Project GAIN (Getting Access to Income Now): Intent-to-Treat Findings at 12- and 24-months Post Randomization, Report to the Wisconsin Child Abuse and Prevention Board and Casey Family Programs

Kristen Slack
Lawrence M. Berger
J. Michael Collins
Aaron Reilly
Emma Monahan

School of Social Work and Institute for Research on Poverty
University of Wisconsin-Madison

February 2020
Updated August 2023

Note. Project GAIN is a collaborative effort of the Wisconsin Child Abuse and Neglect Prevention Board (CANPB), Wisconsin Department of Children and Families, Casey Family Programs (CFP), Social Development Commission (SDC), and Institute for Research on Poverty (IRP) and School of Social Work at the University of Wisconsin-Madison (UW-Madison). The intervention was funded by CANPB and delivered by SDC. The evaluation is funded by CANPB, CFP, and the Centers for Disease Control and Prevention (R01 CE002807), and conducted by UW-Madison researchers (PI's Kristen Slack and Lawrence M. Berger). Additional funding for pilot research was provided by the Clinical and Translational Science Award (CTSA) program, through the NIH National Center for Advancing Translational Sciences (NCATS), grant UL1TR002373. Data were drawn from the Wisconsin Administrative Data Core housed at IRP. We thank the families that participated in Project GAIN and/or the survey component of the evaluation, as well as Project GAIN program staff at SDC. We are grateful to our colleagues at each of the collaborating and funding organizations, and to a host of research assistants currently or formerly at UW-Madison, including Yonah Drazen, Sarah Font, Leah Gjertson, Bomi Kim, Hana Lee, Kathryn Maguire-Jack, June Paul, and Anna Walther. We are also grateful to Jane Smith and Lynn Wimer at IRP for their expert advice and assistance in constructing the data file, and to Nora Cate Schaeffer, John Stevenson, Karen Zoldaz, and other staff and interviewers at the University of Wisconsin Survey Center. We thank the Wisconsin Department of Children and Families, Department of Health Services, and Department of Workforce Development for the use of administrative data for this analysis; however, these agencies do not certify the accuracy of the analyses presented. The content is solely the responsibility of the authors and does not necessarily represent the official views any of the funding agencies, including the NIH.

EXECUTIVE SUMMARY

Child maltreatment is a significant, widespread, and expensive social problem in the United States. Fully 37% of all U.S. children are involved in one or more child protective services (CPS) investigations by age 18 (Kim, Wildeman, Jonson-Reid, & Drake, 2017). And, while the majority of CPS investigations do not conclude with a finding that abuse or neglect—as measured by current legal thresholds—occurred, a substantial number of families that are investigated by CPS but not initially found to have engaged in maltreatment subsequently become re-involved with CPS at high rates (Drake et al., 2003). Yet, preventive interventions are not currently systematically offered to this group of high-risk families. In all, more than 1 in 10 U.S. children are determined by CPS to have been abused or neglected by age 18 (Wildeman, Emanuel, Putnam-Hornstein, Waldfogel & Lee, 2014).

Low income, lack of economic resources, and economic stress are among the most consistent predictors of child maltreatment in the United States (Institute of Medicine and the National Research Council, 2014; Pelton, 2015). Moreover, the vast majority of families involved with CPS exhibit unstable employment and earnings, and high levels of social welfare benefit receipt, attesting to their economically precarious status (Cancian, Noyes, & Kim, 2017). A growing body of evidence suggests that the link between economic resources and child maltreatment is likely causal in nature (Berger, Font, Slack, & Waldfogel, 2017; Cancian, Yang & Slack, 2013; Raissian & Bullinger, 2017; Schneider, Waldfogel, and Brooks-Gunn, 2017; Wildeman & Fallesen, 2017). However, existing child maltreatment prevention services primarily focus on parental psychosocial functioning and parenting behaviors rather than assisting families to increase their economic resources.

Amid concerns about both the link between economic resources and child maltreatment and also the large number of families that are investigated but not initially substantiated by CPS, only to subsequently re-enter the system, leadership of the Wisconsin Child Abuse and Neglect Prevention Board and Wisconsin Department of Children and Families, in collaboration with University of Wisconsin faculty designed a pilot intervention entitled ***Project GAIN (Getting Access to Income Now)***, which was experimentally evaluated in Milwaukee, Wisconsin from 2012 to 2016. Project GAIN was ***explicitly designed to assist families who had been investigated by CPS but had not been found to be abusive or neglectful to access economic resources, with the goal of preventing their circumstances and functioning from persisting or deteriorating to the point where a new CPS investigation occurred***.

Intervention Components

GAIN eligible families were offered—on a voluntary basis—the services of a financial resource worker whose primary functions were to provide (1) a ***comprehensive assessment*** of eligibility for an array of public and private economic benefits and supports, and to advocate on the family's behalf to access these resources; (2) ***financial counseling***, including collaborating to identify economic needs, financial goals, and financial decision-making steps to achieve them (e.g., prioritization of bills, reduction in use of high-fee financial services); and (3) ***access to one-time emergency assistance*** in the form of limited flexible funds to assist families experiencing a crisis to alleviate immediate financial stressors associated with a specific short-term economic need (e.g., a utility shut-off, food shortage) via a one-time purchase of goods or services on their behalf.

Project GAIN was strategically designed to be a ***'light touch' intervention aimed at improving access to economic resources. It explicitly aimed to provide linkages to existing economic resources, financial strategies, and limited amounts of emergency funding, over a short period of time, in a non-stigmatizing context, and delivered by staff with the training and skills to engage and establish rapport with vulnerable families***. The intervention period was short, lasting approximately 10 weeks, and consisted of one or more home visits by the financial resource worker as well as follow-up phone calls and emails. GAIN staff were extensively trained in public benefits eligibility assessment and economic resource linkages, and had expertise working with low-income, racially and ethnically diverse populations on their financial situations.

Evaluation

An experimental evaluation followed, for 24 months, 6,053 families that were randomly assigned to GAIN services or status quo services subsequent to a CPS investigation with no maltreatment finding from November 2012 to September 2016 to assess differences between the treatment and control groups in CPS involvement, income levels, income sources, and income stability. The evaluation focused on 3 cohorts of families. "Early Cohort" families (2,433 observations) were randomized into the study between November 2012 and October 2014, conditional on the primary caregiver being at least 18 years old and English-speaking, the family having had a CPS investigation that did not result in an ongoing CPS case because there were no child safety concerns and, based on information included in the CPS report, the family being believed to have at least one child age five or younger. "Late Cohort" families (2,899 observations) were randomized between October 2014 and January 2016. These families were not subject to the child age restriction but were otherwise subject to the same sample (randomization) inclusion criteria as the Early Cohort. "Survey Cohort" families (721 observations) were randomized from February 2016 through December 2016. These families were subject to the same inclusion criteria as the Late Cohort; however, because they were followed via a two-wave survey of the primary caregiver identified in the initial CPS report, they were not randomized (included in the sample) unless the primary caregiver on the case completed a baseline survey interview and was therefore eligible for randomization.

The evaluation focused on ***intent-to-treat (ITT) effects***—average differences in outcomes between all families assigned to the treatment group and all families assigned to the control group, regardless of program participation or dosage (intensity) among families assigned to the treatment group—at 12- and 24-month follow up for each of the three cohorts. The key outcome of interest was whether families were re-investigated within 12 and 24 months of randomization. Because increased income levels and stability are the primary hypothesized mechanisms through which Project GAIN was intended to reduce maltreatment, however, the evaluation also included ITT effects on income and income (in)stability.

In addition, a two-wave survey, consisting of a baseline interview (immediately prior to randomization) and a follow-up interview approximately 12 months after randomization was administered to eligible families who agreed to participate during the final 8 months of program funding (the Survey Cohort). The data collected via the survey further allowed for analyses of economic resources, economic functioning, and parenting behaviors, which were hypothesized to be important mechanisms linking program participation with CPS re-involvement but that (with the exception of formal earnings and benefit receipt) could not be tracked in administrative data.

Thus, for the Survey Cohort, the evaluation also estimated ITT effects on self-reported measures of economic and family functioning. To the best of our knowledge this is the first randomized evaluation of an economic support program specifically designed to reduce child maltreatment.

Results

Take-up rates for families randomly assigned to receive GAIN services were 31.4%, 21.8%, and 61.5% for the Early, Late, and Survey Cohorts, respectively. Importantly, the considerably higher take-up rate for the Survey Cohort reflects that these families had already been located and contacted, and had agreed to complete a baseline interview. It is also worth noting that GAIN staff were unable to make contact with 35% of Early Cohort, 39% of Late Cohort, and 15% of Survey Cohort treatment families assigned to the treatment group. Thus, among those families with which GAIN staff were able to establish contact, participation rates were 48.5%, 35.7%, and 72.6%, respectively. Nonetheless, that only a fifth to a third of families assigned to the treatment group in the Early and Late cohorts participated in the program serves to dampen any ITT effects in those samples—be they positive or negative. Moreover, Survey Cohort families constitute a select group.

Beyond overall take-up rates, findings indicate that ***more disadvantaged families (those receiving SNAP, Medicaid, or SCHIP, and those with a prior CPS history) were systematically more likely to enroll in GAIN***. This suggests that GAIN staff had more success engaging families in greater need of economic support—but also those families at greater risk of CPS re-involvement. Systematic selection into program participation may have influenced the ITT estimates.

On the whole, ***the ITT estimates do not provide clear evidence that GAIN produced its intended effects on CPS re-investigation, income level, or income stability***. For the most part, ITT estimates were nonsignificant. Moreover, many were close to zero in absolute magnitude. However, they tend to differ in pattern across cohorts. Nonetheless, there are kernels of promise in the findings: there is some suggestion of an income stabilizing and re-investigation reducing ITT effect for the lowest income families, those whose initial investigation included a neglect allegation, and those with children age 5 or younger, particularly in the Late Cohort. There are also some areas of concern: The ITT patterns also suggest potential increases in CPS re-investigation and income instability for higher income but still economically disadvantaged families. Notably, this does not appear to reflect increased surveillance of families already at risk for child maltreatment on the part of GAIN staff—program documentation and direct feedback from staff indicate that they made no CPS reports on any of the families they served. In interpreting these results, it is important to reiterate that GAIN was a low-intensity, low-dosage intervention which may have implications for its impacts.

Despite that the ITT findings were largely nonsignificant, the ITT effects differed to a striking extent in magnitude and, in some cases, direction across cohorts. For the most part, the Early Cohort estimates suggest effect sizes that are quite close to zero or, in some cases, lean toward an adverse effect of the intervention, especially for higher-income families. In contrast, the Late Cohort and Survey Cohort ITT estimates tend to be larger in magnitude and operate in hypothesized directions, particularly for lower-income families.

Discussion and Implications

Why might most of the ITT estimates be nonsignificant and also differ by cohort? First, as noted above, because ***GAIN was a 'light touch' and 'low-intensity' intervention, it may have lacked an impact on average income levels and volatility that may be necessary to influence CPS involvement***. Moreover, considering differences across cohorts, it is important to recognize that the economic climate, in terms of both the labor market and social policy context, was rapidly changing across the observation period, which followed the Great Recession and, in later periods, was characterized by relatively rapid economic growth. Notably, the unemployment rate decreased substantially throughout the study period. Moreover, BadgerCare (Medicaid and SCHIP) participation generally expanded, whereas SNAP participation contracted. The SNAP decline, in particular, likely reflected the expiration of expanded benefits and eligibility criteria under the *American Recovery and Reinvestment Act of 2009*. Thus, ***the cohorts faced very different labor markets and social welfare benefit contexts.*** This may help to explain why we find more promising support for GAIN having potential to reduce income and earnings instability, but also to increase benefit instability, particularly for higher-income families that may have been more likely to lose benefits when they contracted, among the Late and Survey cohorts.

It is also important to consider that ***CPS practices and trends may have changed over time***. On the whole, CPS cases remained opened for longer, on average, during the Early Cohort observation period, and particularly during the latter portion thereof, than during the Late Cohort period and, to a lesser extent, the Survey Cohort period. It is also important to consider that the treatment effect may have varied across time periods with ***different random assignment ratios in response to program capacity, as well as changes in take-up rates and program dosage***. Of course, it is also possible that the ***intervention improved over time as workers gained experience delivering project GAIN***, which may explain larger reductions in CPS re-involvement among the Late and Survey cohorts.

In addition, the Survey Cohort evaluation benefitted from baseline and 12-month follow-up survey tracking of whether GAIN participation impacted family financial functioning and decision making, parenting behaviors and the quality of the home environment, and behaviorally-approximated child maltreatment indicators, in addition to income and income stability (and sources thereof), and CPS re-investigation. As with the administrative data results, ***the Survey Cohort results were largely nonsignificant; they were also inconsistent in pattern***. Notably, however, they tend to suggest that those assigned to the GAIN group reported poorer family functioning relative to the control group in several domains at 12-month follow up. Most notably, the results suggest increases in parental physical aggression toward children, material hardship, economic stress, food hardship, toxic social networks, and debt for the GAIN group. Why might this be?

One possibility is that the involvement of a service provider in the life of a family already experiencing severe disadvantage creates change and disruption, even if the service is considered desirable by a family. A second possibility is that interactions with the GAIN worker led families to pay more attention to their financial situation which, itself, may have led to increased stress. That is, families may have become more aware of their debts or, more generally, their economic vulnerability because of the intervention, which may have led to increased self-reporting of such vulnerability at follow up.

Conclusion

Although ***this study provides no conclusive evidence to support the efficacy of Project GAIN, nor does it fully reject its potential***. On the whole, the ITT estimates are predominantly nonsignificant for the three primary outcomes we examined—CPS re-investigation, income level, and income (in)stability—however, there are some hints, particularly in the Late Cohort and, to a lesser extent, Survey Cohort, that the program—or, perhaps a more intensive version thereof—warrants further testing and evaluation. Indeed, the intervention may have lacked the intensity necessary to impact income levels and volatility. At the same time, we find some suggestive evidence that it may hold potential for the lowest-income families, families with child neglect allegations, and families with young children. CPS involvement is arguably a signal of severe disruptions in family functioning, comprising a highly intrusive intervention that can lead to further challenges for a family. The fact that a brief, light-touch economic support intervention was able to produce any movement in CPS involvement—in spite of the low take-up rates and limited changes in underlying economic resources—in the overall treatment group, suggests that the intervention may hold promise for some, but not all, families. Future research should continue to examine the potential causal role of income level and income stability on child maltreatment, including CPS involvement, as well as the potential for economic support interventions to prevent child abuse and neglect.

INTRODUCTION

Child maltreatment, according to official state records of confirmed abuse and neglect, now affects nearly 1% of all children in the U.S. annually (U.S. Department of Health and Human Services, 2019). The most recent National Incidence Study (NIS-4), which extends estimates of child maltreatment victimization beyond circumstances involving official reports to child protective service (CPS) systems, finds that between 17 and 40 per 1,000 children (depending on whether a more stringent "harm standard" or more inclusive "risk standard" is used) are maltreated annually (Sedlak, Mettenburg, Basena, Petta, McPherson, Greene & Li, 2010). Cumulative estimates of CPS involvement indicate that 37% of all U.S. children are involved in one or more CPS investigations at some point before the age of 18 (Kim, Wildeman, Jonson-Reid, & Drake, 2017), and that more than one in 10 U.S. children are determined by CPS to have been abused or neglected by age 18 (Wildeman, Emanuel, Putnam-Hornstein, Waldfogel & Lee, 2014). Thus, child maltreatment is a far-reaching social problem; yet, efforts to prevent it remain elusive.

Among the most consistent correlates of child maltreatment in the U.S. are low-income status and other indicators of economic stress (Institute of Medicine and the National Research Council, 2014; Pelton, 2015). National surveys estimating the incidence of child maltreatment, spanning four decades, have repeatedly shown an inverse association between household income and child abuse and neglect (Sedlak, 1991, Sedlak & Broadhurst, 1996; Sedlak et al., 2010). Family income is also associated with a range of adverse parenting behaviors (Berger, 2004), and material hardships (e.g., housing instability, food insecurity, utility shut-offs) have been repeatedly linked to various indicators of child maltreatment and CPS involvement (Conrad-Hiebner & Byram, 2020: Doidge et al., 2017; Maguire-Jack & Font, 2017; Slack, Berger,

DuMont, Yang, Erharhd-Dietzel, & Holl, 2011; Slack, Holl, Lee, McDaniel, Altenbernd, & Stevens, 2003; Yang, 2015). This association becomes further evident in the composition of families served by CPS, the vast majority of whom have unstable employment and earnings, and high levels of social welfare benefit receipt (Cancian, Noyes, & Kim, 2017).

In recent years, a small but growing number of studies have begun to provide evidence that the relationship between income and child maltreatment is likely causal (Slack, Berger & Noyes, 2017). Cancian, Yang and Slack (2013) find that exogenous increases in child support pass-through dollars to mothers receiving Temporary Assistance for Needy Families (TANF) benefits are associated with fewer screened-in (i.e., investigated) child maltreatment reports over a two-year period. Wildeman and Fallesen (2017), using Danish administrative data, find that an exogenous decrease in monthly income for mothers who rely heavily on welfare in the absence of unemployment insurance is tied to significant increases in out-of-home placements of children. Using an instrumental variables approach, Schneider, Waldfogel, and Brooks-Gunn (2017) demonstrate an increase in psychologically and physically harsh parenting behaviors associated with an economic downturn (although the same downturns were associated with lower risks of child neglect). Berger and colleagues (2017) show that exogenous increases in income are associated with reductions in child neglect, measured using mothers' self-reported neglectful behaviors, and CPS involvement, and Raissian and Bullinger (2017) find that higher state minimum wages reduce child maltreatment risk, specifically for child neglect cases. Combined, these studies suggest that particularly when focused on a low-income population, the provision of economic supports may prevent child maltreatment, and especially child neglect.

Amid concerns about both the link between economic resources and child maltreatment and the large number of families that are investigated but not substantiated by CPS that

subsequently re-enter the system at high rates (Drake et al., 2003), leadership at the Wisconsin Child Abuse and Neglect Prevention Board[1] and Wisconsin Department of Children and Families, in collaboration with University of Wisconsin Faculty designed and implemented a pilot intervention entitled Project GAIN (Getting Access to Income Now).[2] The initiative, which launched in October 2011 provided economic support services to families in Milwaukee, Wisconsin, who had been the subject of a CPS investigation that resulted in case closure with no child safety concerns, with the intent of reducing subsequent CPS involvement. A randomized experimental evaluation took place from November 2012 to September 2016[3] and followed three distinct cohorts of randomized families. This study presents intent-to-treat (ITT) effects—that is, average differences in outcomes between all families assigned to the treatment group and all families assigned to the control group, regardless of program participation or dosage (intensity) among those assigned to the treatment group—at 12- and 24-month follow up for each of the three cohorts (totaling 6,053 randomized families). The primary outcome of interest across the three cohorts is whether families were re-investigated within 12 and 24 months of randomization. However, because increased income levels and stability are the primary hypothesized mechanisms through which Project GAIN was intended to reduce maltreatment, we also present ITT effects for income and income (in)stability. In addition, a two-wave survey, consisting of a baseline interview prior to randomization (N=721) and a follow-up interview approximately 12 months thereafter (N=655), was administered to families eligible for randomization during the final 8 months of program funding. The data collected via the survey enables analyses of

[1] Formerly Children's Trust Fund.
[2] The intervention and some components of the evaluation were funded by the Wisconsin Child Abuse and Neglect Prevention Board.
[3] The evaluation was funded by the Centers for Disease Control and Casey Family Programs and conducted by researchers at the Institute for Research on Poverty and School of Social Work at the University of Wisconsin-Madison.

economic resources, economic functioning, and parenting behavior, which may also function as mechanisms linking program participation with CPS re-involvement, but that (with the exception of formal earnings and benefit receipt) are not tracked in administrative data. Thus, for the cohort of families participating in the survey, we also estimate ITT effects on self-reported measures of economic and family functioning. To the best of our knowledge this is the first randomized evaluation of an economic support program specifically designed to reduce child maltreatment.

THEORETICAL FRAMEWORK UNDERGIRDING THE INTERVENTION

Why might economic support reduce child maltreatment? One hypothesis—stemming from a resource inadequacy model—is that low-income and poverty result in low-quality environmental conditions in the home if families are without adequate resources for basic necessities (Shook [Slack], 1999). These situations, themselves, may present a risk to child well-being that is significant enough to warrant child protection system (CPS) concerns about child safety (see Figure 1). Given that neglect is the most common form of maltreatment (Kim et al., 2017; Sedlak et al., 2010; U.S. Department of Health and Human Services, 2019), and the form of maltreatment most closely associated with income (Sedlak et al., 2010), the resource inadequacy model may be particularly relevant for child neglect.

Another hypothesis—informed by a psychosocial model—posits that the way families cope with or respond to poverty may increase the risk of child maltreatment (Shook [Slack], 1999). In this framework, economic hardships, either at their onset or after sustained periods, may produce changes in caregivers' mental health, well-being, caregiving behaviors, and family dynamics. In turn, these changes may pose a threat to child safety and well-being (See Figure 2). Both the resource inadequacy and psychosocial models incorporate pathways linking poverty to child maltreatment risk through exogenous factors (e.g., a plant closing that results in an

unexpected job loss) as well as through pre-disposing individual and family characteristics (social selection). In other words, it is recognized that some circumstances of poverty or economic stress are beyond family control, whereas pre-existing characteristics may also predict both poverty and child maltreatment risk. What is important to note is that regardless of the cause of economic stress, the experience of it may elevate child maltreatment risk either by creating new risk factors or by exacerbating existing risk. Furthermore, if the above assumption is correct, then regardless of the mechanism linking poverty to child maltreatment, the introduction of economic supports should reduce child maltreatment risk to some extent.

THE INTERVENTION: PROJECT GAIN (GETTING ACCESS TO INCOME NOW)

This study examines whether assisting families with acquiring economic resources in the form of increased earnings, social welfare benefits, and access to other material necessities (e.g., adequate food, housing, medical care) significantly reduces risk for CPS involvement and, thereby, functions as a prevention strategy with regard to child maltreatment. Project GAIN ("Getting Access to Income Now") was designed to serve families who are reported to CPS in Milwaukee, Wisconsin, but whose cases are closed after an investigation because no indicators of child maltreatment or imminent threats to child safety are present. Such families are at high risk for future CPS involvement; approximately 25% are re-reported to CPS within one year (Wisconsin Department of Children and Families, 2016), and past research has found that various indicators of poverty predict this recurrence (Barth, Gibbons & Guo, 2006; Connell, Bergeron & Katz, 2007; Kohl, Jonson-Reid & Drake, 2009; Stith et al., 2009; White, Hindley & Jones, 2015). Yet, in the absence of Project GAIN, no service interventions (and, economic support interventions, in particular) are systematically offered to this group of high-risk families.

Despite their high levels of child maltreatment risk, there are few systematic efforts across the U.S. to engage this population of investigated families with no resulting case opening.

The overarching goal of Project GAIN was to prevent families' circumstances and functioning from persisting or deteriorating to the point where a new CPS investigation occurs. Families randomly assigned to the treatment group were offered—on a voluntary basis—the services of a Project GAIN worker whose primary functions were to provide (1) a comprehensive assessment of eligibility for an array of public and private economic benefits and supports, and to advocate on the family's behalf to access these resources; (2) financial counseling, including collaborating to identify economic needs, financial goals, and financial decision-making steps to achieve them (e.g., prioritization of bills, reduction in use of high-fee financial services); and (3) access to one-time emergency assistance in the form of limited flexible funds to assist families experiencing a crisis to alleviate immediate financial stressors associated with a specific short-term economic need (e.g., a utility shut-off, food shortage) via a one-time purchase of goods or services on their behalf.

The field period for Project GAIN began in October 2011 and ended in September 2016. There were several phases to the intervention. For the first year, Project GAIN staff engaged in a wide array of training activities related to benefit eligibility rules for various means-tested programs, as well as participant outreach and engagement strategies. Although randomization occurred during this phase, the official start date for the evaluation period was November 2012, once all worker training was completed. From November 2012 through September 2014 (referred to as the "Early Cohort"; N=2,433), families were randomized into the study if the primary caregiver was 18 years old or older and spoke English,[4] and the family (a) had a CPS

[4] GAIN staff were not fluent in other languages.

report that resulted in an investigation (called "initial assessment" in Wisconsin), but no ongoing CPS case, and (b) were believed to have at least one child age five or younger, based on information in the CPS report. Beginning in October 2014 and through January 2016 (referred to as the "Late Cohort"; N=2,899), the child age restriction was lifted, and all families meeting the other eligibility requirements were randomized into the study.

A final phase of the study (referred to as the "Survey Cohort"; N=721) randomized families from February 2016 through December 2016. These families were subject to the same CPS-related inclusion criteria as the Late Cohort and were also followed in the administrative data for 24-months post randomization. However, because they were also to be followed via a two-wave, in-person survey of the primary caregiver identified in the initial CPS report, these families were not considered eligible for GAIN (were not randomized) unless they first completed the baseline survey. As such, the Survey Cohort sample likely reflects a different and more select population than those of the Early and Late Cohorts. For example, the take-up (participation) rate for those assigned to treatment group in the survey cohort was 61.5%, whereas it was 31.4% and 21.8% for the Early and Late Cohorts, respectively.

