic enrollment, but two patterns are notable. First, the fact that all but the 2004 estimated odds that expenditures will exceed the 25th percentile are smaller than 1 suggests a negative effect of that policy regime on Medicaid expenditures at the lower tail. Second, the estimates suggest statistically significant positive effects on Medicaid expenditures at the 50th, 75th, and 90th percentiles for the first full year, 2001. That finding is consistent with the previously discussed possibility that the separate processing of Medicaid applications may help people who are initially denied SSI payments and who therefore do not begin receiving payments until sometime later.

Chart 3A shows average cumulative Medicaid expenditures for the period 2001–2006 in separate-application/nonrestrictive and separate-application/restrictive states as percentages of the average expenditure in automatic enrollment states, by expenditure decile. The percentages for the separate-application/nonrestrictive states are relatively low in the lower deciles, but the overall effects are unclear, suggesting that the scarcity of statistically significant differences seen in the upper panel of Table 6 may simply reflect the absence of meaningful overall effects and not a Type 2 statistical error.

Chart 3A.
Average Medicaid expenditure per awardee in states with separate-application policies as a percentage of average expenditure per awardee in automatic enrollment states: By expenditure decile, 2001–2006



SOURCE: Authors' calculations based on SSA and CMS administrative records.

NOTES: Sample comprises SSI awardees who were aged 18–64 at award in 2000. Sample is limited to first-time awardees who were not DI beneficiaries before or within 72 months of SSI award. SSI awardees who were first entitled to SSI payments prior to their 18th birthdays are not included in the sample cohort.

A value of zero Medicaid expenditures is imputed for individuals who did not have Medicaid coverage in all 12 months of a given year.

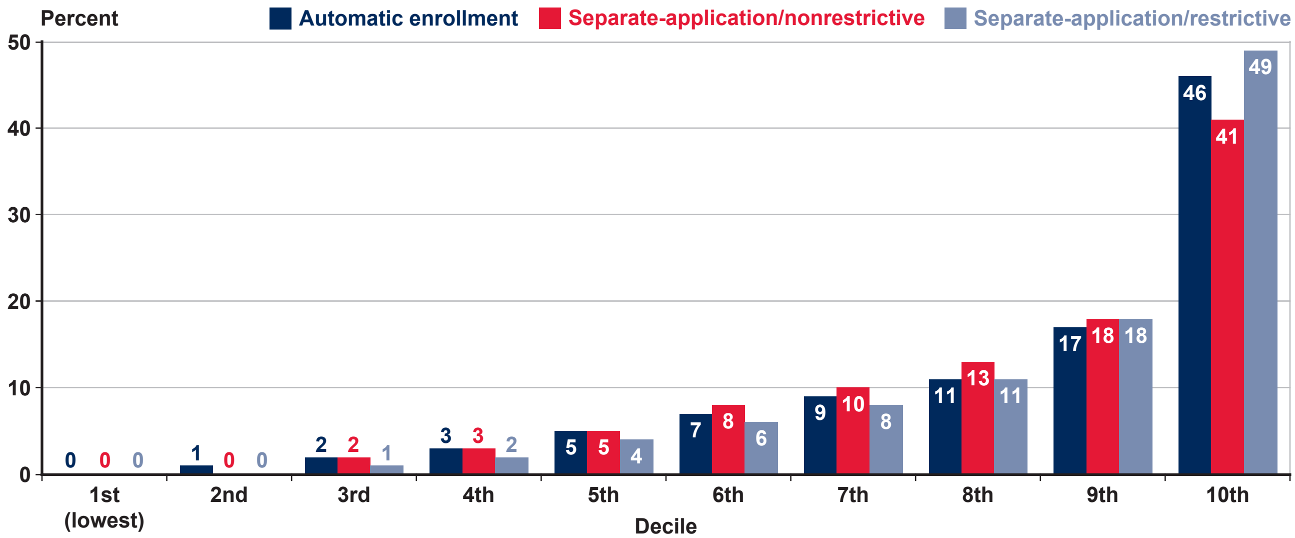
The results for separate-application/restrictive states in Chart 3A are more straightforward and pronounced. At every decile through the 7th, we observe a large negative effect relative to both the automatic enrollment states and the separate-application/nonrestrictive states. This finding is consistent with coverage differences by policy regime (larger negative effects for separate-application/restrictive states than for separate-application/nonrestrictive regimes), up to and beyond the median. However, the pattern is reversed at the 9th and 10th deciles. The reversal at the upper tail may reflect statistical noise, unintended adverse effects of separate-application/restrictive policy, or one or more other factors. However, interpreting the cause of that reversal is beyond the scope of the analysis our data set allows.