The intervention period was relatively short, lasting approximately 10 weeks, and involved one or more home visits by the worker in addition to follow-up phone calls and emails. Project GAIN was delivered in families' homes by the Social Development Commission (SDC), a community agency in Milwaukee, Wisconsin that dedicated project staff exclusively to Project GAIN. These staff were extensively trained in public benefits eligibility assessment and economic resource linkages, and had expertise in working with low-income, racially and ethnically diverse populations on their financial situations. All program activity was routinely recorded by Project GAIN staff in an electronic database. Commonly identified financial needs

of Project GAIN participants were related to accessing public benefits, securing or finding better employment, and retaining or finding affordable housing. Other common needs included assistance with child support, taxes, and legal fees, help with budgeting, credit counseling and financial decision-making, and transportation.

The evaluation of Project GAIN was designed to address a critical overarching question for the child maltreatment prevention field: *To what extent can improved access to economic resources prevent child maltreatment?* However, in considering the implications of the ITT estimates, it is important to consider that GAIN was strategically designed to be a short-term, 'light touch' intervention aimed at improving access to economic resources; it aimed to provide a combination of linkages to existing economic resources, financial strategies, and limited amounts of emergency funds over a short period of time, in a non-stigmatizing context, and delivered by staff with the training and skills to engage and establish rapport with vulnerable families. It is crucial that the results of our evaluation be considered in this context.

In this report, we present ITT effects on CPS re-investigation, income level, and income stability (the two key mechanisms through which GAIN participation was expected to result in reduced CPS re-involvement) within 12 and 24 months of random assignment to project GAIN for each evaluation cohort. We further examine potential heterogeneity in ITT effects by whether the initial investigation, which triggered randomization, included a child neglect allegation, given that neglect is more closely linked to low-income and poverty than are other forms of child maltreatment (Sedlak et al., 2010), as well as by baseline income level. To the extent that Project GAIN generates a meaningful impact on reductions in child maltreatment, there are significant implications for economic support and child maltreatment prevention policies and programs with

respect to their ability to effectively address economic stressors in populations at risk for child maltreatment.

METHODS

Sample

All families meeting the eligibility criteria for Project GAIN were randomized via Wisconsin's State Automated Child Welfare Information System (WiSACWIS) at the point of case closure subsequent to CPS investigation ("initial assessment" in Wisconsin). The primary caregiver in the CPS report is considered the sample member (target of the GAIN intervention) in all cohorts. Eligibility criteria, however, differed for the three cohorts in that Early Cohort families (randomized from November 5, 2012 to October 9, 2014) were required to include a child age 5 or under, based on information available to the CPS worker at the time, in order to be eligible for GAIN, whereas there was no child age restriction for Late Cohort families (randomized October 10, 2014 to January 10, 2016). Likewise, there was no age restriction for Survey Cohort families (randomized February 3 through September 21, 2016). However, these families were only randomized subsequent to participating in a baseline survey interview. Beyond these conditions, eligibility criteria for all cohorts necessitated only that that the family was investigated by CPS and that the case was closed within 90 days of the initiation of the investigation—with no ongoing child safety concerns[5]—subsequent to that investigation, and that the primary caregiver was at least 18 years of age and English speaking. Cases that did not close within 90 days of the initiation and those that closed with child safety concerns were not subject to randomization for inclusion in GAIN. This 90-day criterion was put in place because it aligned with state statutory requirements for the investigation timeline. However, as addressed in

[5] WiSACWIS includes a data field that requires the worker to indicate whether there are ongoing safety concerns in order to close the case.

the discussion section, this requirement was not always met and, at different points in the study period, investigations exceeded the 90-day limit at different rates.

For the Early and Late cohorts, WiSACWIS was programmed such that, as soon as a CPS worker or supervisor electronically closed a case (and indicated no ongoing safety concerns), the software randomly assigned the family to the treatment (offer of GAIN services) or control (no GAIN services offer) condition based on the treatment-to-control ratio set in the WiSACWIS randomization algorithm at that point in time. WiSACWIS was programmed such that, once randomly assigned, a family would not be re-assigned, regardless of subsequent CPS involvement. The WiSACWIS system then relayed families' treatment or control group status and contact information to DCF. In turn, DCF transmitted the list of all cases randomized to treatment group in the prior week to SDC on the first business day of each week. CPS workers/supervisors were blind to the randomization process, which was fully automated. In all, 2,433 families were randomized in the Early Cohort and 2,899 in the Late Cohort.

For the Survey Cohort, DCF transmitted a weekly list of all Milwaukee CPS investigations that were close within 90 days without ongoing safety concerns to the University of Wisconsin Survey Center (UWSC). UWSC interviewers then attempted to conduct in-person interviews with all included families at their home or another location of their choosing. If a baseline interview had not been conducted within 7 weeks of UWSC receipt, the case was coded as an eligible non-interview. DCF transmitted 1,200 cases to UWSC during the sampling period (February-September 2016). Of these, 731 completed an interview, 360 were eligible for an interview bud did not participate, 101 were ineligible, and 8 were of unknown eligibility and not interviewed.[6] Excluding the 101 ineligible cases, this resulted in baseline response rate of 66.5%.

[6] Eligible non-interviews consisted of refusals or broken-off contact by the respondent, failure to contact the household, and the respondent being 'away or unavailable.' Ineligible cases consisted of those in which the

On a weekly basis, UWSC executed an algorithm to randomize all families with a completed interview in that week to either the treatment or control group and transferred contact information for treatment group families to Project GAIN staff.

UWSC attempted to interview each of the 731 families that completed a baseline interview approximately 12 months after their initial interview and successfully re-interviewed 658 families. Of these, 650 completed the interview in person and 8 completed the interview by telephone, at the respondent's request; 5 cases were deemed ineligible for a follow-up interview because the respondent was deceased (2 cases) or incarcerated (3 cases). These cases were excluded from the denominator in calculating the response rate. As such, the follow-up survey response rate was 90.6% (658/726). We excluded 10 cases (from the 731 potential Survey Cohort families) from all Survey Cohort analyses, including those using administrative data. We excluded 7 cases because it was discovered during the Wave 2 interview process that an interviewer had falsified the Wave 1 interviews for these cases. We dropped an additional 3 cases that could not be located in Wisconsin administrative data systems at follow-up. Thus, we focus on an analytic sample of 721 Survey Cohort families for analyses using only administrative data, through which we are able to follow the entire Survey Cohort sample, and an analytic sample of 655 Survey Cohort families in our analyses of survey data, which require that the family participated in the 12-month follow-up survey.[7]

Random Assignment Ratios

respondent was deceased, physically or mentally unable, incarcerated, non-English speaking, living outside of Milwaukee County, in an institution, under 18, having no children in the home, and lacking contact information from DCF (and unable to be identified and located by UWSC).

[7] There were no significant differences between those who completed a follow-up survey and those who did not with respect to any of the covariates (drawn from administrative data), with the sole exception that Asians were less likely to complete a follow-up survey. Nor were there significant differences between the two groups in caregiver CPS re-investigation at either 12- or 24-month follow-up.

The treatment-to-control randomization ratio was altered over the course of the evaluation to flexibly respond to GAIN program capacity in a given period. Table 1 presents summary information on the treatment-to-control ratio periods for each cohort, including the number of families randomized, treatment-to-control ratio in effect, and take-up rate in the period. The Early Cohort treatment-to-control ratio ranged from 1:2 to 2:1 across four periods, although the majority of sample families (69.8% or 1,698 of the 2,433 Early Cohort families) were subject to a 1:2 ratio. The take-up rate (rate of program participation among those randomly assigned to the treatment group), which was defined as the family engaging in at least one in-person (typically in-home) visit with a GAIN worker, also varied across randomization periods, ranging from 20.4% to 33.6%. However, there does not appear to be a systematic pattern of take-up by randomization ratio. For example, the highest take-up rates were observed when the ratio was set at 1:2 (33.6% take-up) and 2:1 (33.5%); during the two periods for which the ratio was 1:1, the take-up rates were 20.4% and 23.1%.

The Late Cohort treatment-to-control ratio ranged from 2:1 to 1:3 across four randomization periods, with the majority of families (80.5% or 2,335 of 2,899) being randomized in the final period with a 1:3 treatment-to-control ratio and a 21.5% take-up rate. Again, while the take-up rate varies across periods (from 17.9% to 25.4%), it does not appear to systematically vary with the randomization rate.

Between February 3 and July 5, 2016, Survey Cohort families were randomized at a 1:1 ratio. From July 5 through August 8, 2016, families were assigned to the treatment group at a ratio of 4:1 because project GAIN's funding (and thus the program) would be ending as of September 30, 2016. From August 8 through September 21, 2016, all cases were assigned to the control group to balance the sample. Take-up rates for the three periods were 61.2%, 64.3%, and

0.0% (because no cases were assigned to the treatment group in the latter period), again suggesting that the randomization ratio did not systematically affect the take-up rate. Nonetheless, we control for period of randomization in all of our regression analyses given differences in both randomization ratios and take-up rates across periods.

Administrative Data (All Cohorts)

Administrative data for the three cohorts are drawn from the Wisconsin Administrative Data Core housed at the Institute for Research on Poverty at the University of Wisconsin-Madison. The Data Core is comprised of linked, harmonized data from the full universe of participants in any of the State of Wisconsin's electronically available social welfare administrative data systems. It supports integrated analyses of earnings, income, multiple program participation trajectories (including CPS involvement), and the health and wellbeing of individuals and families in the state. The Data Core enables linkages among parents and children, members of the same family or household, and individuals comprising a benefit case, resulting in data files that include multi-system administrative data on multiple individuals with familial or residential ties followed across time. It includes program participation files with information on eligibility, participation, spells, and benefit receipt and amount. It currently includes data on almost 8 million unique individuals.

We use data on Supplemental Nutrition Assistance Program (SNAP; formerly Food Stamps) participation and benefit receipt; Medicaid and Badgercare (Wisconsin's State Child Health Insurance program) participation; Temporary Assistance for Needy Families (TANF; known as Wisconsin Works [W-2] in Wisconsin) participation and benefit receipt; Child Support Enforcement participation (as payee or payer), orders, and payments (paid or received); Unemployment Insurance (UI) earnings and benefits; Supplemental Security Income (SSI)

participation; and, CPS involvement. Monthly data are available for all of the above, except earnings, which are measured quarterly. Thus, we conduct our analyses using quarterly data. Because the full population of randomized families for Project GAIN have experienced a CPS investigation, all families will appear in the Data Core.

There are two primary limitations to our data.[8] First, the administrative and program data do not allow us to assess services (beyond social welfare benefits) accessed outside of Project GAIN. While GAIN is only available to the treatment group, this is of concern if the control group is disproportionately likely to seek other prevention services (e.g., home visiting), or if the treatment group declines other services due to GAIN participation. Such actions will bias our estimates toward zero. However, if treatment members engage in additional services that did not result from GAIN participation/referral, our estimates will be upwardly biased. Second, our analyses of administrative data rely on UI earnings data alone in computing earnings. Yet, earnings may include sources that are not subject to UI reporting or that occur outside of the mainstream economy (are "off the books"). Notably, low-income families, such as those in our sample, are disproportionately likely to have off-the-books earnings. Thus, we likely underestimate total earnings and, correspondingly, total income. As described below, for the Survey Cohort, in addition to conducting analyses of total income based solely on administrative data, we also conduct these analyses using self-reported income, for which respondents were explicitly asked to include informal income sources.[9]

[8] With respect to the administrative data, missing data are not an issue, *per se*. Rather, missing data in a particular program or field simply indicates that no services were provided or there was no participation in the program. There is, however, a modest amount of missing data for family demographic characteristics, including age of youngest child, number of fathers to mothers' children, caregiver race/ethnicity, number of children, family structure, and number of days between case closure and randomization. We treat 'missing' as a category in each of these variables and include indicators of such in our regressions. Rates of missing data on these variables are presented in Table 2.

[9] Specifically, for the Survey Cohort, we estimated ITT effects of GAIN assignment on self-reported total income. We are not able to conduct analyses of self-reported income (in)stability, however, because only annual income is reported in the survey, whereas quarterly income is available in the administrative data.

CPS Involvement

Project GAIN explicitly intended to reduce future CPS involvement among participating families. Thus, our key outcome is whether the primary caregiver is re-investigated by CPS (subject to a new CPS investigation) within 12- and 24- months of randomization.[10] In constructing this measure, we exclude re-investigations within 21 days of randomization because they may be related to the initial investigation that triggered GAIN randomization.

Income and Income (In)Stability

Because income level and income stability are the primary mechanisms through which GAIN participation is hypothesized to impact subsequent CPS involvement, we also examine whether treatment group assignment led to higher income and/or more stable income in the 12 and 24 months after random assignment. We constructed total family income to be total earnings, child support received, SNAP received, TANF received, and UI received for the primary caregiver and their spouse (if present in the household) over the course of the 12- or 24-month period of focus (in $10,000s; 2014 constant dollars). Income (in)stability is assessed by the coefficient of variation (CV) on income, which is constructed as the ratio of the standard deviation to the mean of quarterly family income over the 12- or 24-month period. The CV represents the variance (dispersion) in household income relative to its mean over the relevant time period (in quarters), with a greater value indicating greater instability and a value of zero indicating constant quarterly income (Gennetian, Rodrigues, Hill & Morris, 2019; Gennetian, Wolf, Hill & Morris, 2015; Hardy, 2017).

Covariates

[10] We have also examined whether any individual associated with the CPS investigation that triggered inclusion in the GAIN sample was the alleged perpetrator in a subsequent investigation within 12 and 24 months of randomization. Results were consistent with those presented here.

All covariates are drawn from the administrative data, which are available for all three cohorts. Given that families were randomly assigned to treatment or control conditions, the mean difference in 12- and 24-month outcomes should accurately reflect the ITT effect of GAIN assignment if the treatment and control groups are truly identical on all characteristics. Nonetheless, we control in our regression models for a number of background characteristics for two reasons. First, doing so will adjust for any small differences in characteristics between groups that results from chance. Second, doing so improves the precision of the estimates.

Unless otherwise noted, all covariates are measured in the quarter prior to randomization. The covariates include total income[11] in the year (4 quarters) prior to randomization (in $10,000s; 2014 constant dollars), any earnings, SNAP receipt, MA/SCHIP receipt, primary caregiver age, age of youngest child (and age of youngest child missing), number of fathers to mother's children (and number of fathers missing, which includes cases in which the mother was not the primary caregiver identified in the CPS investigation[12]), number of children (and number of children missing), race/ethnicity (white, black, Hispanic, Native American, Asian, missing), primary caregiver was born in the United States, family structure (two-parent household, single-mother household, single-father household, missing), number of previous CPS investigations, any prior CPS substantiation, and number of days between the date of the CPS report that triggered inclusion in the sample and the date of randomization. We also include period of randomization fixed effects (dummies) to account for differences in randomization ratio and associated take-up, as well as quarter of randomization fixed effects to account for any time-

[11] We top-coded total income at the 95th percentile in each cohort.

[12] The administrative data pertain to the primary caregiver associated with the initial CPS investigation that triggered randomization. For cases in which the primary caregiver is not the mother to the focal child(ren) identified in the CPS record, we code the number of fathers to the mothers' children variable as being missing rather than as representing the number of partners with whom the (non-mother) primary caregiver has had children.

specific unobserved factors.

Survey Data (Survey Cohort only)

The administrative data allow assessment of any effects of GAIN participation on CPS re-involvement as well as income and income stability due to formal earnings and benefit receipt. However, they do not allow for analyses of more granular behavioral changes in parenting and economic functioning, which may be key mechanisms linking program participation with CPS re-involvement (and to changes in income, and income stability). The survey was explicitly designed to enable analyses of such differences in behaviors and functioning between GAIN and control group families at 12 months post-randomization. We included in the survey *7 Primary Outcomes* to assess child maltreatment-related parenting behaviors and family economic wellbeing, which we identified as potential mechanisms linking program participation with CPS re-involvement. We also included 11 *Secondary Outcomes* comprising additional measures of parenting behaviors and economic resources that may function as mechanisms directly linking GAIN participation to CPS re-involvement and changes in income and income stability and/or mechanisms indirectly linking GAIN participation with these key outcomes via the primary parental functioning and family economic resources outcomes included in the survey. Finally, we included 7 *Ancillary Outcomes*, which we view as alternative and/or complementary measures to the primary and secondary survey outcomes. Unless otherwise noted, all measures were coded such that a higher score is associated with poorer parenting behaviors or greater economic hardship, such that the direction of estimates will match that for CPS re-involvement. All non-dichotomous measures were standardized to have a mean of 0 and standard deviation of 1. We describe these measures below.[13]

[13] Sample sizes differ slightly across outcomes based on survey data due to missing values on specific items. However, rates of missing data were quite low, with sample sizes for analyses of the survey data outcomes ranging

Primary Survey Outcomes

We focus on 7 primary survey outcomes, including 4 measures of child maltreatment-related parenting behaviors (neglectful behaviors, physical aggression behaviors, and emotional abuse behaviors, parenting distress) and 3 measures family economic wellbeing (self-reported income, economic stress, material hardship). The neglectful behaviors, physical aggression behaviors, emotional abuse behaviors measures were adapted from the Parent-Child Conflict Tactics Scales (PC-CTS; Straus, Hamby, Finkelhor, Moore & Runyan, 1998; Straus & Gelles, 1990). Each subscale consists of a set of items scored on a 5-point scale in which the respondent reported how often each event occurred in the last 12-months, ranging from never to very often. Items were coded so that a higher score represents greater frequency of the event. They were then averaged for each subscale to create an overall score for the subscale. *Neglectful behaviors* are assessed by 6 items, including whether the respondent left their children home alone, even when they thought some adult should be with them; left their children with someone who they were not sure would do a good job taking care of them; were so caught up with their own problems that they were not able to care for their children the way they would have liked; were unable to make sure their children got the food they needed; were unable to make sure their children got the clothing they needed; were unable to take their children to a doctor or hospital when they needed it; and were so drunk or high that they had problems taking care of their children. *Physical aggression behaviors* consist of two items measuring how often the respondent spanked or slapped their children and hit their children with an object. *Emotional abuse behaviors* include how often the respondent's children witnessed someone physically

from 635 to 650 out of a potential sample of 655. Also, as noted above, all covariates are drawn from administrative data, thus no cases are excluded due to missing data on the covariates.

hurting the respondent; how often the respondent swore at their children; and how often they called their children stupid, dumb, or other names.

Parental distress is assessed by the Role Strain subscale of the short form of the Parenting Stress Index (PSI-SF; Abidin, 2012). It includes 12 items, each on a 5-point scale (from either strongly disagree to strongly agree or never to very often) and coded so that a higher score represents greater parental distress. Example items include "you find yourself giving up more of your life to meet your children's needs than you ever expected;" "you feel trapped by your responsibilities as a parent;" "you have the feeling that you cannot handle things very well;" and "there are quite a few things that bother you about your life." Items were averaged to create a total score.

Self-reported *income* was assessed via the respondent's report of the total income of their household over 12 months preceding the survey. Respondents were asked to consider all sources before taxes and deductions, including their own income and the income of everyone living with them from jobs, public assistance programs, and any other sources. We scaled income in $10,000 increments. *Economic stress* is assessed by the average score of 7 items measured on a 5-point scale from strongly disagree to strongly agree, including "you have trouble affording the basic things that your family needs;" "you can usually afford to pay your bills on time;" "you feel overwhelmed about money issues;" "these days, you can generally afford to buy the groceries that your family wants;" "if you suddenly needed $100 for an emergency, you could come up with it;" "you feel stressed about your financial situation;" and "your family often struggles to have enough to eat." This index was developed for the Project GAIN survey. *Material hardship* is measured with eight dichotomous items tapping the occurrence of various hardships within the past 12 months including telephone service, electricity, heating or gas ever being disconnected

because there wasn't enough money to pay the bill; inability to pay the full rent or mortgage payment; use of a food pantry to receive free or low-cost food; moving in with other people even for a little while because of financial problems; staying at a shelter, in an abandoned building, an automobile or any other place not meant for regular housing even for one night; receiving any financial help from family or friends due to inability to pay bills or expenses; and whether there was anyone in the household who needed to see a doctor or go to the hospital but couldn't go because of the cost.

Secondary Survey Outcomes

We focus on 11 secondary survey outcomes, 9 of which are related to parenting and the quality of the caregiving environment and 2 of which are related family economic wellbeing. Five of the parenting-related measures (lack of parenting warmth/affection, lack of parenting attention, parental impatience, parental leniency, lack of parenting routines) included items adapted from the Parent Satisfaction Scale (Guidubaldi & Cleminshaw, 1985), and items developed for the GAIN study. Each item was asked on a 5-point scale ranging from strongly disagree to strongly agree. Items were averaged to create a total score for each subscale. *Lack of parenting warmth/affection* is measured by level of agreement with four items: "too much affection will spoil a child"; "even teenagers need to be told often that they are loved;" "it's very important for a parent to smile a lot at their infants;" and "the more you comfort a crying newborn, the more spoiled the baby will become." *Lack of parenting attention* is measured by 3 items: "you spend a lot of time talking or playing with your children"; "you often lack the time and energy to pay close attention to your children:" and "you are comfortable with the amount of supervision you give to your children." *Parental impatience* includes 2 items: "you wish you did not become impatient so quickly with your children" and "you are bothered by the amount of

yelling you direct toward your children." *Parental leniency* is measured by 3 items: "you would rather give in to a child's demands than deal with a tantrum or outburst;" "setting lots of rules leads kids to grow up resenting their parents;" and "as a parent, you are much more likely to be strict than lenient." *Lack of parenting routines* consists of 4 items: "daily routines help young kids to feel safe and secure;" "it is important to put young children to bed at about the same time every night;" "when kids are old enough to know they are tired, they do not need a set bedtime;" and "doing things together as a family is extremely important to you."

Four parenting-related items were adapted from the Parents' Assessment of Protective Factors Scale (PAPF; Kiplinger & Harper Browne, 2014), each of which was assessed on a 5-point scale from strongly disagree to strongly agree. Items were averaged to create subscale scores. *Lack of parental resilience* is assessed via 8 (reverse-coded) items: "you feel positive about being a parent;" "you take good care of your children even when you are sad;" "you find ways to handle problems related to your children;" "you manage the daily responsibilities of being a parent;" "you have the strength within yourself to solve problems that happen in your life;" "you are confident you can achieve your goals;" "you take care of your daily responsibilities even if problems make you sad;" and "you believe your life will get better even when bad things happen." *Lack of social connections* includes 9 items: "you have someone who will help you get through the tough times; "You have someone who can help you calm down if you get upset;" "You have someone who can help you calm down if you get frustrated with your children;" "You have someone who will encourage you when you need it;" "you have someone who will tell you in a caring way if you need to be a better parent; "you have someone who helps you feel good about yourself; "you are willing to ask for help from your family;" "you have someone to talk to about important things;" and "you have someone you can ask for help when

you need it." *Lack of concrete support* is measured via 9 items: "you don't give up when you run into problems trying to get the services you need;" "you make an effort to learn about the resources in your community that might be helpful for you;" "when you cannot get help right away, you don't give up until you get the help you need;" "you know where to go if your child needs help;" "you are willing to ask for help from community programs or agencies; "you know where you can get helpful information about parenting and taking care of children;" "asking for help for your children is easy for you to do;" "you know where to get help if you have trouble taking care of emergencies;" and "You try to get help for yourself when you need it." *Lack of social and emotional competence* is comprised of 7 items: "you stay patient when your children cry or misbehave:" "you play and talk with your children when you are together;" "you can control yourself when you get angry with your children;" "you make sure your children get the attention they need even when your life is stressful;" "you stay calm when your children misbehave;" "you help your children calm down when they are upset;" and "you are happy when you are with your children.

Housing hardship is measured with 4 dichotomous items drawn from the Illinois Families Study (Lewis et al., 2000). Respondents were asked whether their home has broken windows or windowpanes, peeling paint, open cracks or holes in the walls, ceilings or floors, a leaky roof, ceiling or walls, or exposed electrical wires or uncovered electrical outlets; mice, rats, or bugs; a broken refrigerator, oven or stove; or, a broken toilet, bathtub, or shower. Items were averaged to compute a total score. *Food hardship* is assessed by 2 items, each measured on a 5-point scale from strongly disagree to strongly agree: "you can generally afford to buy the groceries that your family wants" and "our family often struggles to have enough to eat". Items were averaged to create a total score.

Ancillary Survey Outcomes

Of the 7 ancillary survey outcomes, 5 assess parenting and the quality of the caregiving environment and 2 assess family economic wellbeing. *Depressive symptoms* are measured using the 20-item Center for Epidemiologic Studies Depression Scale, Revised (Radloff, 1977; Eaton, Muntaner, Smith, Tien & Ybarra, 2004). Each item is measured on a 5-point scale, from never to very often, indicating how often each symptom occurred during the past week. The symptoms include: "your appetite was poor;" "you had trouble keeping your mind on what you were doing;" "you felt depressed;" "you felt that you couldn't shake off the blues;" "your sleep was restless;" "you felt sad;" "you could not get going;" "you have felt calm and in control;" "nothing made you happy;" "you felt like a bad person;" "you lost interest in your usual activities;" "you slept much more than usual;" "you felt like you were moving too slowly;" "you felt fidgety;" "you were tired all the time;" "you did not like yourself;" "you lost a lot of weight without trying to;" "you had a lot of trouble getting to sleep;" and "you have felt rested;" "you could not focus on the important things." Items were averaged to create an overall score.

Lack of self-efficacy is assessed by 7 items adapted from the Pearlin Mastery Scale, which was designed to measure the extent to which individuals feel in control of their lives (Pearlin, Lieberman, Menaghan, & Mullan, 1981). Items include "you can do just about anything you really set your mind to;" "there is really no way you can solve some of the problems you have;" "there is little you can do to change many of the important things in your life;" "you often feel helpless in dealing with the problems in your life;" "sometimes you feel that you are getting pushed around in life;" "you have little control of the things that happen to you; and "what happens to you in the future mostly depends on you." Items are measured on a 5-point scale

(from never to very often) indicating how often the respondent feels consistent with the statement and averaged to create a total score.

Toxic social network consists of 6 items, each measured on a 5-point scale (from strongly disagree to strongly agree) tapping the extent to which the respondent agrees with each of the following statements: "your family and friends are always meddling in your personal business;" "there are people in your life who try to get you in trouble with others;" "there are people in your life who act in a threatening way toward you; "your family and friends often criticize you or put you down;" "there are people in your life who ask for too much help from you;" and "your family and friends involve you too deeply in their own problems." These items were created for the GAIN survey.

Poor adult relationship quality is assessed by a 12-item measured drawn from the Fragile Families and Child Wellbeing Study (Fragile Families and Child Wellbeing Study, 2008). It is asked only of caregivers who have a co-resident spouse or partner. Items assess how often, on a 5-point scale from never to very often, the respondent reports that their spouse or partner is fair and willing to compromise when they argue; tries to keep them from seeing their friends and family; expresses affection or love for them; is always criticizing them or their ideas; makes them feel like they are "walking on eggshells;" encourages or helps them to do things that are important to them; listens to them when they need someone to talk to; tries to control their every move; threatens to hurt them or has hurt them physically; really understands their hurts and joys; makes them feel like everything they do is wrong; and, trusts you; as well as the extent to which they are able to make decisions without worrying about upsetting their spouse or partner; they and their spouse or partner are on the same page when it comes to how to raise kids; and they and their spouse or partner have many more good days than bad days.

Recent intimate partner violence consists of a dichotomous indicator that, during the past 12-months, a romantic partner has hit, slapped, or kicked the respondent or thrown or shoved them onto the floor, against a wall, or down stairs.

Debt is assessed via a single indicator that the primary caregiver reported that the family had any bills that were overdue by 60 days or more. Finally, *Earned Income Tax Credit (EITC)* receipt is measured by a dichotomous indicator that the respondent reported receiving the EITC or any tax refund within the last 12 months.

Analytic Approach

All analyses are conducted separately for each cohort because the cohorts are drawn from different underlying populations. For each cohort, we first present simple (unadjusted) mean differences (ITT effects) in CPS re- investigation at 12- and 24-months post-randomization for the treatment (GAIN) and control group. We then present regression adjusted ITT estimates at 12- and 24-months post randomization based on the following equation:

$$CPS_{fpq} = \beta_0 + \beta_1 TMT_{fpq} + \beta_2 COVS_{fpq} + \beta_3 PRD_p + \beta_4 QTR_q + \varepsilon_{fpq} \quad (1)$$

where CPS is a CPS re-investigation by either 12- or 24-months post-randomization for family *f* that was randomized in period *p* and quarter *q*, TMT equals 1 if the family was assigned to the treatment group and zero otherwise, COVS is a vector of covariates, PRD is a period fixed effect, QTR is a quarter of randomization fixed effect, and ε is an error term.[14] We estimate this equation using linear probability (ordinary least squares) regressions. In addition to the primary (CPS) outcomes, we also examine whether treatment assignment to GAIN increased total income and income stability (defined by the CV), which are hypothesized to be the primary mechanisms

[14] In supplemental analyses (not shown), we conducted hazard models (survival analyses) estimating the difference in rate of CPS re-involvement between randomization and either 12- or 24-month follow up for the GAIN and control groups in each cohort. Results were consistent with those presented here.

through which GAIN participating should reduce CPS re-reports, using the same model specification. For the Survey Cohort, we also use this specification to examine whether GAIN had effects on each of the self-reported survey outcomes at 12-month follow-up.

RESULTS

Sample Description

Table 2 presents descriptive statistics by GAIN (treatment) or control assignment for each cohort. On the whole, this is a quite disadvantaged population. Average annual income for these families in the year before random assignment was roughly $15,000 to $17,000. Moreover, while the vast majority (more than 80%) had some earnings in the quarter before randomization, about 42% had at least one quarter with no on-the-books earnings in the year before randomization (not shown). They also had very high rates of SNAP and BadgerCare (MA/SCHIP) participation. Approximately 80% of Early Cohort and upwards of 70% of Late Cohort and Survey Cohort families received SNAP in the quarter before randomization, and 70% of Early Cohort and of upwards of 60% of Late and Survey Cohort families received BadgerCare. The uptick in family income and employment, and decline in SNAP and BadgerCare participation between cohorts likely reflects a combination of the improving labor market over the years following the Great Recession and the expiration in 2014 of many of the benefit expansions under the *American Recovery and Reinvestment Act of 2009*; as noted above, Early Cohort randomization occurred from November 5, 2012 to October 9, 2014, Late Cohort randomization from October 10, 2014 to January 10, 2016, and Survey Cohort randomization from February 3, 2016 to September 21, 2016. In addition to having low incomes and high levels of benefit receipt, sample families are relatively large, including nearly 4 children, on average. They are also predominantly single-parent families and families of color. Most had experienced at least one prior CPS investigation.

Despite randomization, the descriptive statistics also indicate that there were a few small differences in observable characteristics between families randomized to the GAIN and control groups in each cohort. In the Early Cohort, caregivers in the treatment group were slightly older than those in the control group (31.5 versus 30.6), were slightly less likely to be Hispanic (14.3% versus 17.4%) and more likely to be Native American (4.4% versus 2.5%), and slightly more likely to be U.S. born (89.9% versus 86.7%). In the Late Cohort, treatment group families were somewhat less likely to receive BadgerCare (61.0% versus 65.4%), as well as marginally significantly ($p<.10$) more likely to be headed by a single father (7.8% versus 6.0%). They also had fewer prior CPS investigations (1.5 versus 1.7). In the Survey Cohort, Hispanic and Asian families were slightly overrepresented in the control group, and treatment group families had a higher rate of missing information on days between CPS report and randomization. These differences suggest we should prioritize the regression adjusted, rather than raw mean, ITT effects.

Take-up and Dosage

Table 3 presents descriptive statistics regarding program dosage and the characteristics associated with program take-up (participation), defined as engaging in at least on in-person visit with a GAIN worker, among families randomly assigned to Project GAIN (treatment group). Overall, 31.4% of Early Cohort, 21.8% of Late Cohort, and 61.5% of Survey Cohort families assigned to the treatment group agreed to participate in GAIN. Notably, GAIN staff were unable to make any contact with 35.3% of Early Cohort, 39.0% of Late Cohort, and 15.3% of Survey Cohort treatment group families; thus, among those families with which they were able to establish contact participation rates were 48.5%, 35.7%, and 72.6%, respectively.

It is also important to note that GAIN is a relatively light touch intervention. Average length of participation for Early, Late, and Survey cohort members who took up treatment was 10.8, 11.8, and 8.3 (with medians of 8, 10, and 9) weeks respectively. Moreover, mean (and median) number of in-home visits for this group were 3.9 (2), 2.8 (2), and 1.64 (1) for the three cohorts, respectively. For the Early Cohort, 27.2% of GAIN-participating families had only one visit, 24.5% had only two visits, 10.3% had 3 visits and 38.1% had four or more visits. These figures were 39.2%, 18.8%, 15.9%, and 26.1%, for Late Cohort GAIN-participating families, and 39.1%, 13.3%, 14.2%, and 10.7%, for Survey Cohort participant families.[15]

Furthermore, the estimates in Table 3 indicate that participation by those offered services was not random. Rather, they suggest that less-advantaged families were more likely to participate if offered the chance. Although there was no systematic difference in program participation by overall income for the Early and Late cohorts, lower-income Survey Cohort families were significantly more likely to participate. Moreover, in all cohorts families that were receiving SNAP and/or BadgerCare were disproportionately likely to participate. Black families were disproportionately likely, and white families disproportionately unlikely to participate. In addition, U.S. born caregivers were more likely to participate in GAIN than were immigrant caregivers.

There were also a few differences in the characteristics of participants between cohorts. Participants in the Early (but not Late or Survey) Cohort, on average, had children with a greater number of partners (marginally significant at $p<.10$); also, Hispanic caregivers were

[15] Average length of participation for all of those assigned to the treatment group for the Early, Late, and Survey cohorts was 5.7, 4.8, and 6.4 weeks. Mean (and median) number of in-home visits were 1.3 (0), .63 (0), and 1.0 (0) for the three cohorts, respectively. For the Early Cohort, 8.8% of families had only one visit, 7.9% had only two visits, 3.3% had 3 visits and 12.3% had four or more visits. These figures were 8.8%, 4.2%, 3.6%, and 5.9% for the Late Cohort and 24.0%, 8.2%, 8.7%, and 6.6% for the Survey Cohort.

disproportionately unlikely to take up GAIN in the Early Cohort only. In the late cohort, Native American caregivers were disproportionately unlikely to take up the program, as were two-parent households. Single-mother households were disproportionately likely, and two-parent households disproportionately unlikely, to participate in the Late and Survey cohorts. Families that had experienced a prior CPS substantiation were disproportionately likely to participate in the Early and Late cohorts (both marginally significant at $p<.10$), whereas the rate of prior CPS substantiation was similar for participants and non-participants in the Survey Cohort.

ITT Analyses Using Administrative Data

CPS Re-investigation

Full sample. Raw ITT estimates (mean differences) and regression ITT adjusted estimates are presented in Figures 3 and 4, respectively.[16] These estimates are interpreted as average percentage-point differences in CPS re-investigation between all treatment group families (regardless of program take up) and all control group families. Figure 3 shows that there are no statistically significant mean differences between the GAIN and control groups in CPS re-investigation in the 12 or 24 months following random assignment. However, considering the general direction of the estimates suggests almost no effect for the Early Cohort, for which the ITT suggests that GAIN group had 1 percentage point fewer re-investigations than the control group at 12 months, and 1 percentage point more re-investigations than the control group at 24 months; nor for the Survey Cohort, for which the estimates suggest GAIN to be associated with 1 percentage point fewer re-investigations at each time point. Though also nonsignificant, the Late Cohort estimates consistently favor the GAIN group at both time points, with 2 and 3 percentage point fewer re-investigations than the control group at 12 and 24 months, respectively. Given

[16] Full-sample results corresponding to Figures 3, 4, and 6-8 are presented in Appendix Table A1.

baseline re-investigation rates for the control group of 18% at 12 months and 27% at 24 months, these estimates suggest an ITT effect of GAIN on the order on an 11% reduction in CPS re-investigation at both 12 months and 24 months post randomization.

The regression adjusted estimates for the Late Cohort are somewhat smaller in magnitude than the unadjusted estimates, indicating that treatment assignment is associated with a nonsignificant .4 percentage point (2.3%) reduction in subsequent CPS investigation at 12 months and nonsignificant 1.4 percentage point (5.2%) reduction at 24 months. Moreover, the regression adjusted ITT estimates for the Survey Cohort show a nonsignificant and modest decrease in CPS involvement for the GAIN-assigned group of 6.3% at 12 months and 7.0% at 24 months. Consistent with the raw data, the Early Cohort regression adjusted estimates are nonsignificant and close to zero in absolute magnitude.

To examine whether differences in the pattern of results by cohort reflect that the inclusion criteria for the Early Cohort stipulated that the family have a child age 5 or under (although, as noted above, this criterion appears not to have been applied), while there was no such child age criterion for the Late or Survey cohorts, we conducted sensitivity analyses in which we limited each cohort to only those families with a child age 5 or under. These results are presented in Figure 5. They indicate that the differential pattern is not driven by the child age criterion. The regression adjusted ITT effect of GAIN for Late Cohort families with a child 5 and under is considerably larger than that for the full Late Cohort Sample. These estimates indicate a (nonsignificant) 1.6 percentage point (7.4%) reduction in reinvestigation for the treatment group at 12 months and (nonsignificant) 4.4 percentage point reduction (13.4%) at 24 months (compared to .4 and 1.4 percentage point [2.3% and 5.2%] reductions among the full sample). For the Survey Cohort, however, they indicate a small nonsignificant increase in CPS re-

investigation at 12 months (1 percentage point or 5.2%) and a larger nonsignificant increase in CPS re-investigation of 7.5 percentage points 26.7% at 24 months. However, the sample size for this analysis is quite small, with a total of only 193 cases (100 treatment and 93 control).

Heterogeneity by income tercile. Figures 6 through 8 show regression adjusted results separately by within-cohort income tercile in the quarter before birth for the Early (Figure 6), Late (Figure 7), and Survey (Figure 8) cohorts, respectively. Note that, this is a very low-income population: Early Cohort income tercile ranges are $0-$9,818, $9,819-$17,152, and $17,193-$37,002; Late Cohort income tercile ranges are $0-$8,870, $8,875-$17,188, and $17,189-$44,923; and Survey Cohort ranges are $0-$9,925, $9,926-18,958, and $18,959-$66,188. As with the overall results, we find no statistically significant differences. The pattern of estimates, however, again differs somewhat across cohorts. For the Early Cohort, the estimates are all clustered relatively close to zero, though with some indication of a decrease in re-investigation associated with GAIN assignment for the lowest income tercile, at 12 and 24 months, and an increase in re-investigation associated with GAIN assignment for the highest income tercile at 12 months. In contrast, for the Late Cohort, the (nonsignificant) ITT estimates suggest a lower likelihood of re-investigation in the bottom two terciles, though a higher likelihood for the highest tercile, for the GAIN group at both 12 and 24 months. Despite not attaining statistical significance, these effect sizes are relatively large. Re-investigation rates for Late Cohort control group families in the bottom, middle, and top terciles were 17.3%, 22.6%, and 12.3% at 12 months and 26.7%, 35.0%, and 19.4% at 24 months (see Appendix Table A1). This suggests that at 12 (24) months, Late Cohort GAIN assignment was associated with an 11.0% (17.2%) percent decrease in CPS re-investigation for the bottom income tercile and a 9.7% (13.1%) percent decrease for the middle tercile, but with a 14.6% (19.1%) percent increase in CPS re-

investigation for the top income quintile. The Survey Cohort estimates are quite consistent with those from the Late Cohort, suggesting nonsignificant decreases in re-investigation for the GAIN group ranging from 17% to 24% for the lowest and middle terciles, and nonsignificant increases in re-investigation on the order of 11% to 12% for the top.[17]

Neglect allegation in initial investigation. Because prior literature indicates that economic resources are more closely linked to child neglect than to other forms of child maltreatment, we next limit the sample to only those families for which the initial investigation (which triggered inclusion in the sample) included an allegation of child neglect. Here, we see that the full sample estimates (Figure 9) for the Early and Late cohorts are nonsignificant and relatively small in magnitude, but suggestive of decreases in re-investigation for all but the Early Cohort at 12 months. The Survey Cohort estimates however, though nonsignificant, suggest reductions in CPS re-investigation for treatment group families of 2.7 and 3.8 percentage points (12.9% and 11.0%) at 12 and 24 months. In addition, the bottom income tercile estimates, while always nonsignificant, suggest a relatively large reduction in re-investigation, particularly for the Late and Survey cohorts, and the estimates for the middle income tercile of the Survey Cohort also suggest a large decrease in re-investigation (Figures 10 through 12).[18]

Income and Income (In)stability

GAIN participation was intended to increase both income level and income stability. Thus, we present ITT estimates of GAIN assignment on each. Full-sample estimates for total income (Figure 13) indicate no statistically significant effects for the Early and Late cohorts, for

[17] Supplemental analyses by income tercile for only those families with a child age 5 or under (results not shown) reveal a consistent pattern of estimates, indicating that the reduction in CPS re-investigation for the Early and Late cohorts is concentrated among the lowest income tercile and that the increase in re-investigation in the Survey Cohort is concentrated among the top income tercile.

[18] Full-sample results corresponding to Figures 9-12 are presented in Appendix Table A2.

which effect sizes are close to zero (the direction of the estimates suggests that treatment assignment was associated with small nonsignificant increases in income on the order of 1% to 2%). However, treatment assignment was associated with a significant and modest *decrease* in income of roughly $1,000 or 5.6% at 12-month follow up for the Survey Cohort. Though nonsignificant, the Survey Cohort estimate at 24 months indicates a $1,300 (3.6%) *decrease* in income. By comparison, the estimates by income tercile are relatively inconsistent across cohorts. For the Early Cohort, the pattern suggest modest income increases for the lowest tercile and relatively no effect for the upper two terciles (Figure 14). For the Late Cohort, the pattern suggests no effects on for the lowest tercile and a small positive effect for the upper two terciles (Figure 15). Estimates for the survey cohort suggest decreases in income across all terciles, though these tend to be largest for the bottom and top terciles (Figure 16).[19]

ITT effects for income instability, assessed via the CV for income during the 12- or 24-month follow-up period, are presented in Figures 17 through 20. The full sample results (Figure 17) indicate that GAIN assignment was associated with a small nonsignificant decrease in income instability for the Early Cohort and a modest and statistically significant decrease in income instability (roughly 9% at each time point) for the Late Cohort at both 12- and 24-month follow-up. Estimates for the Survey Cohort are nonsignificant but suggest increased income instability at 12-months and decreased income instability at 24 months. As shown in Figures 18 and 19, decreased income instability in the Early and Late cohorts is concentrated among, and statistically significant for, families in the lowest income tercile in each cohort. Effect sizes suggest a 14.2% decrease in income instability at 12 months and a 22.9% decrease at 24 months for bottom-tercile Early Cohort families, and decreases of 19.2% and 20.2%, respectively, for

[19] Full-sample results corresponding to Figures 13-16 are presented in Appendix Table A3.

Late Cohort Families.[20] In contrast, the estimates for the Survey Cohort suggest increased income instability among bottom-tercile families (Figure 20).

Figures 21 through 28 further examine whether any changes in income stability may have been driven by changes in earnings instability (Figures 21 through 24) or benefits instability (Figures 25 through 28). In the Early and Late cohorts we see small nonsignificant decreases in earnings instability, ranging in magnitude from 1.1 to 1.9 percentage points (2.2% to 4.3%); for the Survey cohort we see a small increase in earnings instability of roughly 1.0 to 2.9 percentage points (1.9% to 5.2%) (Figure 21). Among the Early Cohort, decreased earnings instability appears to be concentrated in the top income tercile (Figure 22), whereas it seems to be concentrated in both the bottom and top income terciles for the Late Cohort (Figure 23). For the Survey Cohort, the pattern suggests reduced earnings instability at 12 months but increased earnings instability at 24 months for the bottom and middle terciles, but increased earnings instability at both time points for the top tercile (Figure 24).

We see a different pattern for benefit instability. On the whole, the Early Cohort experienced small nonsignificant increases (0.7 to 2.8 percentage points [1.4% to 8.3%] at 12 and 24 months, respectively) in benefit instability, whereas the Late Cohort experienced small nonsignificant decreases (0.4 to 1.3 percentage points [0.7% to 3.4%]), and the Survey Cohort experienced a modest increase (4.6 percentage points [14.3%]) at 12-month follow up and a small decrease (1.3 percentage points [2.6%]) at 24-month follow up (Figure 25). Of note, for the Early Cohort, increased benefit instability was concentrated among the top income tercile, for which benefit instability significantly increased by 8.5 percentage points (22.0%) at 12 months and 10.3 percentage points (18.0%) at 24 months. For the Early Cohort lowest income tercile,

[20] Full-sample results corresponding to Figures 17-20 are presented in Appendix Table A4.

however, benefit instability decreased by 1.2 percentage points (3.8%) (nonsignificant) at 12 months and 8.2 percentage points (16.8%) at 24 months (marginally significant at p<.10) (Figure 26). The pattern by income tercile was similar for the Late Cohort (though the magnitude of effect for the lowest tercile was considerably larger): Top tercile Late Cohort participants saw increases in benefit instability of 3.9 percentage points (8.9%) (nonsignificant) at 12 months and 9.5 percentage points (15.2%) (significant) at 24 months; their bottom tercile counterparts experienced decreases in benefit instability on the order of 6.0 percentage points (15.8%) (marginally significant) to 9.4 (significant) percentage points (17.5%) at 12 and 24 months, respectively (Figure 27). Though the estimates are nonsignificant and less pronounced for the Survey Cohort, they also suggest decreases in income instability for the lowest income families (Figure 28).

ITT Analyses Using Survey Data

Primary Outcomes

Table 4 presents ITT estimates for the primary survey outcomes. Notably, the estimates for each of these measures, with the exception of self-reported income, are in the unexpected direction, suggesting poorer parenting behaviors and economic functioning. These effects are nonsignificant and generally small in magnitude for neglectful behaviors, emotional abuse behaviors, and parenting distress. They are significant and modest in magnitude in terms of physical aggression behaviors, economic stress, and material hardship, indicating increases in each, respectively, of .18, .18, and .17 standard deviations.

Secondary Outcomes

We find no significant differences between the GAIN and control group for the secondary survey outcomes related to parental functioning (Table 5). Moreover, the direction of

the effects is inconsistent across parenting measures, suggesting poorer parenting among the GAIN than control group on 6 of the 9 outcomes (warmth/affection, patience, leniency, routines, social connections, and social and emotional competence) and better parenting among the GAIN group on 3 (attention, resilience, and concretes support). For the two measures of economic wellbeing, the estimates suggest a small nonsignificant increase in housing hardship at .10 standard deviations and a modest significant increase in food hardship at .17 standard deviations among the GAIN group relative to the control group.

Ancillary Outcomes

Similarly to the secondary outcomes, we find an inconsistent pattern of results for the parental functioning-related ancillary outcomes, with 2 of the 5 measures (adult relationship quality and recent intimate partner violence) suggesting better functioning for the GAIN group and 3 (depressive symptoms, self-efficacy, and toxic social network) suggesting poorer functioning for that group. Notably, only toxic social network is (marginally) significant (at $p<.10$), with a modest effect size of .13 standard deviations. With respect to the two ancillary measures of economic functioning, we find a large and significant 12.6 percentage point (48.8%) increase in the likelihood of reporting having household debt and a nonsignificant (though small and positive) association of GAIN assignment with reporting EITC receipt.

DISCUSSION

To our knowledge, Project GAIN is the first randomized evaluation of an economic support program specifically intended to reduce child maltreatment. The intervention focused on providing an economic resource worker to assist families in increasing and stabilizing their income. It targeted families who had been investigated by CPS but for whom the investigation was closed within 90 days of initiation with no child safety concerns, and no ongoing CPS case

initiated. Such families are, on average, quite socioeconomically disadvantaged and have very high rates of subsequent CPS involvement. Services provided included an assessment of economic needs and assistance with short-term financial decision-making, a comprehensive determination of eligibility for an array of public and private economic benefits and supports, and assistance accessing those resources, and a limited amount of flexible funding to assist families experiencing a specific short-term economic crisis (e.g., a utility shut-off, food shortage) with a one-time purchase of goods or services on the family's behalf.

Notably, GAIN was a relatively short-term, low-intensity intervention, with families that took up the intervention participating on average for about 10 weeks and experiencing 1 to 4 in-person visits. Unfortunately, the ITT estimates do not provide clear evidence that GAIN produced its intended effects on income level, income stability, or CPS re-investigation. For the most part, estimates are nonsignificant. Moreover, many are close to zero in absolute magnitude. However, they tend to differ in pattern across cohorts. As such, there are kernels of promise in our findings: there is some suggestion of an income stabilizing and re-investigation reducing ITT effect for the lowest income families, those whose initial investigation included a neglect allegation, and those with children age 5 or younger, particularly in the Late Cohort. There are also some areas of concern: The ITT patterns also suggest potential increases in CPS re-investigation and income instability for higher-income, but still economically disadvantaged families (those with incomes ranging from about $17,000 to $66,000 in the year before randomization). Notably, this does not appear to reflect increased surveillance of families already at risk for child maltreatment on the part of GAIN staff—program documentation and direct feedback from staff indicate that they made no CPS reports on any of the families they served.

Below, we review and contextualize our major findings and discuss potential explanations thereof.