Chart 3A does not show unambiguously negative relative effects of separate-application policy regimes on overall Medicaid expenditures, and one reason why we cannot be more definitive is the variance of outcomes at the upper tail. However, there is another important reason. Riley and Rupp (2014a, 2014b) show that national aggregate expenditures are heavily skewed toward the upper tail of the distribution (such that expenditures in the lowest decile are equal or close to zero). Chart 3B clearly shows the same pattern for each of the three policy regimes. Thus, any effect of more restrictive Medicaid eligibility and enrollment practices at the lower tail may not extend to aggregate expenditures.

Chart 3A does unambiguously show that barriers arising from Medicaid enrollment policies are associated with lower expenditures at the bottom of the distribution. However, that result may not translate into a meaningful reduction in average Medicaid expenditures overall. An important implication of this finding is that enrollment policies requiring a separate application, especially in combination with more restrictive eligibility criteria, may not be effective in containing Medicaid outlays. Conversely, automatic enrollment may have relatively little or no positive overall effect on Medicaid expenditures. This suggests that automatic enrollment and other policies facilitating Medicaid enrollment among SSI awardees may increase access for this target population without generating major increases in Medicaid outlays.

In summary, although automatic enrollment increases Medicaid coverage, this study does not find evidence of the substantial upward pressure on

Chart 3B.
Percentage distribution of aggregate 2001–2006 Medicaid expenditures, by expenditure decile and state Medicaid policy regime



SOURCE: Authors' calculations based on SSA and CMS administrative records.

NOTES: Sample comprises SSI awardees who were aged 18–64 at award in 2000. Sample is limited to first-time awardees who were not DI beneficiaries before or within 72 months of SSI award. SSI awardees who were first entitled to SSI payments prior to their 18th birthdays are not included in the sample cohort.

A value of zero Medicaid expenditures is imputed for individuals who did not have Medicaid coverage in all 12 months of a given year.

Because Medicaid expenditures are heavily skewed toward the upper tail, expenditures in the 1st decile are equal or close to zero.

Rounded components of percentage distributions do not necessarily sum to 100.

Medicaid expenditures that policymakers in states opting for more restrictive Medicaid eligibility and enrollment policies might have feared. However, automatic enrollment's positive effect on access is muted by the potentially substantial lags inherent in the disability determination process, such that automatic enrollment is effective in increasing coverage only after SSA determines that the applicant meets the agency's definition of disabled. Combining the requirement of a separate application with more restrictive enrollment criteria has a clear negative effect on coverage and on expenditures at the lower tail. However, the average expenditure effects are muted by the strongly skewed nature of Medicaid expenditures and the lack of clear evidence that higher expenditures at the upper tail are attributable to restrictive enrollment policies.

Differences in Medicaid coverage patterns arising from different policy regimes do not translate into large average Medicaid expenditure effects because the policies seem to affect expenditures only at the lower tail. The results also suggest that automatic enrollment facilitates greater access to Medicaid, with little or no increase in Medicaid expenditures. Conversely, we do not find strong evidence that restrictive Medicaid enrollment policies reduce access at the upper tail, as some policymakers might have feared. Nevertheless, restrictive Medicaid enrollment policies do not have a clear cost-containment effect, as some policymakers might have hoped.

Demographic characteristics. Table 7 shows death rates among SSI recipients by Medicaid coverage status by the 12th month of SSI eligibility, overall and disaggregated by state policy regime. We select the 12th month because policy-regime effects on coverage are greatest at roughly the 1-year mark, and we want to examine whether Medicaid policy regime modifies the relationship between coverage and mortality. We do not intend to evaluate the causal relationship between mortality risk and Medicaid policy regime. Instead, we examine the mortality rate among SSI awardees who do not acquire Medicaid coverage by the 12th month, and assess whether they are more or less likely to die within 12 months than Medicaid-covered SSI awardees are. We also examine whether that pattern varies by Medicaid policy regime.