Take-up rates for participants randomly assigned to receive Project GAIN services were 31.4%, 21.8%, and 61.5% for the Early, Late, and Survey Cohorts, respectively. These rates are lower than estimates of participation based on an implementation evaluation of several pilot interventions around the state of Wisconsin targeting families reported to CPS whose cases closed at the hotline call or investigation closure stage (Maguire-Jack, Slack, & Berger, 2014). These program participation rates averaged 54%, although they ranged from 28 to 83%. In comparison to home visiting programs, where take-up rates can range from half to nearly 100%, Project GAIN take-up rates are low (MIECHV, 2015). However, unlike most home visiting programs designed to prevent child maltreatment, Project GAIN sought to involve families that had recently been investigated for child maltreatment allegations—a highly intrusive and likely unwelcome intervention—which could lead to less willingness on the part of parents to participate in GAIN services. Furthermore, participation (or "enrollment") rates for home visiting programs are often based on the percentage of service capacity used (i.e., the number of participating families divided by the number of service slots; MIECHV, 2015) whereas, in Project GAIN, the denominator (at least in the Early and Late Cohorts) was the entire population of eligible families, a fair number of whom the Project GAIN staff were not able to locate. As noted above, GAIN staff were unable to make any contact with 35.3% of Early Cohort, 39.0% of Late Cohort, and 15.3% of Survey Cohort treatment group families.[21] Thus, among those families with which they were able to establish contact, participation rates were 48.5%, 35.7%, and 72.6%, respectively, which more closely align with those of existing programs for similarly

[21] The considerably higher contact rate for the Survey Cohort reflects that the UWSC had already located and contacted these families and that they also agreed to complete a baseline interview.

vulnerable populations. Nonetheless, that only a fifth to a third of families assigned to the treatment group in the Early and Late cohorts participated in the program will serve to dampen any ITT effects in those samples—be they positive or negative.

While take-up was much higher for the Survey Cohort, as noted above that sample consists of families that agreed to participate in a baseline survey prior to randomization. These families were much more likely to take-up the program; fully, 61.5% of those assigned to the treatment group participated. The high rate of program participation in the cohort should result in much larger ITT effects, all else equal. At the same time, the considerably smaller sample size for the cohort should result in less precise estimates. Of course, families that agreed to participate in the baseline survey (and therefore to be randomized into GAIN) make up a different population than those randomly assigned without first agreeing to an interview. Beyond limited take up among the Early and Late cohorts, however, it is also important to reiterate that GAIN was a low-intensity, low-dosage intervention, which may have contributed to its lack of significant impacts.

Beyond the overall take-up rate, there is evidence that more disadvantaged families (i.e., those receiving SNAP or Wisconsin's Medicaid program, BadgerCare, and, although only marginally significant, those who had experienced CPS involvement at some point prior to the index report generating randomization into the GAIN evaluation) were more likely to enroll in the GAIN program. This, itself, could be considered a measure of success, since it can be argued that more disadvantaged families are in greater need of an economic support intervention—and may also be willing to engage in one. At the same time, however, that the group that chose to participate in GAIN was more disadvantaged that the non-participant groups may also have influenced the ITT estimates.

It is striking that the ITT estimates differ so considerably in magnitude and, in some cases, direction across cohorts (despite little evidence of statistical significance in any cohort). For the most part, the Early Cohort estimates suggest effect sizes that are quite close to zero or, in some cases, lean toward an adverse effect of the intervention, especially for higher-income sample families. In contrast, the Late Cohort and Survey Cohort ITT estimates tend to be larger in magnitude and operate in hypothesized directions, particularly for lower-income families. We offer several hypotheses to contextualize these different trends.

First, the economic climate, in terms of both the labor market and social policy context, was rapidly changing across the observation period, which followed the Great Recession and, in later periods, was characterized by relatively rapid economic growth. Figure 29 presents unemployment, social welfare benefit participation, and CPS trends by month in Milwaukee County, across the study period. Notably, the unemployment rate decreased substantially throughout the study period from a high of 9.2% in early 2013 to a low of 4.6% in late 2016. The mean unemployment rate facing Early Cohort families was 7.8%, whereas it was 5.6% for Late Cohort families and 4.6% for Survey Cohort families. Moreover, during the course of the evaluation, BadgerCare (Medicaid and SCHIP) participation generally expanded, whereas SNAP participation contracted. The SNAP decline, in particular, likely reflected the expiration of expanded benefits and eligibility criteria under the *American Recovery and Reinvestment Act of 2009*. Thus, the two cohorts faced very different labor markets and social welfare benefit contexts. This may help to explain why we find more promising support for GAIN having potential to reduce income and earnings instability, but also to increase benefit instability for

higher-income families who may have been more likely to lose benefits when they contracted,[22] among the Late Cohort.

Second, CPS practices and trends may have changed over time. Figure 29 shows both that the overall CPS report rate in Milwaukee grew substantially over the course of the study period and that the screen-in-to-screen-out ratio for CPS investigations also changed considerably. Specifically, families were more likely to be screened in for a CPS investigation during the Early Cohort period and more likely to be screened out during the Late and Survey cohort periods. Our presumption is that the increase in CPS reports over time likely reflected the expanding opioid epidemic. If so, however, we would have expected better results for GAIN in the earlier, rather than later period (e.g., if the earlier period were characterized more heavily by economic issues and the latter by substance abuse issues). However, our results do not support this supposition.

An additional aspect of CPS practices that may have changed during the study period relates to the amount of time during which cases remained open for investigation. The vertical background lines in Figure 29 depict the proportion of cases in the sample for which the investigation was completed in under 60, 60 to 90, and more than 90 days. There is extreme variation in these proportions, with periods with the largest number of investigations remaining open for more than 90 days likely reflecting periods of high CPS worker turnover (Stephenson, 2014). On the whole, however, cases remained open for longer, on average, during the Early Cohort observation period and, particularly, during the latter portion thereof, than during the Late Cohort period and, to a lesser extent, the Survey Cohort period. Of particular concern, only those families whose cases were closed within 90 days of the initiation of the investigation were

[22] Note that increased benefit instability may reflect gains or losses in benefits.

randomized for GAIN inclusion. Thus, if cases that remained open for more than 90 days were systematically excluded, the study sample may have varied during different periods of randomization. Specifically, differences in timing from investigation initiation to randomization could potentially be linked to participant characteristics, maltreatment risk, likelihood of participation, and response to the intervention. That we control for time from investigation to randomization, as well as both randomization period and quarter fixed effects within each cohort, should reduce such bias in the within-cohort estimates; but, between-cohort differences in estimates may reflect such factors. More generally, we cannot be certain we have fully accounted for the possibility that the population of randomized families may have changed over time

Fourth, the treatment effect may have varied across time periods with different random assignment ratios in response to program capacity, as well as changes in take-up rates and program dosage. Again, that we control for time from investigation to randomization, as well as both randomization period and quarter fixed effects within each cohort should reduce potential within-cohort bias. However, we cannot assume differences in ITT estimates between cohorts are free from such bias. Of particular note, GAIN take up was considerably lower in the Late Cohort than the Early Cohort (21.8% versus 31.4%).[23] This may imply that workers had greater capacity to serve clients in the Late Cohort (because they were serving fewer clients) and/or that those who took up GAIN in the Late Cohort were systematically more likely to benefit from the program. Each of these considerations may have implications for why the ITT effects were generally more promising among the Late Cohort. Finally, it is possible that the intervention simply improved over time as workers gained experience delivering project GAIN.

[23] As noted above, take up in the Survey Cohort (61.5%) was much higher than that in the other cohorts. However, this likely reflects differences in sample selection with respect to eligibility for randomization which, for the Survey Cohort, was conditional on completing a baseline survey interview.

The Survey Cohort evaluation benefitted from baseline and 12-month follow-up survey data through which to assess whether GAIN participation impacted family financial functioning and decision making, parenting behaviors and the quality of the home environment, and behaviorally-approximated child maltreatment indicators, in addition to income and income stability (and sources thereof), and CPS re-investigation. As with the administrative data results, these results were largely nonsignificant. They were also inconsistent in pattern. Notably, however, they also suggest that those assigned to the GAIN group reported poorer family functioning relative to the control group in several domains at 12-month follow up. Most notably, the results suggest increases in parental physical aggression toward children, material hardship, economic stress, food hardship, toxic social networks, and debt for the GAIN group. Why might this be?

One possibility is that the involvement of a service provider in the life of a family already experiencing severe disadvantage, itself, can create change and disruption, even if the service is considered desirable by a family. In the short-term, such disruption may create a certain amount of stress that can, in turn, produce (adverse) behavior changes. For example, it is possible that participating in GAIN introduced a short-term influx of resources in a family, which for some may have led to short-term spending decisions that created longer-term hardship, such as an increase in debt. Families are also embedded in a social network and, for some, this network may be a financial drain. Increased resources for one family in a network could lead to increased requests for economic support to others in the network, which could result in a net loss for the GAIN-participant family. It is also possible that participating in GAIN helped primary caregivers make a housing move or end a relationship, both of which could be viewed as positive outcomes, but could also be associated with increased economic hardship in the longer-term.

An additional possibility is that interactions with the GAIN worker led families to pay more attention to their financial situation and that doing so, itself, may have led to increased stress. That is, families may have become more aware of their debts or, more generally, their economic vulnerability because of the intervention, which may have led to increased reporting of such vulnerability at follow up. Notably, we find little evidence for the Survey Cohort of any changes in income or income stability as a result of the intervention.

One could also argue that adverse family functioning outcomes such as those observed in the Survey Cohort are not of the same magnitude as the observed reductions in CPS involvement for some sample subgroups. That is, the fact that CPS involvement declines for some of the more vulnerable subgroups in the latter two cohorts, and that this effect appears to strengthen over time—even if statistically nonsignificant—is potentially a much more impactful finding than a point-in-time caregiver assessment of economic stress or physical aggression with children. While these outcomes are indeed concerning, CPS involvement is arguably a signal of much more severe disruptions in family functioning, comprising a highly intrusive intervention that can lead to further challenges for a family. The fact that a brief, light-touch economic support intervention was able to produce any movement in CPS involvement, even if only at the trend level, in spite of low take-up rates and limited changes in underlying economic resources, suggests that the intervention may hold promise for some, but not all, families.

In sum, while this study provides no conclusive evidence to support the efficacy of Project GAIN, nor does it fully reject its potential. On the whole, the ITT estimates are predominantly nonsignificant for the three outcomes we examined—CPS re-investigation, income level, and income (in)stability—however, there are some hints, particularly in the Late Cohort, that the program—or, perhaps a more intensive version thereof—warrants further testing

and evaluation. Indeed, the intervention may have lacked the intensity necessary to impact income levels and volatility. At the same time, we find some suggestive evidence that it may hold potential for the lowest-income families, families with child neglect allegations, and families with young children. More generally, future research should continue to examine the potential causal role of income level and income stability on child maltreatment, including CPS involvement.

REFERENCES

Abidin, R. (2012). *Parenting Stress Index, 4th Edition.* Odessa, FL: Psychological Assessment Resources. *Inc.*

Barth, R.P., Gibbons, C. & Guo S. (2006). Substance abuse treatment and the recurrence of maltreatment among caregivers with children living at home: A propensity score analysis. *Journal of Substance Abuse Treatment, 30*, 93–104.

Berger, L.M. (2004). Income, family structure, and child maltreatment risk. *Children and Youth Services Review, 26*(8), 725-748.

Berger, L. M., Font, S. A., Slack, K. S., & Waldfogel, J. (2017). Income and child maltreatment in unmarried families: evidence from the earned income tax credit. *Review of Economics of the Household, 15(4)*, 1345-1372.

Cancian, M., Noyes, J., & Kim, H. (2017). *Using linked administrative data to understand the dynamics of multiple program participation*. Report to the Wisconsin Department of Children and Families, December, 2017.

Cancian, M., Yang, M. Y., & Slack, K. S. (2013). The effect of additional child support income on the risk of child maltreatment. *Social Service Review, 87(3)*, 417-437.

Connell, C.M., Bergeron, N., Katz, K.H., et al. (2007). Re-referral to child protective services: The influence of child, family, and case characteristics on risk status. *Child Abuse & Neglect, 31,* 573–588.

Conrad-Hiebner, A. & Byram, E. (2020). The temporal impact of economic insecurity on child maltreatment: A systematic review. *Trauma, Violence & Abuse, 21(1),* 157-178.

Doidge, J. C., Higgins, D. J., Delfabbro, P., Edwards, B., Vassallo, S., Toumbourou, J. W., & Segal, L. (2017). Economic predictors of child maltreatment in an Australian population-based birth cohort. *Children and Youth Services Review, 72*, 14-25.

Drake, B., Jonson-Reid, M., Way, I., & Chung, S. (2003). Substantiation and recidivism. *Child Maltreatment, 8*(4), 248.

Eaton, W.W., Muntaner, C., Smith, C., Tien, A., Ybarra, M. (2004). Center for Epidemiologic Studies Depression Scale: Review and revision (CESD and CESD-R), pp. 363-377. In M.E. Maruish (Ed.), *The Use of Psychological Testing for Treatment Planning and Outcomes Assessment, 3rd Edition*. Mahwah, N.J.: Lawrence Erlbaum.

Fragile Families and Child Wellbeing Study. (2008). Scales documentation and question sources for five-year questionnaires. Retrieved from http://www.fragilefamilies.princeton.edu/documentation/core/scales/ff_5yr_scales.pdf.

Gennetian, L. A., Rodrigues, C., Hill, H. D., & Morris, P. A. (2019). Income Level and Volatility by Children's Race and Hispanic Ethnicity. *Journal of Marriage and Family*, 81(1), 204-229.

Gennetian, L. A., Wolf, S., Hill, H. D., & Morris, P. A. (2015). Intrayear household income dynamics and adolescent school behavior. *Demography*, 52(2), 455-483.

Hardy, B. L. (2017). Income instability and the response of the safety net. *Contemporary Economic Policy*, 35, 312-330.

IOM (Institute of Medicine) and NRC (National Research Council) (2014). *New Directions in Child Abuse and Neglect Research* (Chapter 2). Washington D.C.: The National Academies Press.

Kim, H., Wildeman, C., Jonson-Reid, M. & Drake, B. (2017). "Lifetime Prevalence of Investigating Child Maltreatment Among US Children." *American Journal of Public Health,* 107:274-280.

Kiplinger, V. L. & Harper Browne, C. (2014). *Parents' Assessment of Protective Factors: User's Guide and Technical Report*. Washington, DC: Center for the Study of Social Policy.

Kohl, P.L., Jonson-Reid, M. & Drake, B. (2009). Time to leave substantiation behind: Findings from a national probability study. *Child Maltreatment, 14*, 17–26.

Lewis, D., Shook, K., Stevens, A.B.. Kleppner, P., Lewis, J. & Riger, S. (2000). Work, welfare, and well-being: an independent look at welfare reform in Illinois (Illinois Families Study: project description and first year report). Evanston, IL: Northwestern University.

Maguire-Jack, K., & Font, S.A. (2017). Intersections of Individual and Neighborhood Disadvantage: Implications for Child Maltreatment. *Children and Youth Services Review 72,* 44-51.

Maguire-Jack, K., Slack, K.S., and Berger, L.M. (2014). Wisconsin's Community Response Program for families that have been reported for child maltreatment. *Child Welfare*, 92(4), 95-121.

MIECHV TACC, Maternal, Infant, and Early Childhood Home Visiting Technical Assistance Coordinating Center (2015). *MIECHV Issue Brief on Family Enrollment and Engagement.* Accessed on November 30, 2019 at: https://mchb.hrsa.gov/sites/default/files/mchb/MaternalChildHealthInitiatives/HomeVisiting/tafiles/enrollmentandengagement.pdf

Pearlin, L.I., Lieberman, M.A., Menaghan, E.G., & Mullan, J.T. (1981). The stress process. *Journal of Health and Social Behavior*, 22(4), 337–356.

Pelton, L. (2015). The continuing role of material factors in child maltreatment and placement. *Child Abuse & Neglect, 41*, 30-39.

Radloff, L.S. (1977). The CES-D scale: A self-report depression scale for research in the general population. Applied Psychological Measurement, 1, 385-401.

Raissian, K. M., & Bullinger, L. R. (2017). Money matters: Does the minimum wage affect child maltreatment rates? *Children and Youth Services Review, 72*, 60-70.

Sedlak, A.J. (1991). National incidence and prevalence of child abuse and neglect: 1988 (revised report). Rockville, MD: Westat.

Sedlak, A.J. and Broadhurst, D.D. (1996). *Third National Incidence Study of Child Abuse and Neglect: Final Report.* Washington, D.C.: U.S. Department of Health and Human Services.

Sedlak, A.J., Mettenburg, J., Basena, M., Petta, I., McPherson, K., Greene, A., & Li, S. (2010). *Fourth National Incidence Study of Child Abuse and Neglect (NIS-4): Report to Congress*. Washington, D.C.: U.S. Department of Health and Human Services, Administration for Children and Families.

Schneider, W., Waldfogel, J., & Brooks-Gunn, J. (2017). The Great Recession and risk for child abuse and neglect. *Children and Youth Services Review, 72*, 71-81.

Shook [Slack], K.S. (1999). Does the loss of welfare income increase the risk of involvement with
the child welfare system? *Children and Youth Services Review*, *21 (8/9*), 693-724.

Slack, K. S., Berger, L. M., DuMont, K., Yang, M., Erharhd-Dietzel, S., & Holl, J. (2011). Predicting child neglect of young children: A comparison of three samples. *Children and Youth Services Review, 33,* 1354-1363.

Slack, K.S., Berger, L.M., and Noyes, J.L. (2017). Introduction to the special issue on the economic causes and consequences of child maltreatment. *Children and Youth Services Review, 72,* 1-4.

Slack, K. S., Holl, J. L., Lee, B. J., McDaniel, M., Altenbernd, L., & Stevens, A. B. (2003). Child protective intervention in the context of welfare reform: The effects of work and welfare on maltreatment reports. *Journal of Policy Analysis and Management, 22(4),* 517-536.

Stephenson, C. (2014, September 18). *Child welfare bureau turnover: Backlog fuels concern about child safety.* Milwaukee Journal Sentinel. Accessed on November 30, 2019 at: http://archive.jsonline.com/watchdog/watchdogreports/child-welfare-bureau-turnover-backlog-fuel-concerns-about-safety-b99351742z1-275698211.html

Stith, S.M., Liu, T., Davies, L.C., Boykin, E.L., Alder, M.C., Harris, J.M. … (2009). Risk factors in child maltreatment: A meta-analytic review of the literature. *Aggression and Violent Behavior, 14,* 13-29.

Straus, M. A., & Gelles, R. J. (Eds.). (1990). *Physical violence in 8145 families in American families: risk factors and adaptations to violence*. New Brunswick, NJ: Transaction Publishers.

Straus, M. A., Hamby, S. L., Finkelhor, D., Moore, D. W., & Runyan, D. (1998). Identification of child maltreatment with the parent-child conflict tactics scales: development and psychometric data for a national sample of american parents. *Child Abuse & Neglect, 22*(4), 249-270.

White, O.G., Hindley, N., & Jones, D.P.H. (2015). Risk factors for child maltreatment recurrence: An updated systematic review. *Medicine, Science & the Law, 55(4),* 259–277.

Wildeman, C, Emanuel, N., Leventhal, J.M., Putnam-Hornstein, E., Waldfogel, J. & Lee, H. (2014). The prevalence of confirmed maltreatment among U.S. children, 2004-2011. *JAMA Pediatrics, 168(8)*, 706-713.

Wildeman, C., & Fallesen, P. (2017). The effect of lowering welfare payment ceilings on children's risk of out-of-home placement. *Children and Youth Services review, 72,* 82-90.

Wisconsin Department of Children and Families (2016). Unpublished data.

U.S. Department of Health and Human Services, Administration for Children and Families, Administration on Children, Youth and Families, Children's Bureau. (2019). *Child Maltreatment 2017.* Available from https://www.acf.hhs.gov/cb/resource/child-maltreatment-2017.

Yang, M. Y. (2015). The effect of material hardship on child protective service involvement. *Child Abuse & neglect, 41*, 113-125.

Table 1. Randomization periods, number of families randomized, treatment-to-control randomization ratio, and take-up rate, by cohort.

	Number of Families Randomized	Treatment-to-Control Randomization Ratio	Take-up rate
Panel A: Early Cohort			
Period 1: 11/5/12-10/31/13	1,697	1:2	34.0%
Period 2: 11/1/13-1/12/14	106	1:1	20.4%
Period 3: 1/13/14-5/26/14	303	2:1	33.5%
Period 4: 5/27/14-10/9/14	327	1:1	23.1%
Observations		2,433	
Panel B: Late Cohort			
Period 5: 10/10/14-10/27/14	95	2:1	25.4%
Period 6:10/28/14-11/9/2014	78	1:1	17.9%
Period 7: 11/10/14-1/31/15	391	1:2	25.9%
Period 8: 2/1/15-1/10/16	2,335	1:3	21.5%
Observations		2,899	
Panel C: Survey Cohort			
Period 9: 2/3/16-7/5/16	611	1:1	61.2%
Period 10: 7/6/16-8/8/16	110	4:1	64.3%
Period 11: 8/9/16-9/21/16	0	0:1	0%
Observations		721	

Note. Early Cohort families were randomized from November 5, 2012-October 9, 2014. Late Cohort families were randomized from October 10, 2014 to January 10, 2016. Survey Cohort families were randomized from February 3, 2016 to September 21, 2016.

Table 2. Sample characteristics by cohort and random assignment status

	Early Cohort			Late Cohort			Survey Cohort		
	Treatment	Control	T-test	Treatment	Control	T-test	Treatment	Control	T-test
Income in prior year (in $10,000s)	1.488	1.461		1.541	1.529		1.655	1.663	
	(1.010)	(0.981)		(1.248)	(1.173)		(1.147)	(1.269)	
Any earnings	0.841	0.849		0.859	0.841		0.825	0.820	
Received SNAP	0.794	0.805		0.711	0.732		0.762	0.730	
Received BadgerCare (MA/SCHIP)	0.727	0.714		0.610	0.654	*	0.699	0.677	
Caregiver Age	31.480	30.560	**	35.010	34.670		34.130	34.640	
	(8.960)	(8.055)		(10.310)	(9.538)		(9.789)	(9.380)	
Age of youngest child	3.246	3.132	*	6.757	6.420		8.039	8.602	
	(3.794)	(3.496)		(5.291)	(5.185)		(5.532)	(5.593)	
Age of youngest child missing	0.048	0.040		0.064	0.081		0.079	0.076	
Number of fathers to mother's children	0.965	0.974		0.951	0.948		0.940	0.955	
	(0.417)	(0.432)		(0.418)	(0.431)		(0.459)	(0.448)	
Number of fathers missing	0.100	0.105		0.105	0.117		0.132	0.118	
Number of Children	4.024	3.916		3.585	3.612		3.441	3.453	
	(2.810)	(2.620)		(2.557)	(2.533)		(2.514)	(2.462)	
Number of Children missing	0.000	0.001		0.000	0.000		0.005	0.008	
White	0.319	0.303		0.343	0.336		0.282	0.323	
Black	0.597	0.579		0.562	0.580		0.652	0.596	
Hispanic	0.143	0.174	*	0.171	0.163		0.142	0.188	+
Native American	0.044	0.025	*	0.031	0.037		0.030	0.042	
Asian	0.017	0.024		0.024	0.018		0.011	0.037	*
Race missing	0.008	0.010		0.005	0.002		0.000	0.000	
Caregiver US born	0.899	0.867	*	0.847	0.868		0.888	0.876	
Two-parent household	0.212	0.208		0.209	0.213		0.216	0.219	
Single-mother household	0.507	0.532		0.448	0.464		0.468	0.458	
Single-father household	0.039	0.041		0.078	0.060	+	0.049	0.053	
Family structure missing	0.164	0.162		0.198	0.199		0.200	0.188	

Number of previous investigations	1.308	1.272		1.472	1.690	+	1.480	1.440	
	(2.471)	(2.436)		(2.707)	(2.794)		(2.662)	(2.786)	
Ever previous substantiation	0.109	0.118		0.117	0.140		0.137	0.126	
Days from report to randomization	58.250	58.330		57.600	57.460		59.320	58.610	
	(16.160)	(16.001)		(16.690)	(15.620)		(14.444)	(15.216)	
Days to randomization missing	0.072	0.056		0.039	0.053		0.093	0.059	+
Observations	960	1473		779	2120		365	356	
Percent	39.5	60.5		26.9	73.1		50.6	49.4	

Note. 2,433 total observations for the Early Cohort; 2,899 total observations for the Late Cohort; 721 total observations for the Survey Cohort. Measures assessed in quarter prior to randomization unless otherwise noted.
+p<.10; *p<.05; **p<.01.

Table 3. Program dosage and sample characteristics associated with program participation among those assigned to treatment (GAIN), by cohort

	Early Cohort - Treatment			Late Cohort - Treatment			Survey Cohort - Treatment		
	Participant	Nonpart.	T-test	Participant	Nonpart.	T-test	Participant	Nonpart.	T-test
Program dosage:									
Mean # weeks participating	10.8			11.8			8.3		
	(11.4)			(10.6)			(5.2)		
Median # weeks participating	8.0			10.0			8.6		
Mean # home visits	3.9			2.8			1.6		
	(3.64)			(2.43)			(1.75)		
Median # home visits	2.0			2.0			1.0		
% with 1 home visit	27.2			39.2			39.1		
% with 2 home visits	24.5			18.8			13.3		
% with 3 home visits	10.3			15.9			14.2		
% with 4 or more home visits	38.1			26.1			10.7		
Family characteristics:									
Income in prior year (in $10,000s)	1.425	1.516		1.552	1.538		1.477	1.950	**
	(0.869)	(1.068)		(0.967)	(1.316)		(0.951)	(1.363)	
Any earnings	0.834	0.844		0.882	0.852		0.827	0.823	
Received SNAP	0.864	0.762	***	0.835	0.677	***	0.822	0.660	**
Received BadgerCare (MA/SCHIP)	0.774	0.706	*	0.724	0.578	***	0.738	0.638	*
Caregiver Age	31.060	31.670		34.390	35.190		34.130	34.050	
	(8.313)	(9.244)		(10.350)	(10.300)		(10.490)	(8.603)	
Age of youngest child	3.321	3.374		6.671	6.781		8.122	7.864	
	(3.790)	(3.799)		(5.280)	(5.298)		(5.463)	(5.659)	
Age of youngest child missing	0.007	0.003		0	0.001		0.089	0.064	
Number of fathers to mother's children	0.980	0.958	+	0.953	0.951		0.942	0.936	
	(0.365)	(0.439)		(0.433)	(0.415)		(0.474)	(0.434)	
Number of fathers missing	0.076	0.111		0.100	0.107		0.138	0.121	
Number of Children	4.156	3.964		3.812	3.522		3.713	3.000	
	(2.855)	(2.789)		(2.638)	(2.532)		(3.841)	(1.796)	
Number of Children missing	0.000	0.000	+	0.000	0.000		0.004	0.007	

White	0.246	0.352	**	0.229	0.374	***	0.209	0.404	***
Black	0.704	0.548	***	0.718	0.519	***	0.733	0.518	***
Hispanic	0.099	0.162	**	0.141	0.179		0.129	0.163	
Native American	0.049	0.041		0.006	0.038	*	0.027	0.036	
Asian	0.013	0.018		0.018	0.026		0.013	0.007	
Race missing	0.000	0.012	+	0.012	0.003		0.000	0.000	
Caregiver US born	0.930	0.885	*	0.924	0.826	**	0.916	0.844	*
Two-parent household	0.193	0.222		0.124	0.233	**	0.151	0.326	***
Single-mother household	0.528	0.498		0.541	0.422	**	0.516	0.390	*
Single-father household	0.043	0.036		0.059	0.084		0.031	0.078	*
Family structure missing	0.173	0.159		0.224	0.190		0.218	0.170	
Number of previous investigations	1.415	1.259		1.700	1.409		1.636	1.234	
	(2.685)	(2.367)		(2.745)	(2.695)		(2.792)	(2.506)	
Ever previous substantiation	0.136	0.097	+	0.159	0.105	+	0.138	0.135	
Days from report to randomization	57.070	58.790		56.010	58.040		59.490	59.060	
	(15.460)	(16.450)		(16.750)	(16.670)		(14.630)	(15.990)	
Days to randomization missing	0.063	0.076		0.041	0.039		0.089	0.099	
Observations	301	659		170	609		224	141	
Percent	31.4	68.6		21.8	78.2		61.5	38.5	

Note. 960 treatment group observations for the Early Cohort; 779 treatment group observations for the Late Cohort; 366 treatment group observations for the Survey Cohort. Measures assessed in quarter prior to randomization unless otherwise noted.
+p<.10; *p<.05; **p<.01; ***p<.001.

Table 4. ITT Effects for Primary Survey Outcomes at 12-Months Post-Randomization

	GAIN observations	Control observations	GAIN mean	Control mean	Raw mean difference (ITT)	Regression-adjusted difference (ITT)
Neglectful behaviors	325	321	0.029	-0.03	0.058	0.092
						(0.081)
Physical aggression behaviors	325	321	0.07	-0.075	0.144	0.184*
						(0.080)
Emotional abuse behaviors	324	321	-0.005	-0.001	-0.005	0.017
						(0.079)
Parenting distress	327	323	0.033	-0.042	0.075	0.020
						(0.081)
Income (self-reported; in $10,000s)	321	314	2.738	2.895	-0.157	0.096
						(0.211)
Economic stress	327	323	0.79	-0.83	0.161*	0.177*
						(0.081)
Material hardship	326	323	0.094	-0.097	0.190*	0.170*
						(0.078)

Note. Models adjust for the full set of covariates presented in Table 2, as well as quarter of randomization fixed effects. All measures except income have been standardized to have a mean of 0 and standard deviation of 1, such that estimates are in standard deviation units.
+p<.10; *p<.05.

Table 5. ITT Effects for Secondary Survey Outcomes at 12-Months Post-Randomization

	GAIN observations	Control observations	GAIN mean	Control mean	Raw mean difference (ITT)	Regression-adjusted difference (ITT)
Lack of parenting warmth/affection	327	323	0.037	-0.042	0.079	0.102
						(0.080)
Lack of parenting attention	327	323	-0.043	0.038	-0.081	-0.043
						(0.079)
Parental impatience	327	322	-0.027	0.012	-0.039	0.003
						(0.081)
Parental leniency	327	323	0.038	-0.033	0.070	0.054
						(0.080)
Lack of parenting routines	327	323	0.015	-0.015	0.030	0.084
						(0.084)
Lack of parental resilience	325	321	-0.035	0.032	-0.067	-0.054
						(0.080)
Lack of social connections	324	320	0.045	-0.042	0.087	0.063
						(0.078)
Lack of concrete support	326	323	-0.061	0.056	-0.117	-0.118
						(0.088)
Lack of social and emotional competence	327	323	-0.064	0.055	0.119	0.094
						(0.081)
Housing hardship	327	323	0.025	-0.039	0.065	0.095
						(0.080)
Food hardship	327	323	0.076	-0.079	0.154*	0.169*
						(0.082)

Note. Models adjust for the full set of covariates presented in Table 2, as well as quarter of randomization fixed effects. All measures have been standardized to have a mean of 0 and standard deviation of 1, such that estimates are in standard deviation units.
+p<.10; *p<.05.

Table 6. ITT Effects for Ancillary Survey Outcomes at 12-Months Post-Randomization

	GAIN observations	Control observations	GAIN mean	Control mean	Raw mean difference (ITT)	Regression-adjusted difference (ITT)
Depressive symptoms	327	323	0.02	-0.016	0.037	0.009
						(0.082)
Lack of self-efficacy	325	318	0.065	-0.065	0.130	0.114
						(0.082)
Toxic social network	327	323	0.087	-0.082	0.169*	0.134+
						(0.080)
Poor adult relationship quality[a]	176	168	-0.057	0.036	-0.093	-0.133
						(0.107)
Recent intimate partner violence	324	321	0.117	0.112	0.005	-0.004
						(0.026)
Debt	326	322	0.368	0.258	0.110**	0.126***
						(0.038)
EITC receipt	299	289	0.612	0.595	0.017	0.051
						(0.040)

Note. Models adjust for the full set of covariates presented in Table 2, as well as quarter of randomization fixed effects. All measures except recent intimate partner violence, debt, and EITC receipt (which are dichotomous) have been standardized to have a mean of 0 and standard deviation of 1, such that estimates are in standard deviation units.

[a]Administered only to caregivers who reported being co-resident with their partner or spouse.

+p<.10; *p<.05.

Figure 1. Resource Inadequacy Model

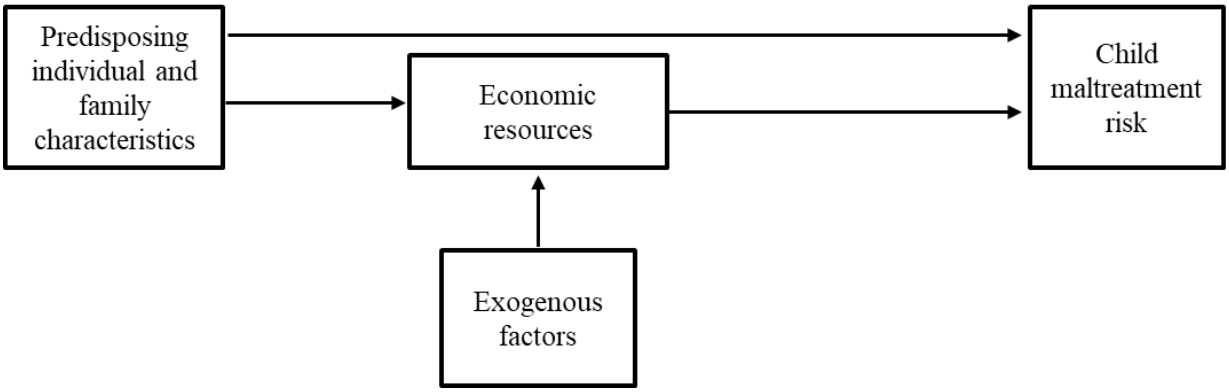


Figure 2. Psychosocial Model

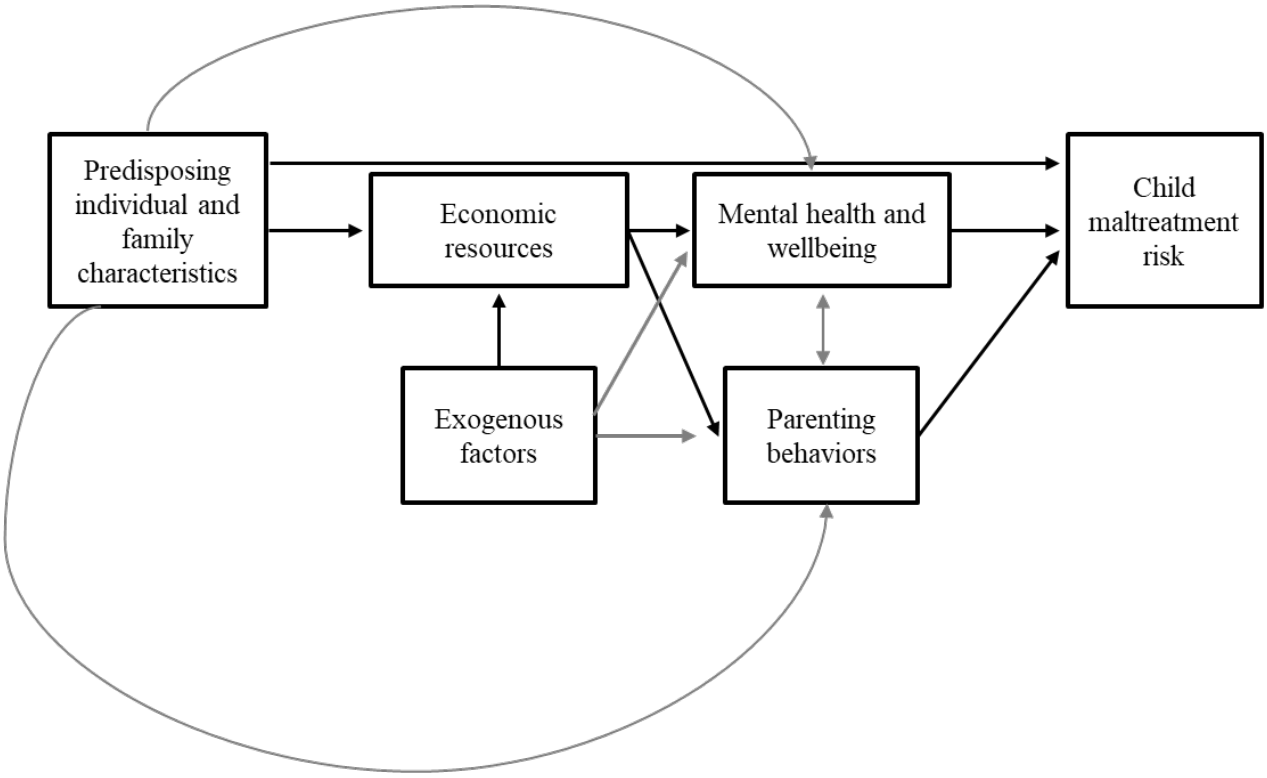
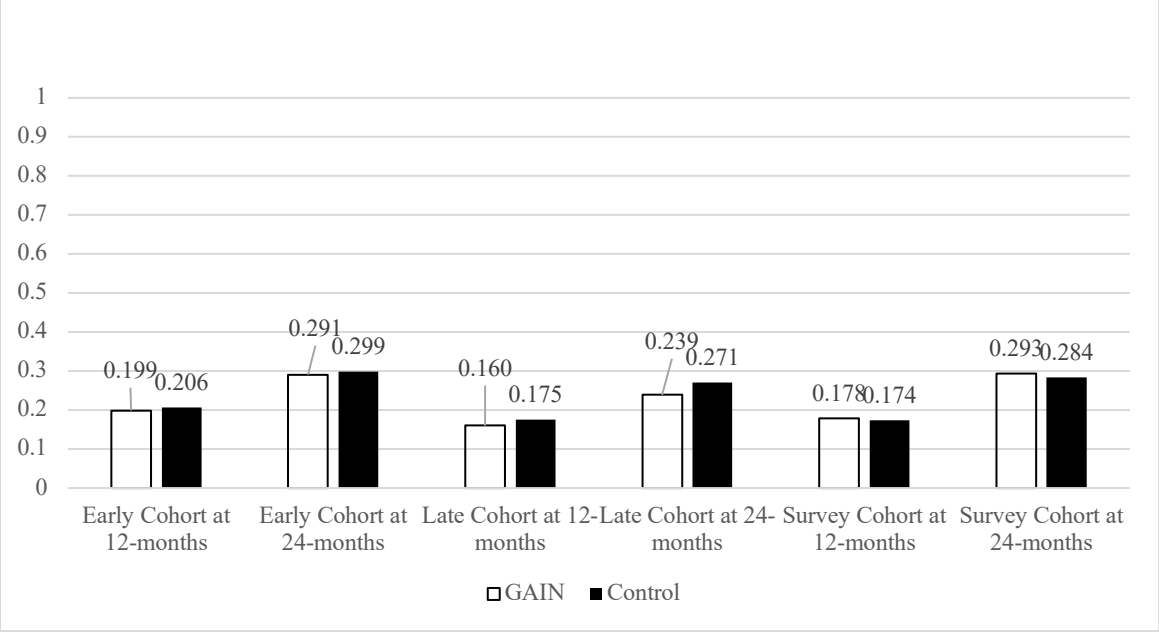
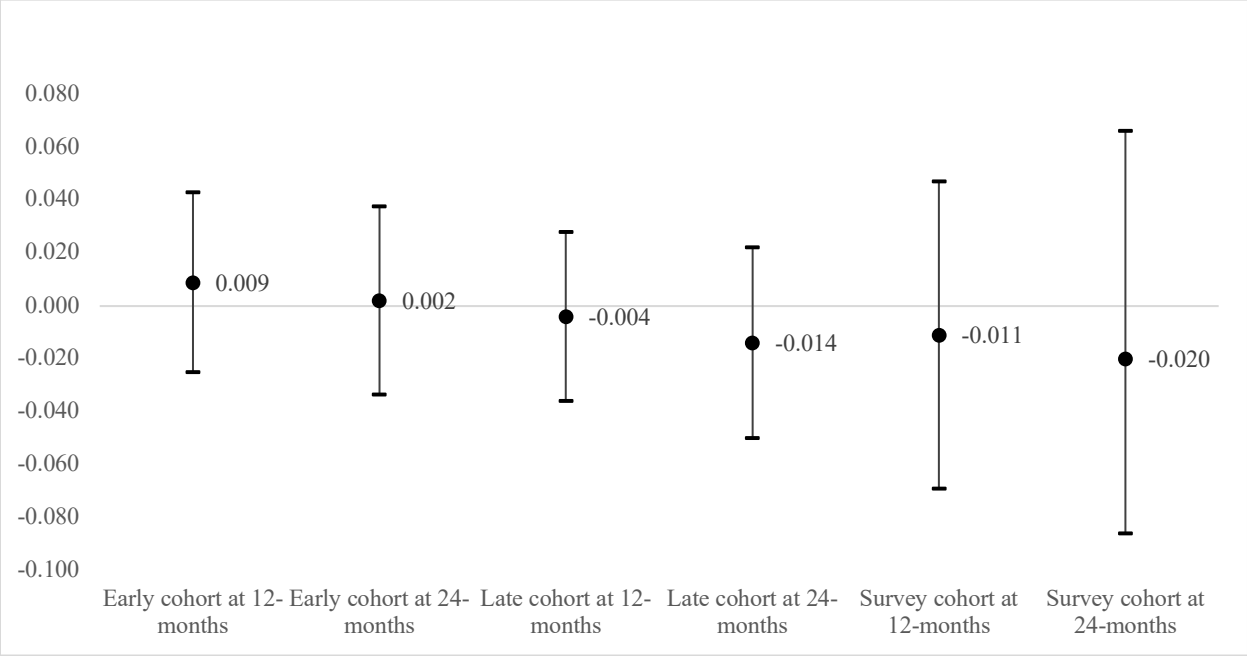


Figure 3. Full Sample Mean Differences for Caregiver CPS Re-Investigation at 12- and 24-Months Post-Randomization, by cohort



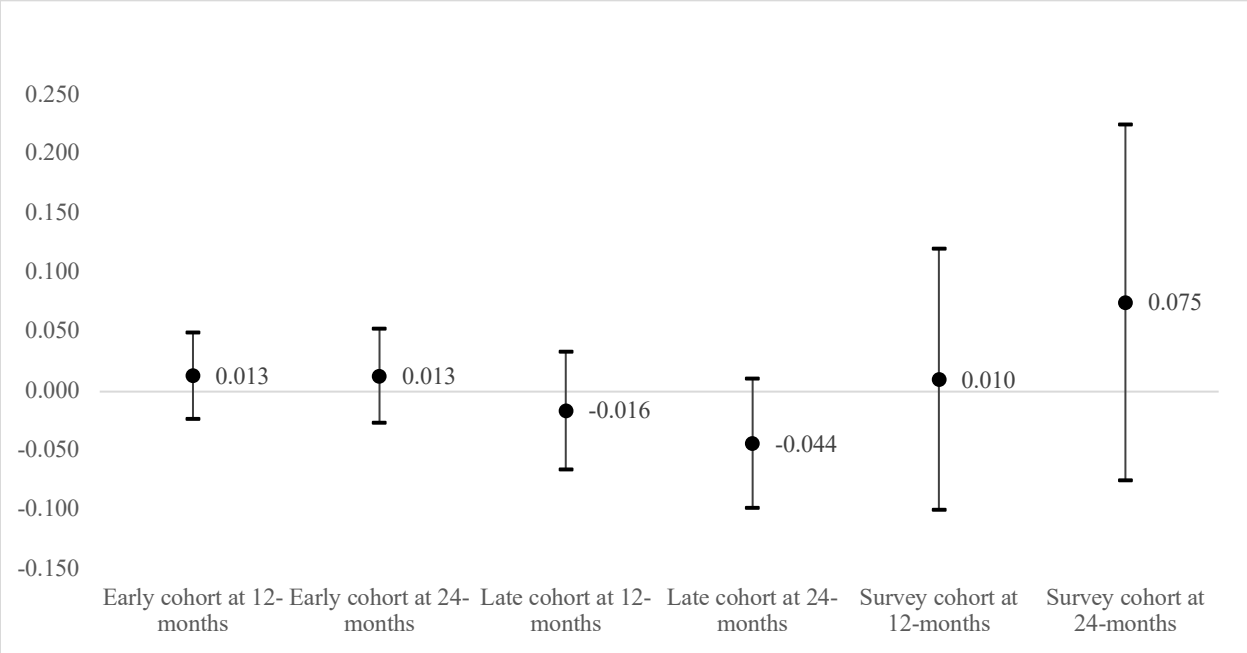
Note. Treatment (GAIN) and control group means.

Figure 4. Full Sample Regression-Adjusted ITT Effects for Caregiver CPS Re-Investigation at 12- and 24-Months Post-Randomization, by cohort



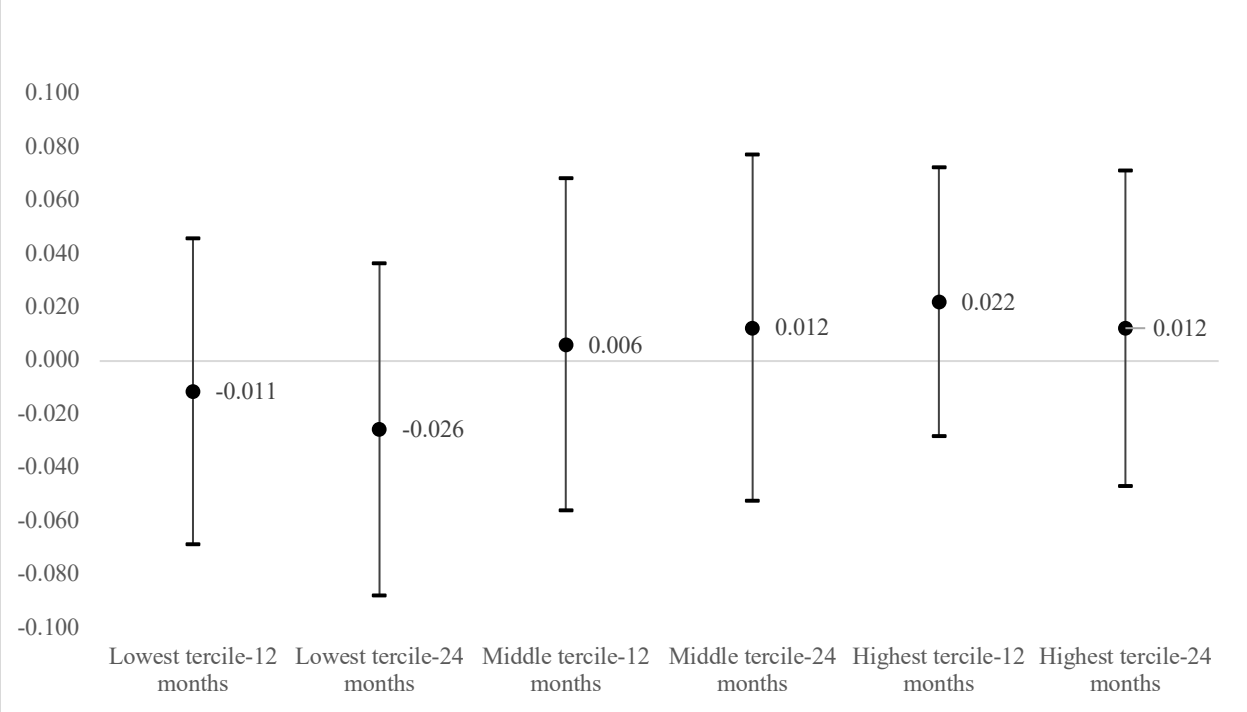
Note. Coefficients and standard errors from OLS regressions for any CPS re-investigation on the primary caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects.

Figure 5. Full Sample Regression-Adjusted ITT Effects for CPS Investigation within 12- and 24-Months of Randomization, Comparisons for Samples Limited to Families with a Child Age 5 or Younger



Note. Coefficients and standard errors from OLS regressions for any CPS re-investigation on the primary caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects.