Table 7.
Death rates within 12 months among 2000 SSI awardees aged 18–64, by Medicaid coverage status and state Medicaid policy regime

Medicaid coverage status by the 12th month of SSI eligibility	Overall	Medicaid policy regime			Percentage-point difference: Automatic enrollment subtracted from—[a]	
		Automatic enrollment	Separate-application/ nonrestrictive	Separate-application/ restrictive	Separate-application/ nonrestrictive	Separate-application/ restrictive
			Number			
Total	10,763	8,639	394	1,730	. . .	. . .
Not covered	1,342	828	63	451	. . .	. . .
Covered	9,421	7,811	331	1,279	. . .	. . .
			Death rate (%)			
Total	8.0	9.6	12.2	8.2	2.6	-1.4*
Not covered	6.3	5.1	9.5	8.0	4.4	2.9*
Covered	8.3	10.1	12.7	8.3	2.6	-1.8*
Net difference by coverage status	-2.0*	-5.0*	-3.2	-0.3	1.8	4.7*
			Standard error			
Total	0.3	0.3	1.6	0.7	1.7	0.7
Not covered	0.7	0.8	3.7	1.3	3.8	1.5
Covered	0.3	0.3	1.8	0.8	1.9	0.8
Net difference by coverage status	0.7	0.8	4.1	1.5	4.2	1.7

SOURCE: Authors' calculations based on SSA and CMS administrative records.

NOTES: Sample comprises SSI awardees who were aged 18–64 at award in 2000. Sample is limited to first-time awardees who were not DI beneficiaries before or within 72 months of SSI award. SSI awardees who were first entitled to SSI payments prior to their 18th birthdays are not included in the sample cohort.

. . . = not applicable.

* = statistically significant at the 0.05 level (two-tailed test).

The death rates among SSI awardees are higher for those with Medicaid coverage by the 12th month than they are for those without coverage, regardless of policy regime. This finding is not surprising, given the severity of medical conditions and the risk of death that are associated with eligibility for Medicaid coverage. Indeed, SSA expedites initial award decisions in certain cases on those grounds. To analyze the effects of Medicaid policy regimes, we perform a difference-in-differences analysis. We observe a lower unadjusted death rate among all SSI awardees by the 12th month in separate-application/restrictive states (8.2 percent) than in automatic enrollment states (9.6 percent). However, the pattern is reversed for noncovered awardees, with rates of 8.0 percent in separate-application/restrictive states and 5.1 percent in automatic enrollment states. This finding is consistent with the hypothesis that delays in obtaining Medicaid coverage (as well as possible denials because of stricter eligibility criteria) extend the period during which SSI awardees in separate-application/restrictive states are exposed to mortality risk. We find a net differential of 4.7 percentage points associated with separate-application/restrictive policies versus automatic enrollment. This is an important finding, but it does not imply that lack of Medicaid coverage increases the risk of death.[11] Although the relationship in death-rate patterns for separate-application/nonrestrictive states relative to automatic enrollment states is more complex, the net differential is likewise positive (1.8 percentage points). However, that differential is not statistically significant.

Table 8 describes the demographic and diagnostic characteristics of surviving SSI awardees by Medicaid coverage status. We consider awardees “covered” if they have any Medicaid coverage during their first 12 months of SSI eligibility. Overall, the age distribution of the noncovered group is similar to the age distribution of the Medicaid-covered group. However,

Table 8.
Percentage distributions of 2000 SSI awardees aged 18–64 who survived 12 months from award, by Medicaid coverage status during first year, state Medicaid policy regime, and selected characteristics