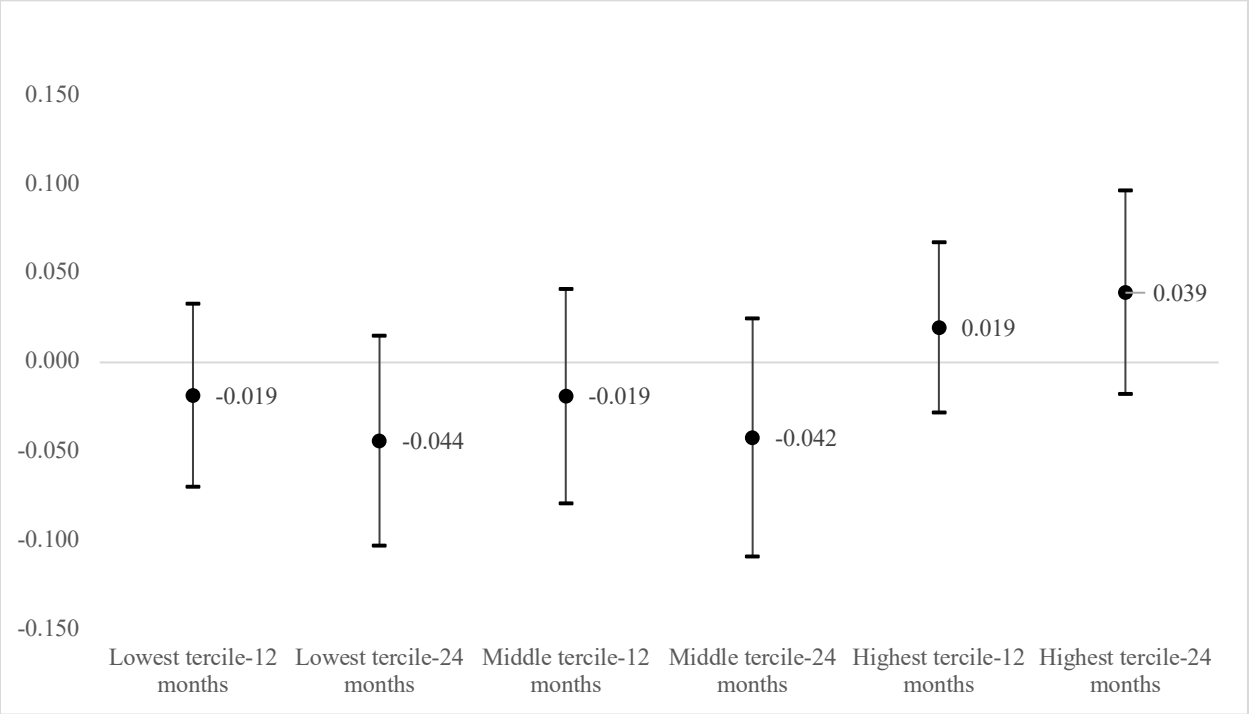
Figure 6. Early Cohort Regression-Adjusted ITT Effects for Caregiver CPS Re-Investigation at 12- and 24-Months Post-Randomization, by Baseline Income Tercile



Note. Coefficients and standard errors from OLS regressions for any CPS re-investigation on the primary caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust

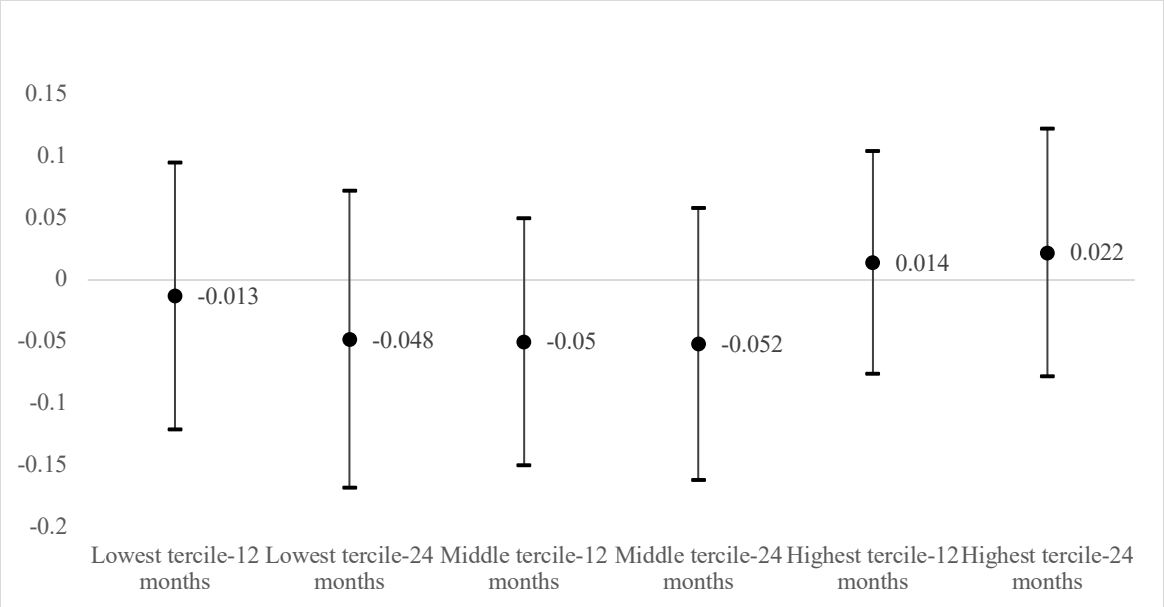
for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. Early Cohort income tercile ranges are $0-$9,818, $9,819-$17,152, and $17,193-$37,002.

Figure 7. Late Cohort Regression-Adjusted ITT Effects for Caregiver CPS Re-Investigation at 12- and 24-Months Post-Randomization, by Baseline Income Tercile



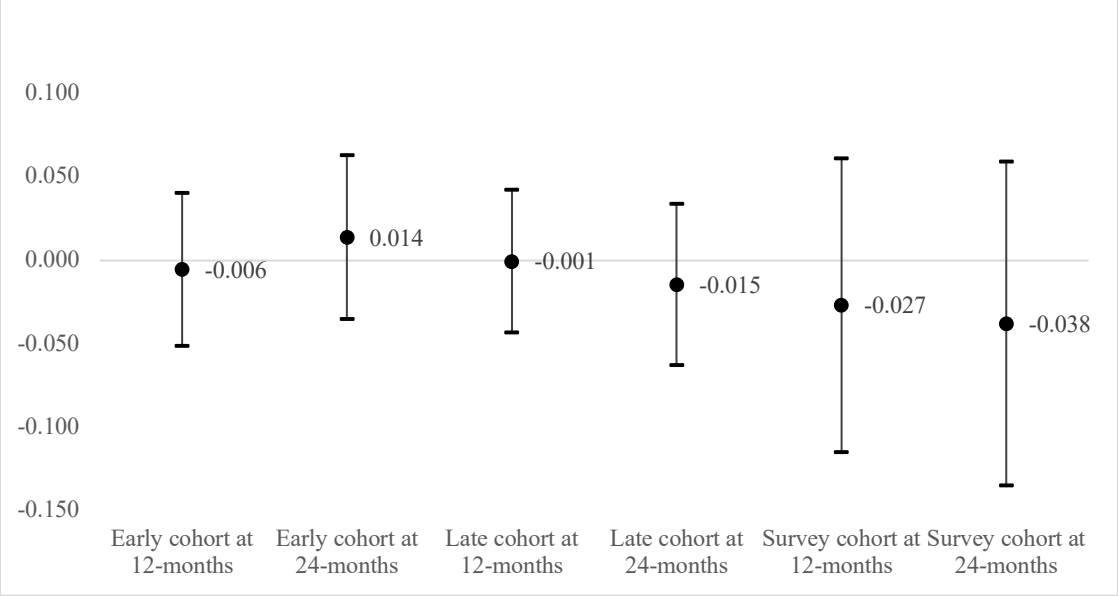
Note. Coefficients and standard errors from OLS regressions for any CPS re-investigation on the primary caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. Late Cohort income tercile ranges are $0-$8,870, $8,875-$17,188, and $17,189-$44,923.

Figure 8. Survey Cohort Regression-Adjusted ITT Effects for Caregiver CPS Re-Investigation at 12- and 24-Months Post-Randomization, by Baseline Income Tercile



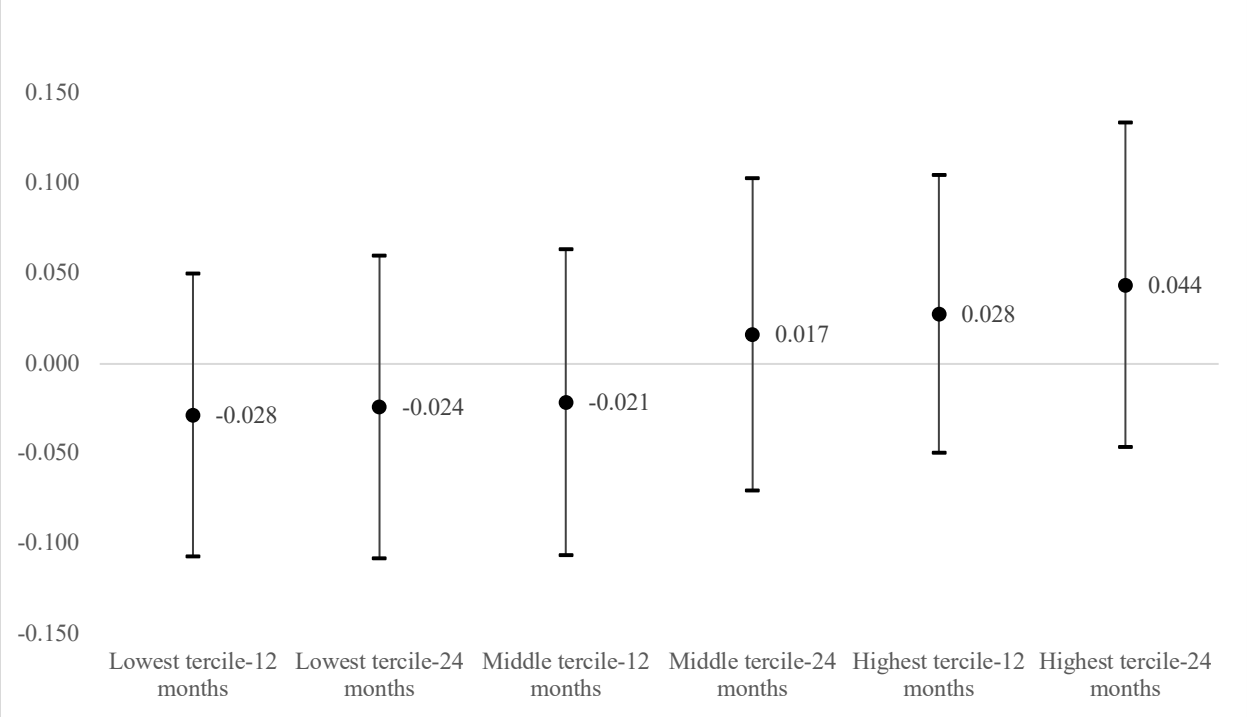
Note. Coefficients and standard errors from OLS regressions for any CPS re-investigation on the primary caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. Survey Cohort income tercile ranges are $0-$9,925, $9,926-18,958, and $18,959-$66,188.

Figure 9. Full Sample Regression-Adjusted ITT Effects for Caregiver CPS Re-Investigation at 12- and 24-Months Post-Randomization Among Families in which the Initial Investigation (at Randomization) Included a Neglect Allegation



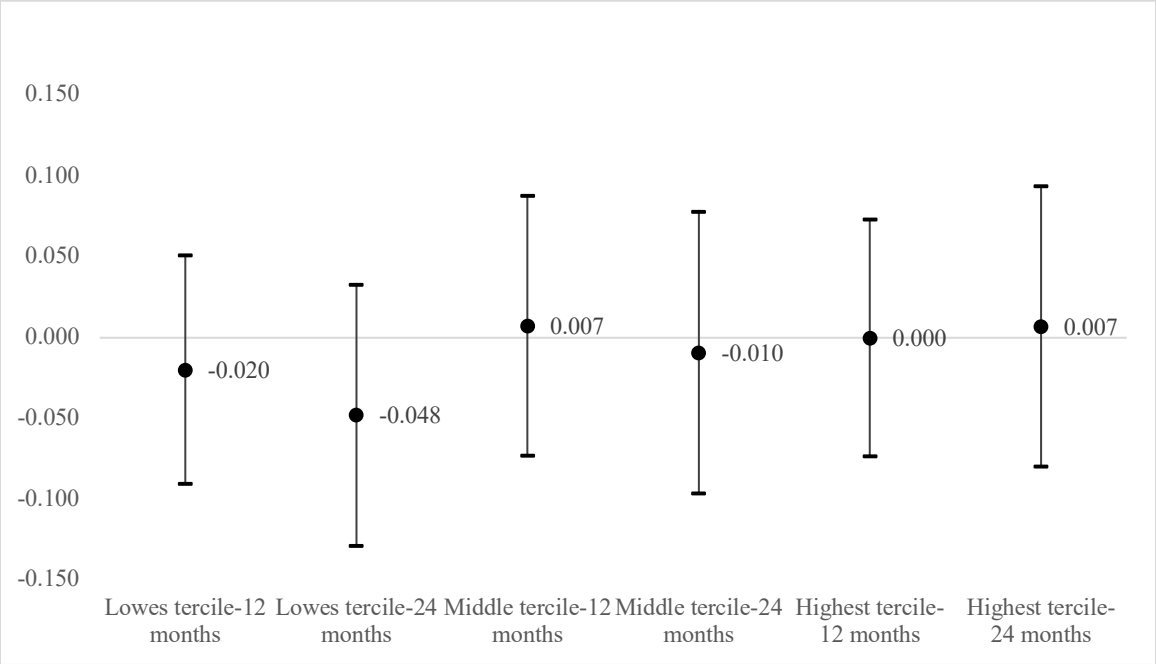
Note. Coefficients and standard errors from OLS regressions for any CPS re-investigation on the primary caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. +p<.10.

Figure 10. Early Cohort Regression-Adjusted ITT Effects for Caregiver CPS Re-Investigation at 12- and 24-Months Post-Randomization Among Families in which the Initial Investigation (at Randomization) Included a Neglect Allegation, by Baseline Income Tercile



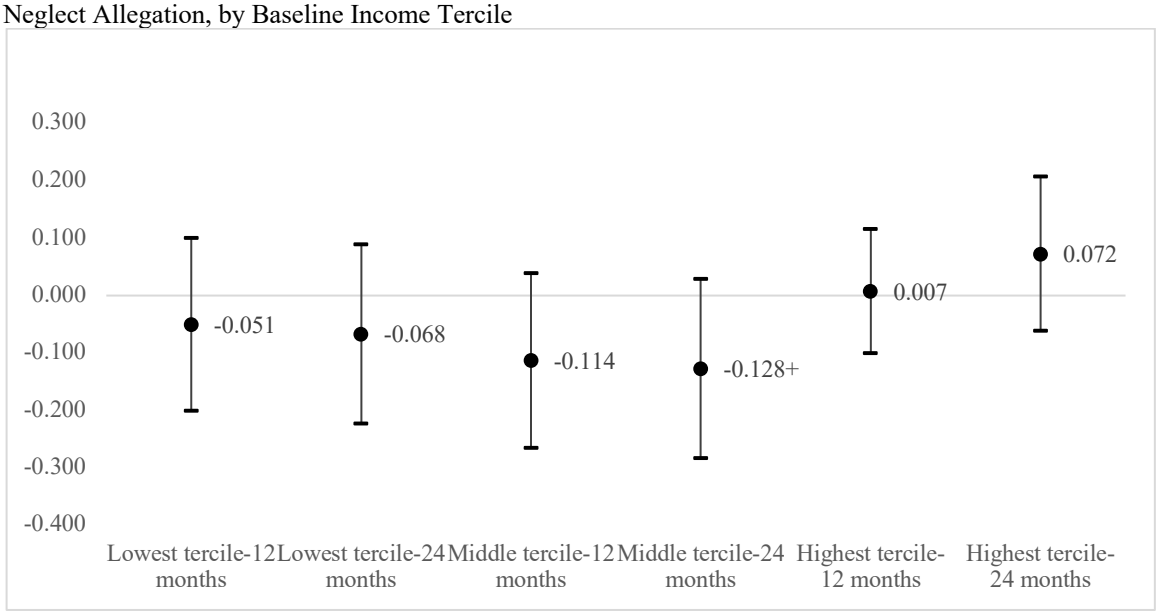
Note. Coefficients and standard errors from OLS regressions for any CPS re-investigation on the primary caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. Early Cohort income tercile ranges are $0-$9,818, $9,819-$17,152, and $17,193-$37,002.

Figure 11. Late Cohort Regression-Adjusted ITT Effects for Caregiver CPS Re-Investigation at 12- and 24-Months Post-Randomization Among Families in which the Initial Investigation (at Randomization) Included a Neglect Allegation, by Baseline Income Tercile



Note. Coefficients and standard errors from OLS regressions for any CPS re-investigation on the primary caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. Late Cohort income tercile ranges are $0-$8,870, $8,875-$17,188, and $17,189-$44,923.

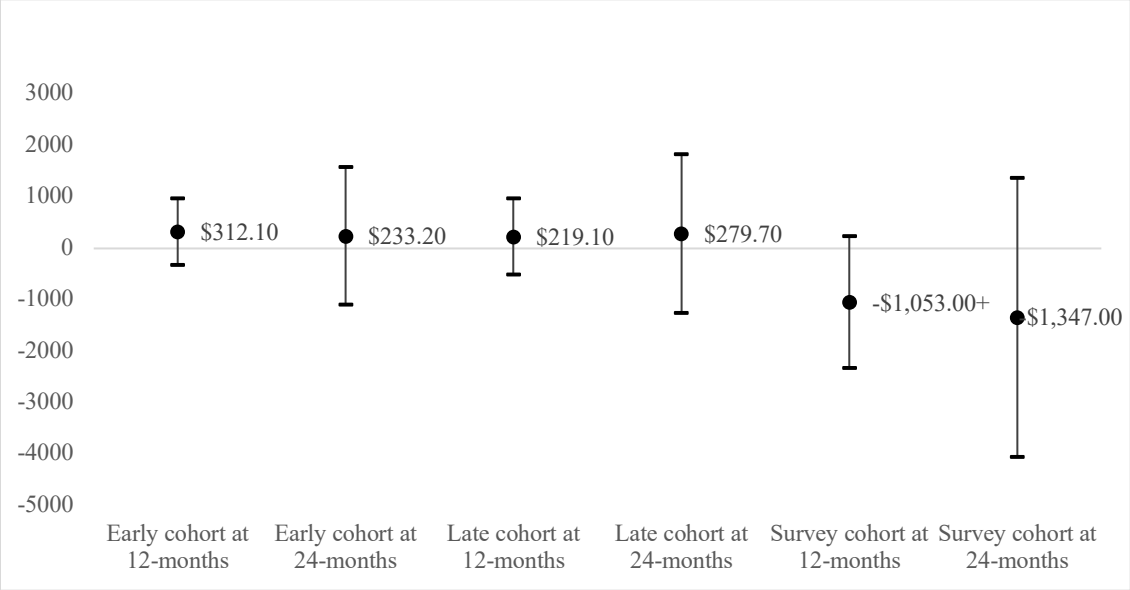
Figure 12. Survey Cohort Regression-Adjusted ITT Effects for Caregiver CPS Re-Investigation at 12- and 24-Months Post-Randomization Among Families in which the Initial Investigation (at Randomization) Included a Neglect Allegation, by Baseline Income Tercile



Note. Coefficients and standard errors from OLS regressions for any CPS re-investigation on the primary caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust

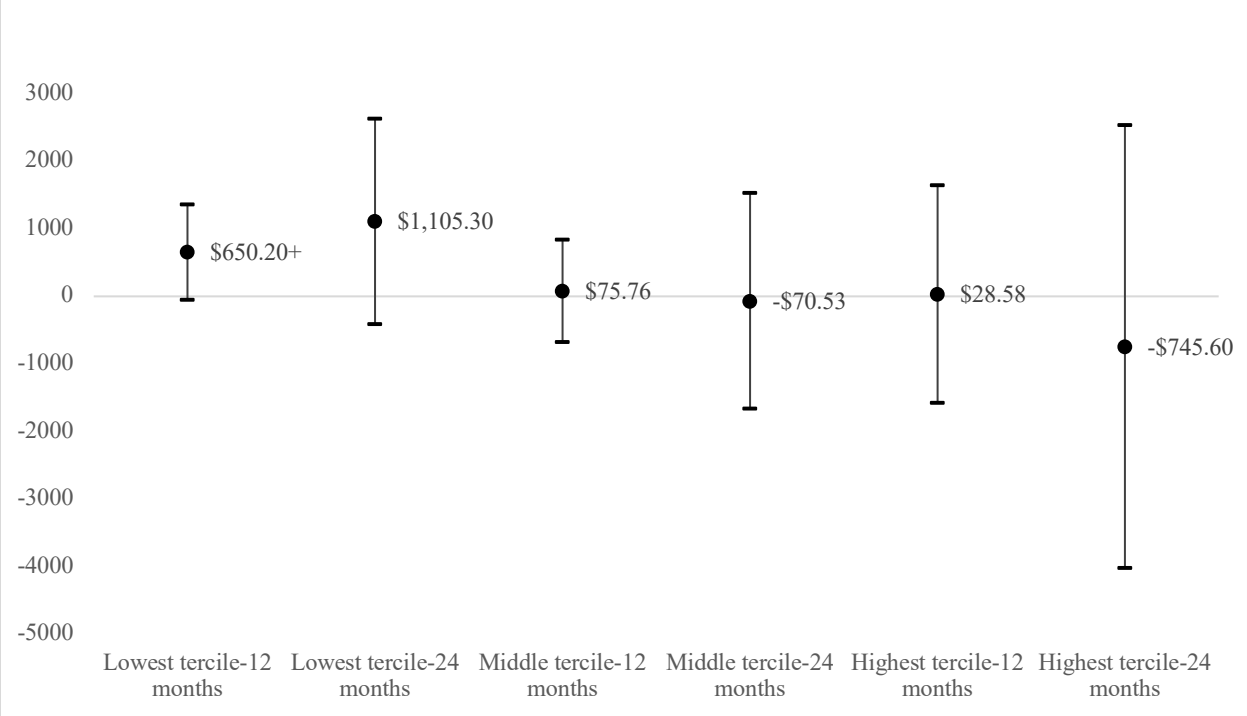
for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. Survey Cohort income tercile ranges are $0-$9,925, $9,926-18,958, and $18,959-$66,188.

Figure 13. Full Sample Regression-Adjusted ITT Effects for Total Income at 12- and 24-Months Post-Randomization



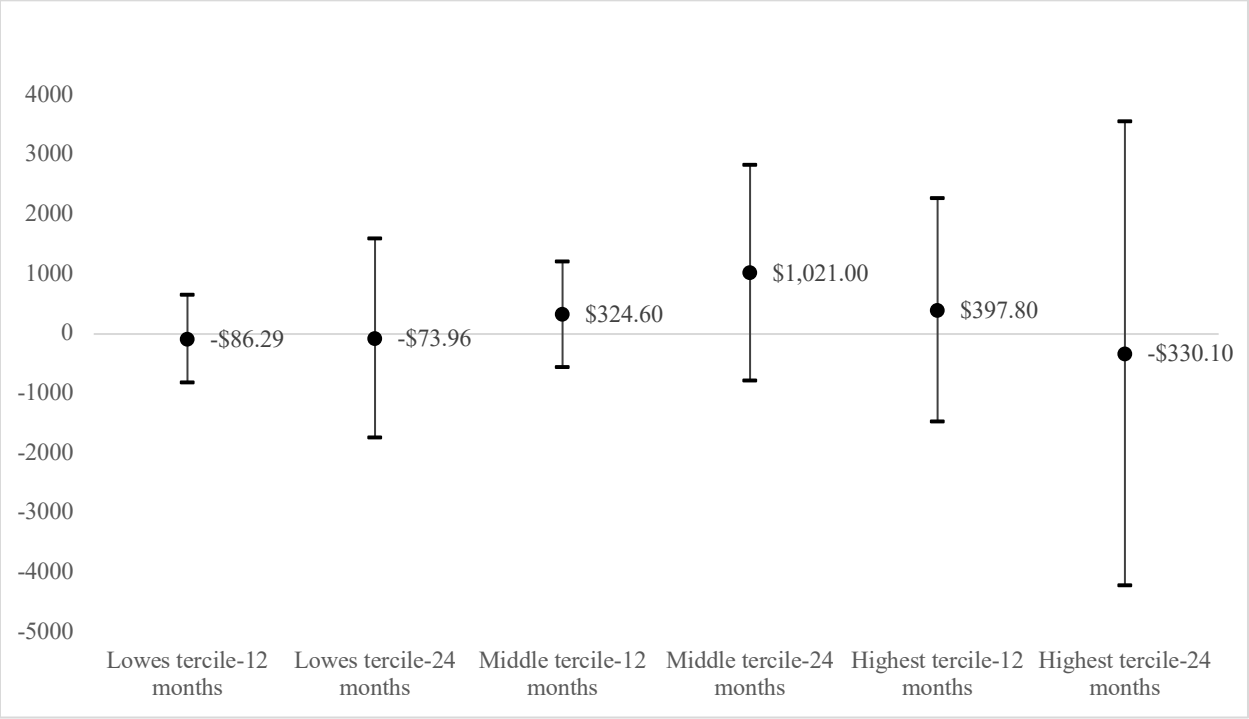
Note. Coefficients and standard errors from OLS regressions for total household income of the caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects.

Figure 14. Early Cohort Regression-Adjusted ITT Effects for Total Income at 12- and 24-Months Post-Randomization, by Baseline Income Tercile



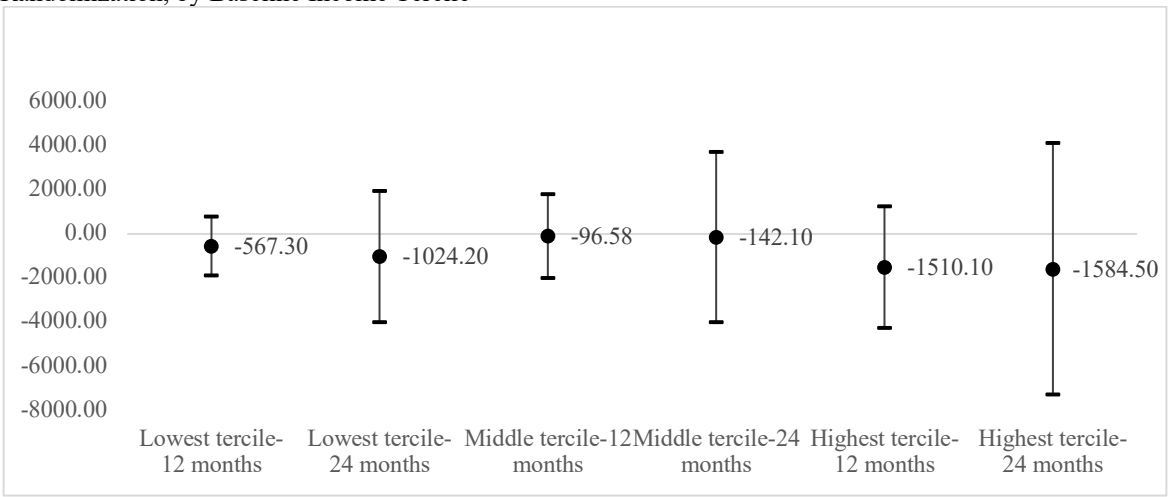
Note. Coefficients and standard errors from OLS regressions for total household income of the caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. Early Cohort income tercile ranges are $0-$9,818, $9,819-$17,152, and $17,193-$37,002.