Characteristic	Overall		Medicaid policy regime					
			Automatic enrollment		Separate application/ nonrestrictive		Separate application/ restrictive	
	Not covered	Covered	Not covered	Covered	Not covered	Covered	Not covered	Covered
Number	1,258	8,640	786	7,161	57	306	415	1,173
				Percentage distributions				
Age group								
18–30	23.9	24.4	14.5	23.9	40.4	24.2	39.3	27.4
31–45	28.1	28.7	31.6	28.0	21.1	31.7	22.7	32.2
46–64	48.0	47.0	53.9	48.2	38.6	44.1	38.1	40.4
Sex								
Women	53.7	59.6	56.1	59.4	50.9	57.5	49.4	61.1
Men	46.3	40.4	43.9	40.6	49.1	42.5	50.6	38.9
SSA primary diagnosis								
Mental impairments [a]	27.0	34.5	24.8	33.7	22.8	35.0	31.8	39.4
Nonmental impairments								
Neoplasms	2.4	4.5	2.9	4.4	0.0	2.3	1.7	5.3
Diseases of the—								
Circulatory system	9.0	9.5	9.4	9.9	7.0	8.2	8.4	7.2
Musculoskeletal system	17.5	13.5	18.5	13.9	17.5	14.1	15.7	11.3
Other nonmental impairments	44.1	38.0	44.4	38.1	52.6	40.5	42.4	36.8
Retained SSI eligibility for 12 months								
Yes	84.7	91.6	85.8	91.9	82.5	87.9	83.1	90.3
No	15.3	8.4	14.2	8.1	17.5	12.1	16.9	9.7
				Standard errors				
Age group								
18–30	1.2	0.5	1.3	0.5	6.5	2.4	2.4	1.3
31–45	1.3	0.5	1.7	0.5	5.4	2.7	2.1	1.4
46–64	1.4	0.5	1.8	0.6	6.4	2.8	2.4	1.4
Sex								
Women	1.4	0.5	1.8	0.6	6.6	2.8	2.5	1.4
Men	1.4	0.5	1.8	0.6	6.6	2.8	2.5	1.4
SSA primary diagnosis								
Mental impairments [a]	1.3	0.5	1.5	0.6	5.6	2.7	2.3	1.4
Nonmental impairments								
Neoplasms	0.4	0.2	0.6	0.2	0.0	0.9	0.6	0.7
Diseases of the—								
Circulatory system	0.8	0.3	1.0	0.4	3.4	1.6	1.4	0.8
Musculoskeletal system	1.1	0.4	1.4	0.4	5.0	2.0	1.8	0.9
Other nonmental impairments	1.4	0.5	1.8	0.6	6.6	2.8	2.4	1.4
Retained SSI eligibility for 12 months								
Yes	1.0	0.3	1.2	0.3	5.0	1.9	1.8	0.9
No	1.0	0.3	1.2	0.3	5.0	1.9	1.8	0.9

SOURCE: Authors' calculations based on SSA and CMS administrative records.

NOTES: Sample comprises SSI awardees who were aged 18–64 at award in 2000 and who survived to month 12 of award. Sample is limited to first-time awardees who were not DI beneficiaries before or within 72 months of SSI award. SSI awardees who were first entitled to SSI payments prior to their 18th birthdays are not included in the sample cohort.

"Covered" indicates any Medicaid coverage during first 12 months, including the month of award.

Rounded components of percentage distributions do not necessarily sum to 100.0.

a. Includes intellectual disability.

the patterns by policy regime are different. Those who are covered in automatic enrollment states tend to be younger than the noncovered. In the two groups of states that require a separate Medicaid application, the covered tend to be relatively old.

For other characteristics, the distribution patterns of covered versus noncovered are similar among all three policy regimes and are also similar to the patterns observed for the overall national average. Women, people with mental impairments, and people who retained Medicaid eligibility for 12 months are overrepresented among the Medicaid covered compared to the noncovered. All of these differences are statistically significant overall. Although some of the differences disaggregated by policy regime are not statistically significant, all differ in the same direction. These findings are not surprising in light of Medicaid coverage patterns observed in previous work (Rupp and Riley 2011, 2012).

In Table 9, we use difference-in-differences methodology to analyze the relationship between policy regime and characteristics. Here we focus on the key results presented in two columns headed "not covered minus covered." The pattern of net age differences (that is, the difference-in-differences results) is highly significant and similar for both of the separate-application regimes. The (rounded) difference in differences (25.5 percentage points) reveals a much higher proportion of younger adults associated with no Medicaid coverage (compared with those with Medicaid coverage) in separate-application/nonrestrictive states relative to automatic enrollment states. The corresponding figure is 21.3 percentage points for separate-application/restrictive states relative to automatic enrollment states. The only other statistically significant difference in differences is the comparatively high proportion of SSI recipients with no Medicaid coverage in separate-application/restrictive states who are men. Importantly, we find no significant differential associations between policy regimes according to SSA diagnosis and SSI retention status at month 12.

Discussion

SSI recipients face the double jeopardy of disability and limited financial resources. Recognizing the economic vulnerability of those individuals, Congress enabled states to make SSI awardees categorically eligible for Medicaid if they chose to do so. Yet the adoption of varying Medicaid eligibility rules may create some barriers to Medicaid enrollment in some states. In this article, we hypothesized that three factors reduce Medicaid enrollment: the requirement of separate SSI and Medicaid applications in some states, the imposition of stricter Medicaid eligibility requirements in some of those states, and the processing time required for SSI disability applications.

Consistent with our hypotheses, we observed Medicaid participation rates among SSI recipients in separate-application/nonrestrictive states that were lower than those in automatic enrollment states. As expected, we also observed that SSI recipients in separate-application/restrictive states generally had even lower rates of Medicaid participation.

Also consistent with our expectations, we found that the timing of a key event in the processing of SSI awards—the actual receipt of the first SSI payment by successful SSI applicants—was strongly associated with Medicaid enrollment. The time between first month of SSI eligibility and first month of actual payment reflects the time required to adjudicate an SSI claim. When a claim is approved, eligibility for SSI is typically established retroactively. We found that longer periods between initial eligibility and first payment were associated with lower Medicaid coverage rates during those intervening times. Specifically, we found that the first month of SSI payments is associated with a substantial, statistically significant, and abrupt increase in Medicaid enrollment. This finding suggests that, with other things being equal, the earlier the SSI award, the higher the probability of Medicaid enrollment. We found an important interaction effect between the timing of the first SSI payment and automatic enrollment. As expected, automatic enrollment generally affects Medicaid coverage positively, but this effect is exclusively attributable to higher Medicaid enrollment rates among those who have successfully completed their SSI eligibility determinations and have been paid their first SSI benefits. This result is not surprising, because automatic enrollment is triggered by a positive award decision and, prior to such a favorable decision, an applicant's SSI eligibility status is uncertain; many applicants are denied at least once before a favorable award decision is reached. This finding is especially important because Medicaid coverage can be established in principle as of the date of SSI application (or even as much as 3 months before then if the date of disability onset precedes the application date). It also means that improving the timeliness of disability determination decisions could improve the effectiveness of automatic enrollment and result in increased Medicaid participation among SSI disability recipients.

Table 9.
Difference-in-differences analysis of Medicaid coverage status in first 12 months from SSI award for 2000 SSI awardees aged 18–64 who survived 12 months from award, by Medicaid coverage status during first year, state Medicaid policy regime, and selected characteristics