Figure 15. Late Cohort Regression-Adjusted ITT Effects for Total Income at 12- and 24-Months Post-Randomization, by Baseline Income Tercile



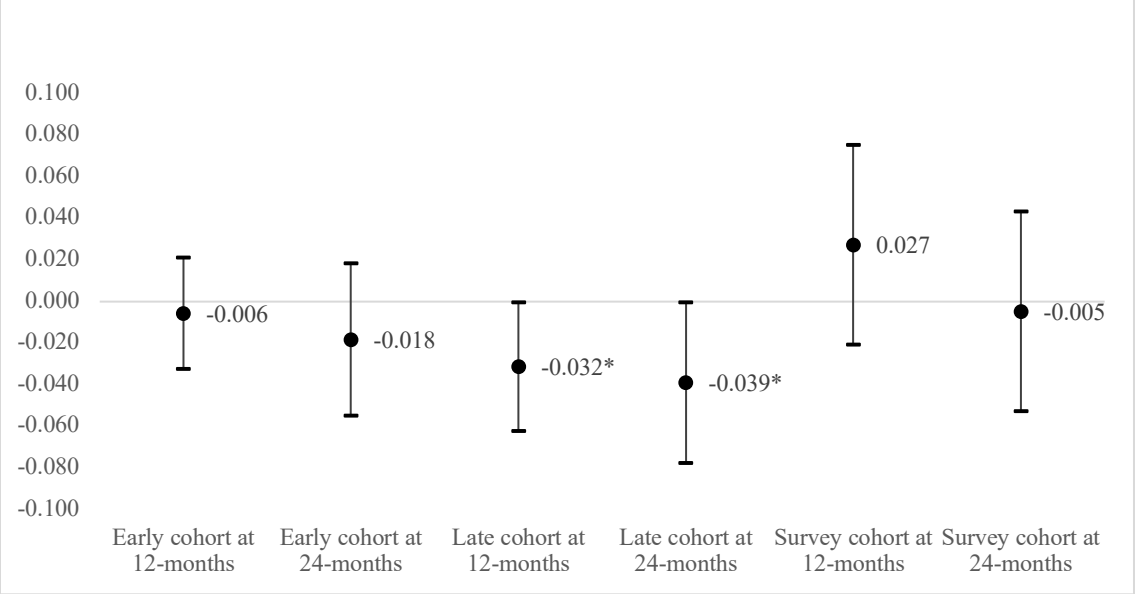
Note. Coefficients and standard errors from OLS regressions for total household income of the caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. Late Cohort income tercile ranges are $0-$8,870, $8,875-$17,188, and $17,189-$44,923.

Figure 16. Survey Cohort Regression-Adjusted ITT Effects for Total Income at 12- and 24-Months Post-Randomization, by Baseline Income Tercile



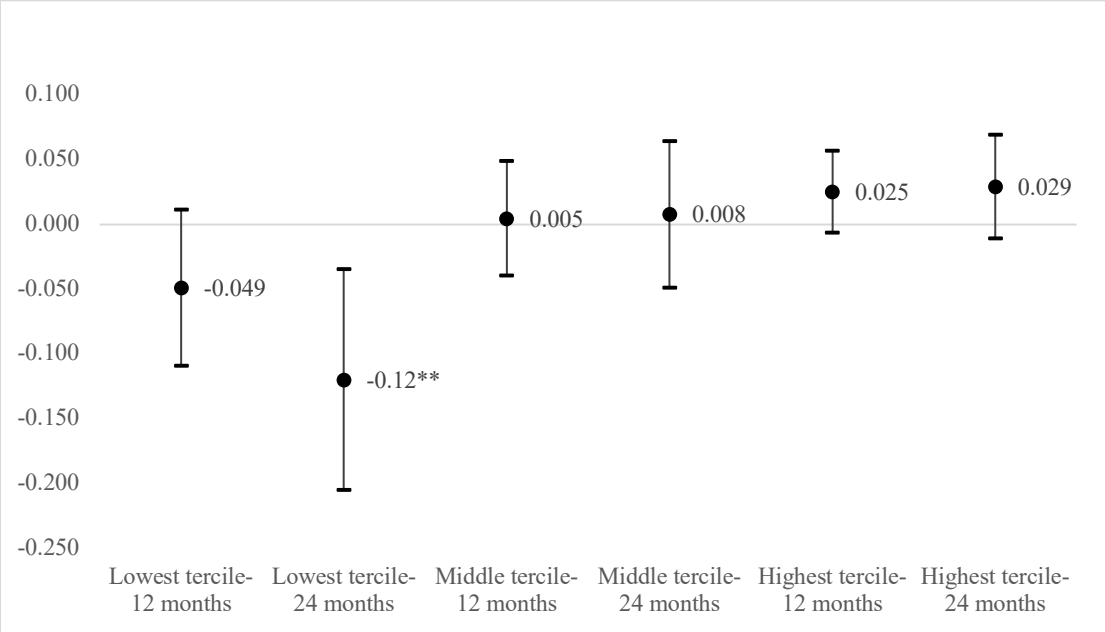
Note. Coefficients and standard errors from OLS regressions for any CPS re-investigation on the primary caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. Survey Cohort income tercile ranges are $0-$9,925, $9,926-18,958, and $18,959-$66,188.

Figure 17. Full Sample Regression-Adjusted ITT Effects for Income Instability at 12- and 24-Months Post-Randomization



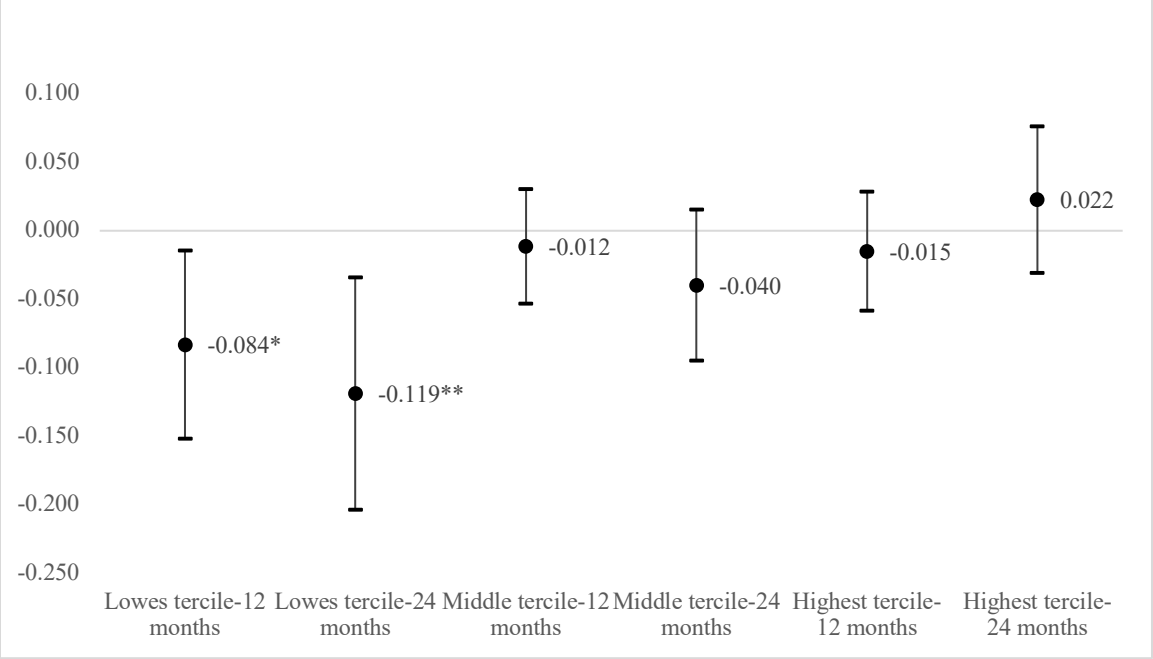
Note. Coefficients and standard errors from OLS regressions for income instability (coefficient of variation) of the caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects.

Figure 18. Early Cohort Regression-Adjusted ITT Effects for Income Instability at 12- and 24-Months Post-Randomization, by Baseline Income Tercile



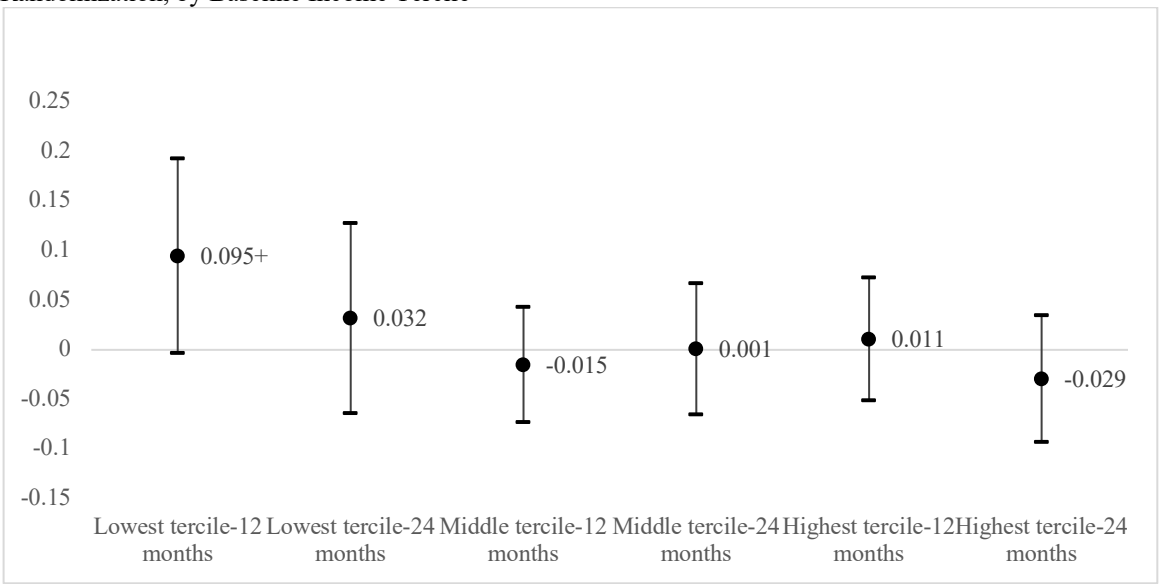
Note. Coefficients and standard errors from OLS regressions for income instability (coefficient of variation) of the caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. Early Cohort income tercile ranges are $0-$9,818, $9,819-$17,152, and $17,193-$37,002.

Figure 19. Late Cohort Regression-Adjusted ITT Effects for Income Instability at 12- and 24-Months Post-Randomization, by Baseline Income Tercile



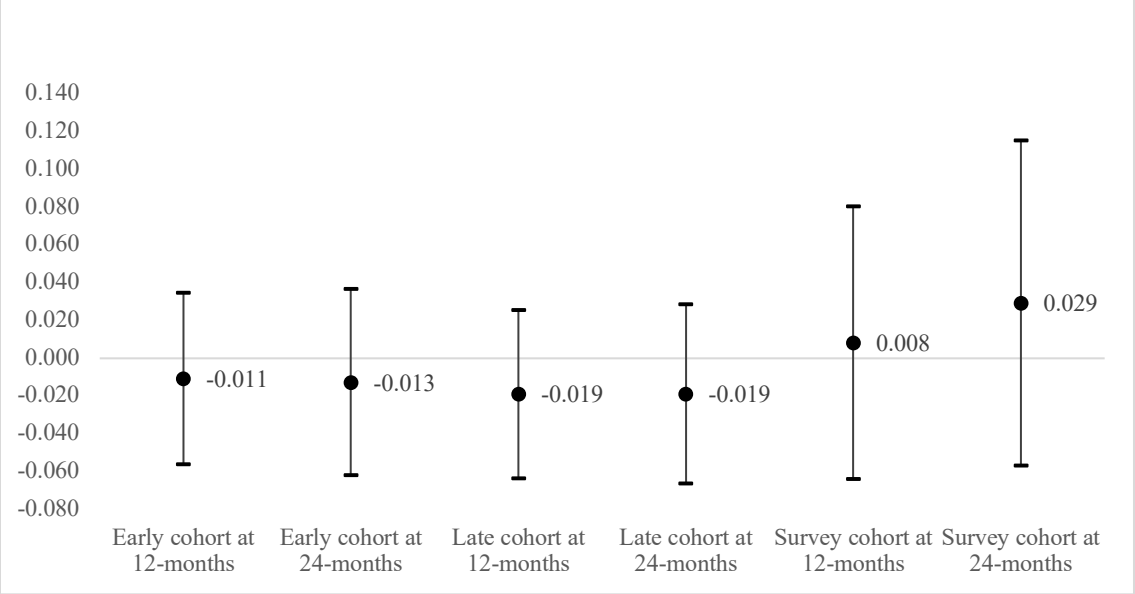
Note. Coefficients and standard errors from OLS regressions for income instability (coefficient of variation) of the caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. Late Cohort income tercile ranges are $0-$8,870, $8,875-$17,188, and $17,189-$44,923.

Figure 20. Late Cohort Regression-Adjusted ITT Effects for Income Instability at 12- and 24-Months Post-Randomization, by Baseline Income Tercile



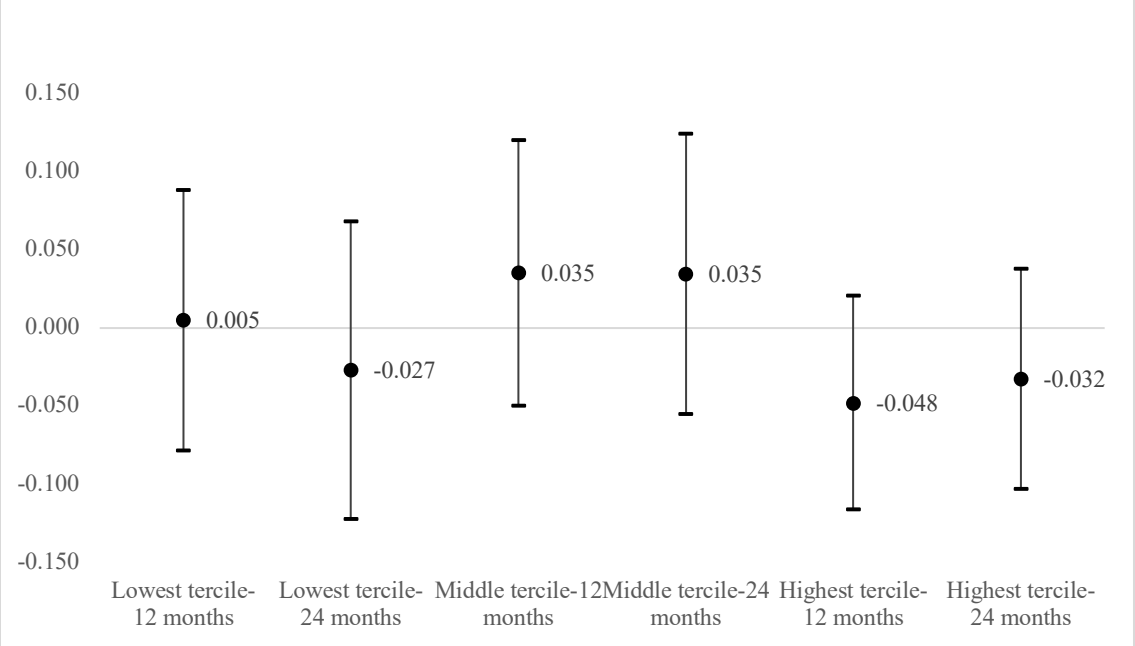
Note. Coefficients and standard errors from OLS regressions for any CPS re-investigation on the primary caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. Survey Cohort income tercile ranges are $0-$9,925, $9,926-18,958, and $18,959-$66,188.

Figure 21. Full Sample Regression-Adjusted ITT Effects for Earnings Instability at 12- and 24-Months Post-Randomization



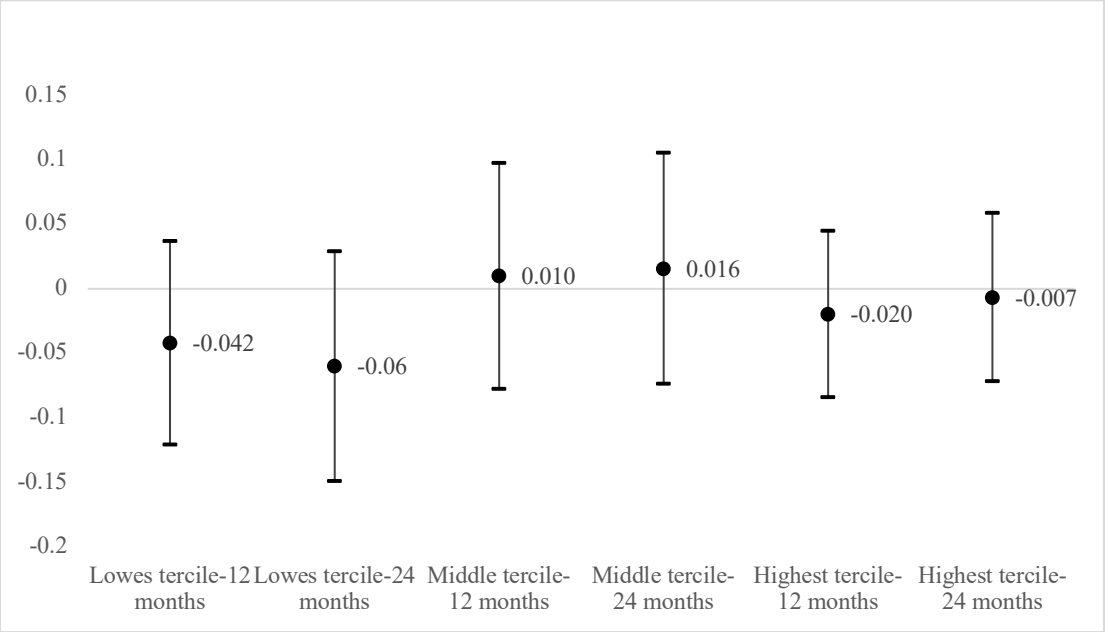
Note. Coefficients and standard errors from OLS regressions for earnings instability (coefficient of variation) of the caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects.

Figure 22. Early Cohort Regression-Adjusted ITT Effects for Earnings Instability at 12- and 24-Months Post-Randomization, by Baseline Income Tercile



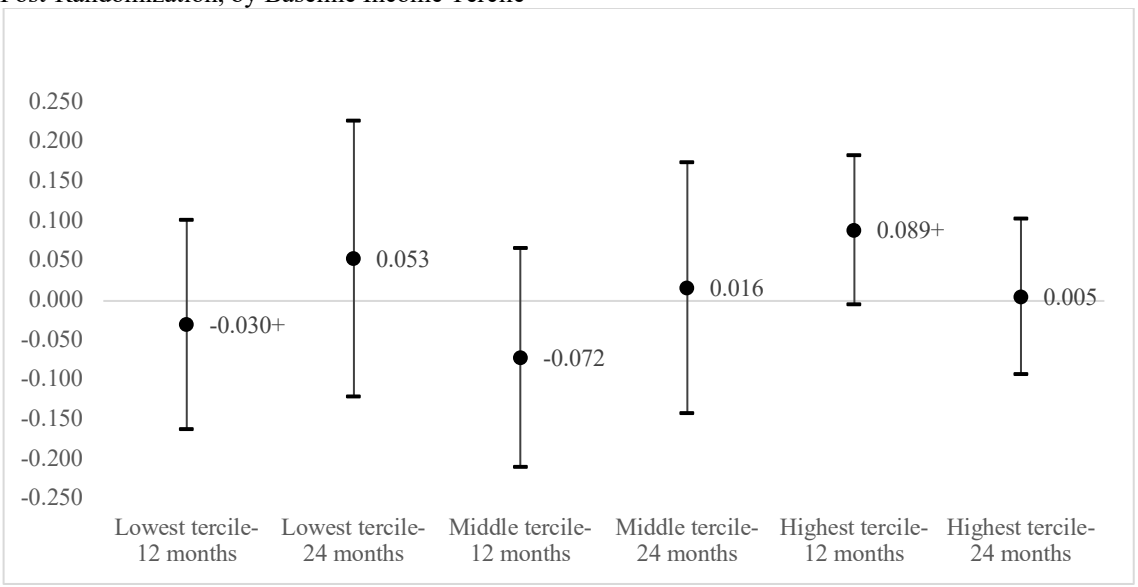
Note. Coefficients and standard errors from OLS regressions for earnings instability (coefficient of variation) of the caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. Early Cohort income tercile ranges are $0-$9,818, $9,819-$17,152, and $17,193-$37,002.

Figure 23. Late Cohort Regression-Adjusted ITT Effects for Earnings Instability at 12- and 24-Months Post-Randomization, by Baseline Income Tercile



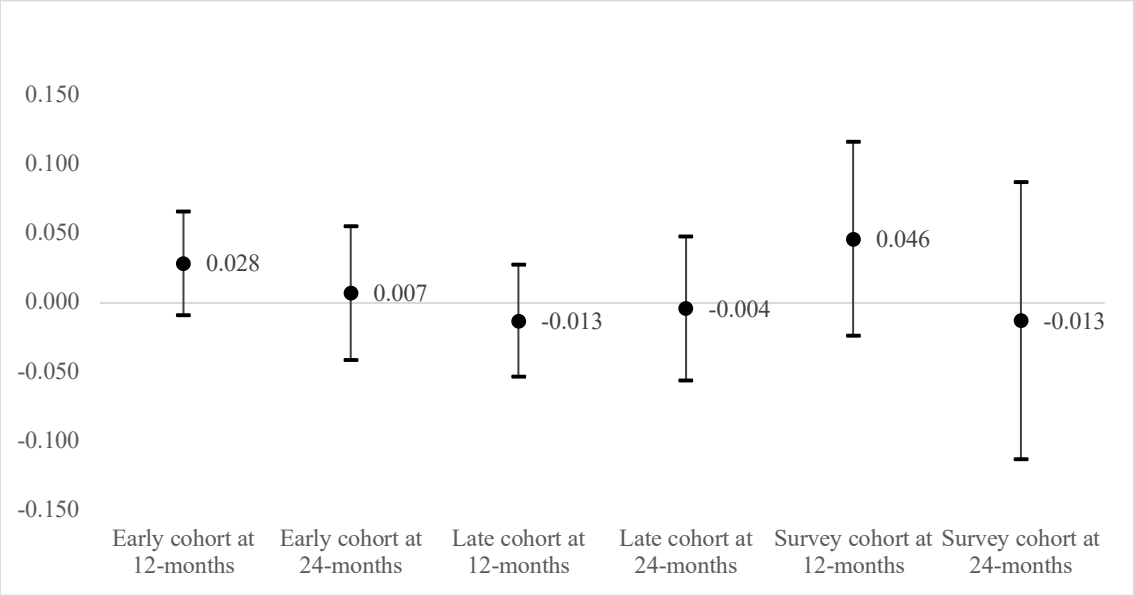
Note. Coefficients and standard errors from OLS regressions for earnings instability (coefficient of variation) of the caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. Late Cohort income tercile ranges are $0-$8,870, $8,875-$17,188, and $17,189-$44,923.