Characteristic	Automatic enrollment subtracted from—					
	Separate-application/nonrestrictive			Separate-application/restrictive		
	Not covered	Covered	Not covered minus covered	Not covered	Covered	Not covered minus covered
	Percentage points [a]					
Age group						
18–30	25.9*	0.3	25.5*	24.8*	3.5*	21.3*
31–45	-10.5	3.7	-14.2*	-8.9*	4.3*	-13.2*
46–64	-15.3*	-4.0	-11.3	-15.9*	-7.8*	-8.1*
Sex						
Women	-5.2	-1.9	-3.4	-6.7*	1.7	-8.5*
Men	5.2	1.9	3.4	6.7*	-1.7	8.5*
SSA primary diagnosis						
Mental impairments [b]	-2.0	1.3	-3.3	7.0*	5.7*	1.3
Nonmental impairments						
Neoplasms	-2.9*	-2.1*	-0.8	-1.2	0.9	-2.1
Diseases of the—						
Circulatory system	-2.4	-1.7	-0.7	-1.0	-2.8*	1.8
Musculoskeletal system	-0.9	0.2	-1.1	-2.8	-2.5*	-0.3
Other nonmental impairments	8.2	2.5	5.8	-2.0	-1.2	-0.8
Retained SSI eligibility for 12 months						
Yes	-3.3	-4.0*	0.7	-2.6	-1.7	-1.0
No	3.3	4.0*	-0.7	2.6	1.7	1.0
	Standard errors					
Age group						
18–30	6.6	2.5	7.1	2.7	1.4	3.0
31–45	5.6	2.7	6.3	2.6	1.5	3.0
46–64	6.7	2.9	7.3	3.0	1.5	3.4
Sex						
Women	6.9	2.9	7.4	3.0	1.5	3.4
Men	6.9	2.9	7.4	3.0	1.5	3.4
SSA primary diagnosis						
Mental impairments [b]	5.8	2.8	6.4	2.8	1.5	3.2
Nonmental impairments						
Neoplasms	0.6	0.9	1.1	0.9	0.7	1.1
Diseases of the—						
Circulatory system	3.5	1.6	3.9	1.7	0.8	1.9
Musculoskeletal system	5.2	2.0	5.6	2.3	1.0	2.5
Other nonmental impairments	6.8	2.9	7.4	3.0	1.5	3.4
Retained SSI eligibility for 12 months						
Yes	5.2	1.9	5.5	2.2	0.9	2.4
No	5.2	1.9	5.5	2.2	0.9	2.4

SOURCE: Authors' calculations based on SSA and CMS administrative records.

NOTES: Sample comprises SSI awardees who were aged 18–64 at award in 2000 and who survived to month 12 of award. Sample is limited to first-time awardees who were not DI beneficiaries before or within 72 months of SSI award. SSI awardees who were first entitled to SSI payments prior to their 18th birthdays are not included in the sample cohort.

"Covered" indicates any Medicaid coverage during first 12 months, including month of award.

* = statistically significant at the 0.05 level (two-tailed test).

a. Values may not equal the differences between percentage values in Table 8 because of rounding.

b. Includes intellectual disability.

Finally, we looked at two sets of possible indirect effects. Our results suggest that automatic enrollment is not associated with a substantial, if any, increase in average Medicaid expenditures. We also found no evidence of alarming associations of Medicaid enrollment policies with any particular demographic characteristics.

Because our analysis covers a period prior to ACA implementation, our results cannot be generalized to post-ACA conditions; yet they do have important implications. First, our study demonstrates that low-cost administrative procedures (such as automatic enrollment) can increase Medicaid enrollment among vulnerable population segments. Second, our study provides an important baseline for future studies of the relationship between the SSI and Medicaid programs. Third, the ACA should bring opportunities to increase the effectiveness of automatic enrollment in states opting for Medicaid expansion. Specifically, Medicaid expansion under the ACA allows the administrative decoupling of the SSI award decision and SSA's automatic notification of state Medicaid offices. This is because the ACA eliminates the uncertainty about Medicaid eligibility among adults who meet the SSI income test in the Medicaid-expansion states; the ACA income screen is set at 135 percent of the family poverty threshold, while SSI uses a subpoverty-level income screen. Thus, categorical eligibility tied to meeting the SSA definition of disabled is no longer relevant for Medicaid eligibility. The Medicaid provisions of ACA potentially enable Medicaid enrollment of a huge segment of the adult population—including many individuals who do not meet the strict disability criteria required for categorical eligibility under SSI program rules—that comprised much of the uninsured prior to ACA implementation (Dorn 2008). Thus, one should expect a substantial increase in Medicaid eligibility among denied SSI applicants, and the pre-ACA reason why automatic enrollment was delayed until after the SSI award decision no longer applies.

A simple procedural modification of the automatic enrollment process could vastly improve Medicaid coverage rates among nonelderly SSI applicants. In Medicaid expansion states, SSA could simply notify the Medicaid authority shortly after SSI application; for example, when the field office establishes that the applicant meets the SSI means test. At that point in the current process, SSA automatically transfers the applicants' records to the state Disability Determination Service (DDS). Under ACA, the agency could simply transmit electronic records of those applicants simultaneously to the DDS and the state Medicaid office. This change would improve targeting of potential Medicaid enrollees in two ways. First, it would vastly increase the number of applicant records SSA could transfer to the state Medicaid authority, given that automatic enrollment currently omits roughly half of SSI disability applicants because their SSI applications are ultimately denied. Second, it would greatly accelerate automatic enrollment for allowances as well, because even though roughly half of applicants are ultimately awarded SSI, only about one-third are awarded in the initial DDS decision. Under the proposed modification, SSA would have no reason to wait even for the initial DDS decision before notifying the Medicaid authority in the expansion states. Because SSI disability applicants (including those who are denied) are among the most vulnerable Americans, this simple process modification could increase Medicaid enrollment, improve quality of care, and perhaps even reduce Medicaid expenditures for reasons similar to those discussed (in the context of churning) by Swartz and others (2015).

Notes

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[1] These are sometimes called the "1634 states" because section 1634 of the Social Security Act authorizes states and SSA to enter into automatic-enrollment agreements.

[2] These are sometimes called the "criteria states" because they use the SSI eligibility criteria for Medicaid.

[3] These are sometimes called the "209b states" because section 209(b) of the 1972 Amendments to the Social Security Act enabled states to establish their own Medicaid eligibility criteria. The 209b states use at least one eligibility criterion that is more restrictive than SSI eligibility rules.

[4] Awardees affected by such lags receive a lump-sum retroactive payment (in some cases, in several installments).

[5] Burns and others (2012) note another potential complication. In some cases, the first episode of receiving Medicaid-covered services precedes rather than follows Medicaid application (in separate-application states) or the final determination of categorical Medicaid eligibility (in automatic enrollment states). This may occur upon an emergency room admission. In such a case, the hospital administrator initiates Medicaid application and, in some cases, SSI application as well. These instances may reduce the estimated effect of policy regime.

[6] As noted earlier, those who are eventually approved will receive a lump-sum retroactive SSI payment (or several payments in installments) for the period between the first month of payment eligibility and the first month of payment receipt.

[7] Of course, SSI applicants may apply separately for Medicaid in automatic enrollment states. Whether applicant self-referral is affected by the information SSA provides on automatic enrollment is not known. Any effect might be negative if applicants, not realizing that SSA's notification of applicant eligibility to the state Medicaid office will not occur until after a long determination process, believe they do not have to (or cannot) do anything to affect Medicaid enrollment. It is also possible that special procedures that address the predicament of severely disabled SSI applicants are less likely to exist in automatic enrollment states than they are in separate-application states, where SSI applicants must apply for Medicaid on their own initiative. Unfortunately, there are no process-study data to provide direct evidence on the factors affecting SSI applicants whose final award status is yet unknown.

[8] For example, in a recipient's 12th month of eligibility, the independent variable takes the value 1 if the first payment occurred after a lag of 11 or fewer months and takes the value 0 if it occurred after a lag of 12 or more months. Thus, discontinuity is represented by a hypothesized jump in the size of the odds ratio from right to left.

[9] The Medicaid coverage results for 3 months before eligibility are also consistent with a reverse pattern of Medicaid and SSI application that may occur frequently; for example, the Medicaid-covered emergency room (ER) admission of a potentially SSI-eligible nonapplicant (see note 5). In such a case, the ER admission might result in a successful SSI application for two reasons. First, the provider has a financial interest in encouraging and facilitating SSI application. Second, the health shock resulting in ER admission may reveal a serious, perhaps life-threatening, condition that may indicate categorical eligibility for SSI. In any event, the results are consistent with our main hypothesis.

[10] For example, the point estimates for month 3 are 0.17 and 0.46 for "paid" and "unpaid," respectively (versus 1, representing the counterfactual that would suggest no difference compared to automatic enrollment). Assume that the effect of restrictive SSI eligibility policies alone is 0.46 for both groups. A much smaller point estimate (0.17) represents the combined effect of the absence of automatic enrollment and the presence of restrictive SSI eligibility, which suggests a reduction in the relative odds that would be attributable to the absence of automatic enrollment alone. In other words, the drop in odds ratios from 1 (the counterfactual) to 0.17 can be seen as having two components, the first being represented by the drop from 1 to 0.46 and the second being represented by the further drop to 0.17.

[11] The additional analysis required to make such a determination is beyond the scope of this article.

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