Figure 24. Survey Cohort Regression-Adjusted ITT Effects for Earnings Instability at 12- and 24-Months Post-Randomization, by Baseline Income Tercile



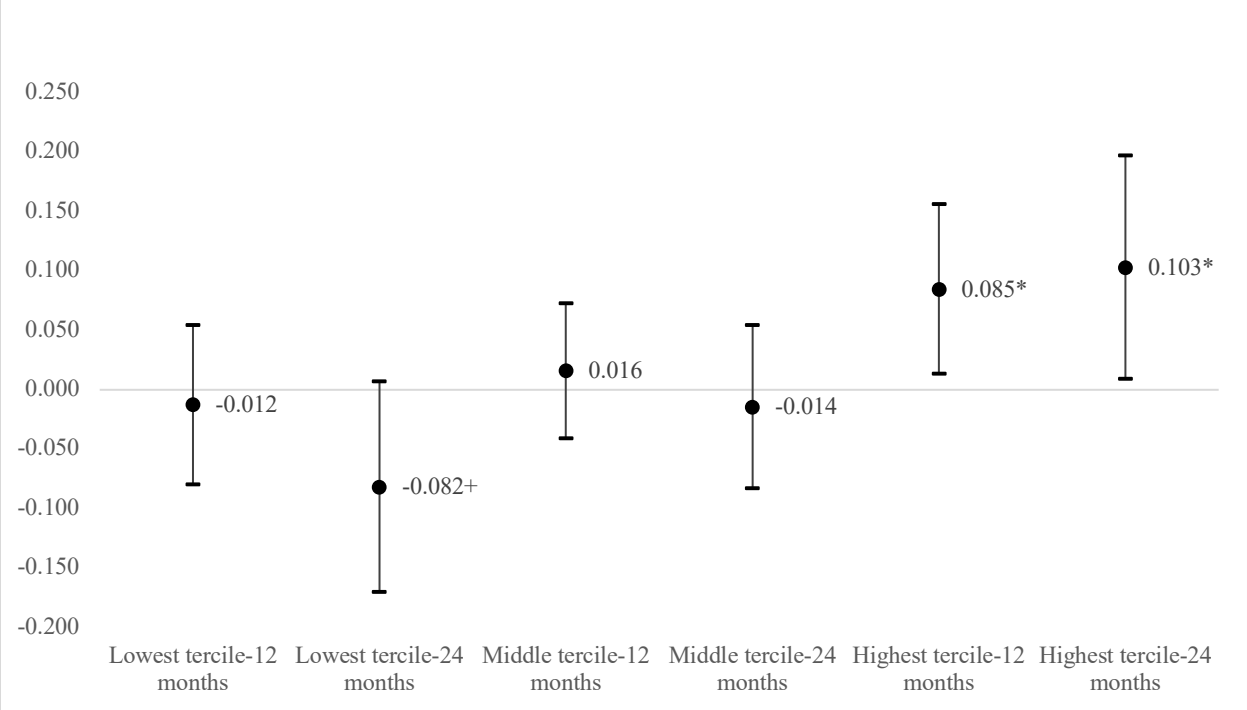
Note. Coefficients and standard errors from OLS regressions for any CPS re-investigation on the primary caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. Survey Cohort income tercile ranges are $0-$9,925, $9,926-18,958, and $18,959-$66,188.

Figure 25. Full Sample Regression-Adjusted ITT Effects for Benefit Instability at 12- and 24-Months Post-Randomization



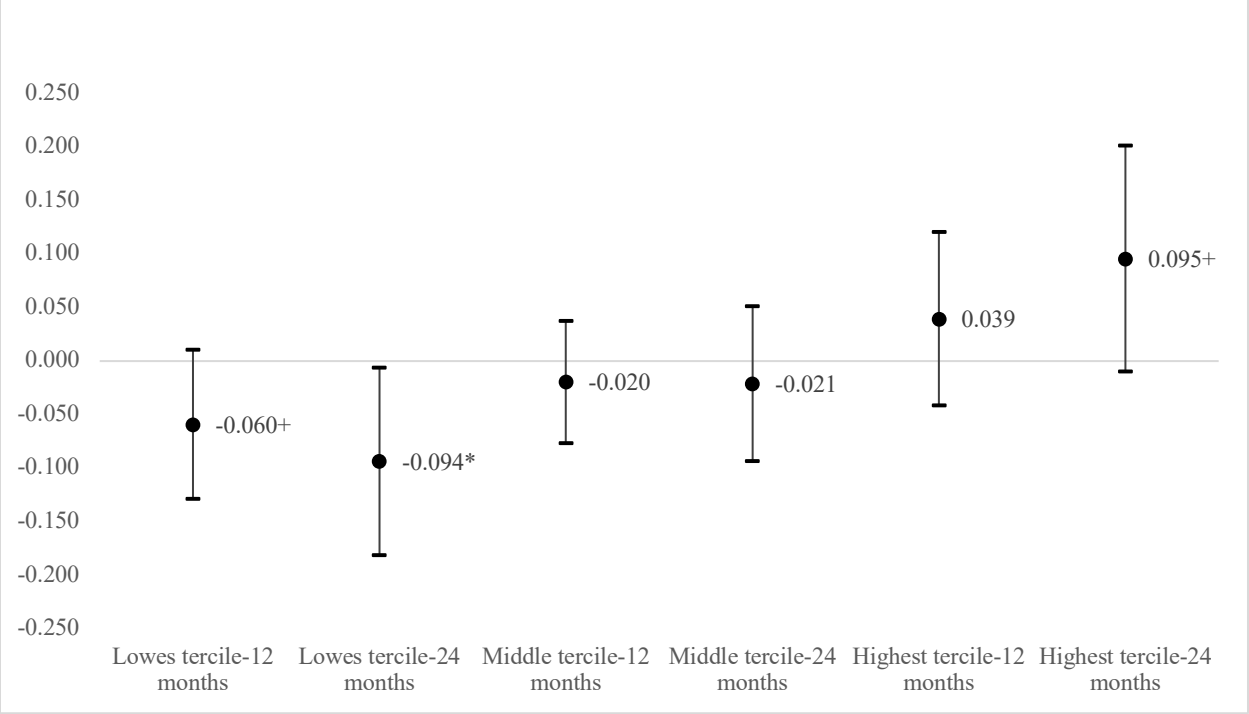
Note. Coefficients and standard errors from OLS regressions for benefit instability (coefficient of variation) of the caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects.

Figure 26. Early Cohort Regression-Adjusted ITT Effects for Benefit Instability at 12- and 24-Months Post-Randomization, by Baseline Income Tercile



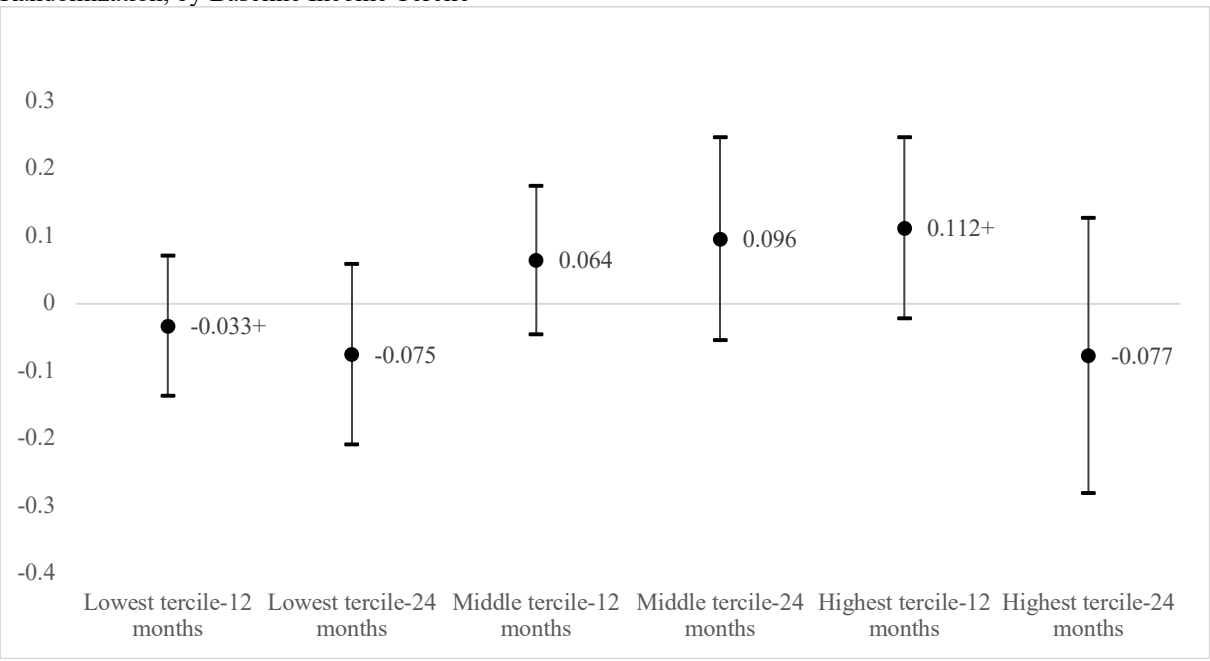
Note. Coefficients and standard errors from OLS regressions for benefit instability (coefficient of variation) of the caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. Early Cohort income tercile ranges are $0-$9,818, $9,819-$17,152, and $17,193-$37,002.

Figure 27. Late Cohort Regression-Adjusted ITT Effects for Benefit Instability at 12- and 24-Months Post-Randomization, by Baseline Income Tercile



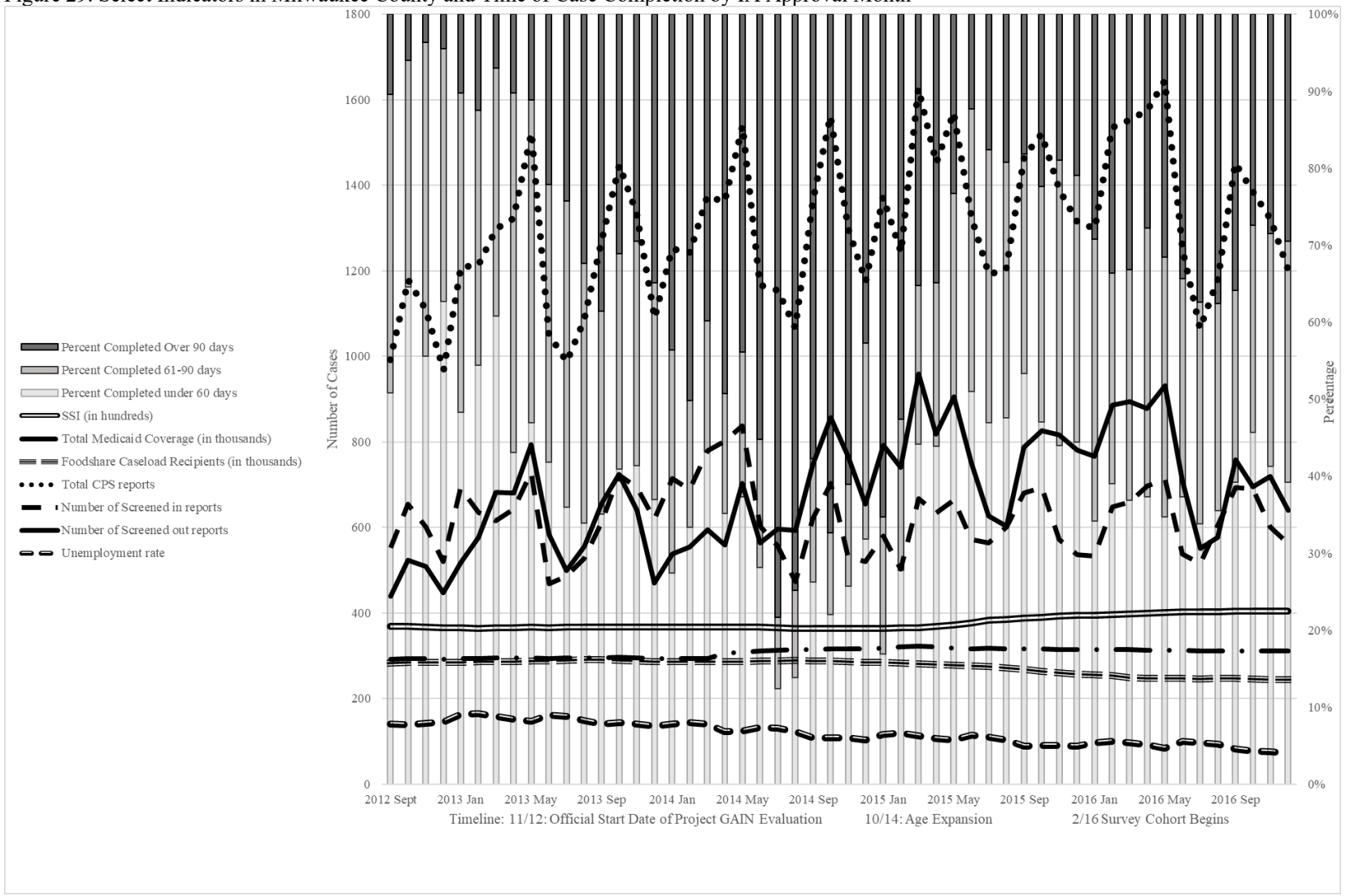
Note. Coefficients and standard errors from OLS regressions for benefit instability (coefficient of variation) of the caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. Late Cohort income tercile ranges are $0-$8,870, $8,875-$17,188, and $17,189-$44,923.

Figure 28. Late Cohort Regression-Adjusted ITT Effects for Benefit Instability at 12- and 24-Months Post-Randomization, by Baseline Income Tercile



Note. Coefficients and standard errors from OLS regressions for any CPS re-investigation on the primary caregiver identified in the baseline CPS investigation with 12 and 24 months of randomization. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. Survey Cohort income tercile ranges are $0-$9,925, $9,926-18,958, and $18,959-$66,188.

Figure 29. Select Indicators in Milwaukee County and Time of Case Completion by IA Approval Month



Appendix Table A1. ITT Effects for Caregiver CPS Re-Investigation at 12- and 24-Months Post-Randomization

	GAIN observations	Control observations	GAIN mean	Control mean	Raw mean difference (ITT)[a]	Regression-adjusted difference (ITT)[a]	Effect size (Adjusted ITT/control mean)[b]
Panel A: Full Sample							
Early Cohort							
12 months	960	1473	0.199	0.206	-0.007	0.009	0.044
						(0.017)	
24 months	960	1473	0.291	0.299	-0.008	0.002	0.007
						(0.018)	
Late Cohort							
12 months	779	2120	0.160	0.175	-0.015	-0.004	-0.023
						(0.016)	
24 months	779	2120	0.239	0.271	-0.032	-0.014	-0.052
						(0.018)	
Survey Cohort							
12 months	365	356	0.178	0.174	0.004	-0.011	-0.063
						(0.029)	
24 months	365	356	0.293	0.284	0.009	-0.020	-0.070
						(0.033)	
Panel B: By income tercile							
Early Cohort							
12 months, bottom tercile	316	495	0.193	0.202	-0.009	-0.011	-0.054
						(0.029)	

24 months, bottom tercile	316	495	0.275	0.285	-0.010	-0.026	-0.091
						(0.032)	
12 months, middle tercile	319	492	0.248	0.270	-0.022	0.006	0.022
						(0.032)	
24 months, middle tercile	319	492	0.361	0.372	-0.011	0.012	0.032
						(0.033)	
12 months, top tercile	325	486	0.157	0.146	0.011	0.022	0.151
						(0.026)	
24 months, top tercile	325	486	0.237	0.241	-0.004	0.012	0.050
						(0.030)	
Late Cohort							
12 months, bottom tercile	273	694	0.147	0.173	-0.026	-0.019	-0.110
						(0.026)	
24 months, bottom tercile	273	694	0.209	0.265	-0.056	-0.044	-0.166
						(0.030)	
12 months, middle tercile	246	720	0.203	0.226	-0.023	-0.019	0.084
						(0.031)	
24 months, middle tercile	246	720	0.301	0.35	-0.049	-0.042	-0.120
						(0.034)	
12 months, top tercile	260	706	0.135	0.123	0.012	0.019	0.154
						(0.024)	
24 months, top tercile	260	706	0.219	0.194	0.025	0.039	0.201
						(0.029)	
Survey Cohort							
12 months, bottom tercile	115	125	0.200	0.184	0.016	-0.013	-0.071
						(0.054)	

24 months, bottom tercile	115	125	0.330	0.350	-0.022	-0.048	-0.137
						(0.060)	
12 months, middle tercile	125	116	0.184	0.207	-0.023	-0.050	-0.242
						(0.050)	
24 months, middle tercile	126	116	0.304	0.302	0.002	-0.052	-0.172
						(0.055)	
12 months, top tercile	125	115	0.152	0.130	0.022	0.014	0.108
						(0.045)	
24 months, top tercile	125	115	0.248	0.191	0.057	0.022	0.115
						(0.050)	

Note. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. Early Cohort income tercile ranges are $0-$9,818, $9,819-$17,152, and $17,193-$37,002. Late Cohort income tercile ranges are $0-$8,870, $8,875-$17,188, and $17,189-$44,923. Survey Cohort income tercile ranges are $0-$9,925, $9,926-18,958, and $18,959-$66,188.

[a]Percentage-point difference between GAIN and control groups.

[b]Percent difference between GAIN and control groups.

Appendix Table A2. ITT Effects for Caregiver CPS Re-Investigation at 12- and 24-Months Post-Randomization Among Families in which the Initial Investigation (at Randomization) Included a Neglect Allegation

	GAIN observations	Control observations	GAIN mean	Control mean	Raw mean difference (ITT)[a]	Regression-adjusted difference (ITT)[a]	Effect size (Adjusted ITT/control mean)[b]
Panel A: Full Sample							
Early Cohort							
12 months	553	839	0.230	0.253	-0.023	-0.006	-0.024
						(0.023)	
24 months	553	839	0.338	0.341	-0.003	0.014	0.041
						(0.025)	
Late Cohort							
12 months	447	1265	0.188	0.202	-0.014	-0.001	-0.005
						(0.022)	
24 months	447	1265	0.282	0.310	-0.028	-0.015	-0.048
						(0.025)	
Survey Cohort							
12 months	224	182	0.196	0.209	-0.012	-0.027	-0.129
						(0.043)	
24 months	224	182	0.330	0.346	-0.016	-0.038	-0.110
						(0.047)	
Panel B: By income tercile							
Early Cohort							
12 months, bottom tercile	201	305	0.209	0.243	-0.034	-0.028	-0.115
						(0.040)	

24 months, bottom tercile	201	305	0.303	0.328	-0.024	-0.024	-0.073
						(0.043)	
12 months, middle tercile	188	297	0.287	0.333	-0.046	-0.021	-0.063
						(0.043)	
24 months, middle tercile	188	297	0.410	0.418	-0.008	0.017	0.040
						(0.045)	
12 months, top tercile	164	237	0.189	0.165	0.025	0.028	0.170
						(0.039)	
24 months, top tercile	164	237	0.299	0.262	0.037	0.044	0.168
						(0.046)	
Late Cohort							
12 months, bottom tercile	169	440	0.166	0.205	-0.040	-0.020	-0.098
						(0.036)	
24 months, bottom tercile	169	440	0.243	0.314	-0.071	-0.048	-0.153
						(0.041)	
12 months, middle tercile	149	463	0.242	0.240	0.002	0.007	0.029
						(0.041)	
24 months, middle tercile	149	463	0.362	0.367	-0.005	-0.010	0.027
						(0.044)	
12 months, top tercile	129	362	0.155	0.149	0.006	-0.0004	-0.003
						(0.037)	
24 months, top tercile	129	362	0.240	0.232	-0.008	0.007	0.030
						(0.044)	
Survey Cohort							
12 months, bottom tercile	72	76	0.208	0.224	-0.015	-0.051	-0.228
						(0.075)	

24 months, bottom tercile	72	76	0.375	0.421	-0.046	-0.068	-0.162
						(0.078)	
12 months, middle tercile	85	54	0.224	0.259	-0.036	-0.114	-0.440
						(0.076)	
24 months, middle tercile	85	54	0.353	0.389	-0.036	-0.128	-0.329
						(0.078)	
12 months, top tercile	67	52	0.149	0.135	0.015	0.007	0.052
						(0.054)	
24 months, top tercile	67	52	0.254	0.192	0.061	0.072	0.375
						(0.067)	

Note. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. Early Cohort income tercile ranges are $0-$9,818, $9,819-$17,152, and $17,193-$37,002. Late Cohort income tercile ranges are $0-$8,870, $8,875-$17,188, and $17,189-$44,923. Survey Cohort income tercile ranges are $0-$9,925, $9,926-18,958, and $18,959-$66,188.

[a]Percentage-point difference between GAIN and control groups.

[b]Percent difference between GAIN and control groups.

Appendix Table A3. ITT Effects for Total Income at 12- and 24-Months Post-Randomization

	GAIN observations	Control observations	GAIN mean	Control mean	Raw mean difference (ITT)[a]	Regression-adjusted difference (ITT)[a]	Effect size (Adjusted ITT/control mean)[b]
Panel A: Full Sample							
Early Cohort							
12 months	960	1473	16024.45	15537.82	486.63	312.10	0.020
						(329.90)	
24 months	960	1473	31778.40	31195.74	582.66	233.20	0.007
						(679.90)	
Late Cohort							
12 months	779	2120	16548.87	16090.47	458.40	219.10	0.014
						(378.30)	
24 months	779	2120	32750.30	31939.89	810.41	279.90	0.009
						(787.00)	
Survey Cohort							
12 months	365	356	17327.35	18789.57	-1462.20	-1053.00*	-0.056*
						(637.70)	
24 months	365	356	34843.02	37183.52	-2340.50	-1347.00	-0.036
						(1356.10)	
Panel B: By income tercile							
Early Cohort							
12 months, bottom tercile	316	495	7073.30	6912.70	160.60	650.20+	0.094+
						(359.30)	
24 months, bottom tercile	316	495	14430.50	14292.10	138.40	1105.30	0.077

						(775.10)	
12 months, middle tercile	319	492	13891.50	13456.50	435.00	75.76	0.006
						(385.50)	
24 months, middle tercile	319	492	27628.70	27018.20	610.50	-70.53	-0.003
						(811.10)	
12 months, top tercile	325	486	26821.30	26429.60	391.70	28.58	0.001
						(819.90)	
24 months, top tercile	325	486	52718.90	52641.50	77.40	-745.60	-0.014
						(1668.20)	
Late Cohort							
12 months, bottom tercile	273	694	5366.90	5890.80	-523.90	-86.29	-0.015
						(373.30)	
24 months, bottom tercile	273	694	11560.30	12325.70	-765.40	-73.96	-0.006
						(849.50)	
12 months, middle tercile	246	720	13494.10	13002.90	491.20	324.60	0.025
						(450.70)	
24 months, middle tercile	246	720	27039.30	25750.40	1288.90	1021.00	0.040
						(917.90)	
12 months, top tercile	260	706	31180.20	29265.60	1914.60	397.80	0.014
						(953.80)	
24 months, top tercile	260	706	60403.30	57533.00	2870.30	-330.10	-0.006
						(1980.60)	
Survey Cohort							
12 months, bottom tercile	115	125	6708.50	7314.30	-605.80	-567.30	-0.078
						(666.60)	
24 months, bottom tercile	115	125	14389.20	15419.50	-1030.30	-1024.20	-0.066

						(1483.30)	
12 months, middle tercile	125	116	14718.00	15105.50	-387.60	-96.58	-0.006
						(950.10)	
24 months, middle tercile	126	116	29240.70	29932.00	-691.30	-142.10	-0.005
						(1924.60)	
12 months, top tercile	125	115	29706.10	34978.70	-5272.60*	-1510.10	-0.043
						(1372.00)	
24 months, top tercile	125	115	59262.90	68154.70	-8891.80*	-1584.50	-0.130
						(2846.10)	

Note. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. Early Cohort income tercile ranges are $0-$9,818, $9,819-$17,152, and $17,193-$37,002. Late Cohort income tercile ranges are $0-$8,870, $8,875-$17,188, and $17,189-$44,923. Survey Cohort income tercile ranges are $0-$9,925, $9,926-18,958, and $18,959-$66,188.
[a]Percentage-point difference between GAIN and control groups.
[b]Percent difference between GAIN and control groups.
+p<.10; *p<.05.

Appendix Table A4. ITT Effects for Income Instability at 12- and 24-Months Post-Randomization

	GAIN observations	Control observations	GAIN mean	Control mean	Raw mean difference (ITT)[a]	Regression-adjusted difference (ITT)[a]	Effect size (Adjusted ITT/control mean)[b]
Panel A: Full Sample							
Early Cohort							
12 months	960	1473	0.281	0.280	0.001	-0.006	-0.021
						(0.014)	
24 months	960	1473	0.391	0.396	-0.006	-0.018	-0.045
						(0.019)	
Late Cohort							
12 months	779	2120	0.306	0.343	-0.037*	-0.032*	-0.093*
						(0.016)	
24 months	779	2120	0.408	0.451	-0.0433*	-0.039*	-0.086*
						(0.020)	
Survey Cohort							
12 months	365	356	0.307	0.289	0.107	0.027	0.093
						(0.024)	
24 months	365	356	0.361	0.368	0.008	-0.005	-0.014
						(0.024)	
Panel B: By income tercile							
Early Cohort							
12 months, bottom tercile	316	495	0.317	0.344	-0.027	-0.049	-0.142
						(0.031)	
24 months, bottom tercile	316	495	0.472	0.525	-0.053	-0.120**	-0.229**

						(0.043)	
12 months, middle tercile	319	492	0.273	0.270	0.003	0.005	0.019
						(0.023)	
24 months, middle tercile	319	492	0.379	0.369	0.010	0.008	0.022
						(0.029)	
12 months, top tercile	325	486	0.253	0.225	0.028	0.025	0.111
						(0.016)	
24 months, top tercile	325	486	0.323	0.293	0.030	0.029	0.099
						(0.020)	
Late Cohort							
12 months, bottom tercile	273	694	0.360	0.437	-0.077*	-0.084*	-0.192*
						(0.035)	
24 months, bottom tercile	273	694	0.483	0.588	-0.105*	-0.119**	-0.202
						(0.043)	
12 months, middle tercile	246	720	0.281	0.302	-0.021	-0.012	-0.040
						(0.021)	
24 months, middle tercile	246	720	0.367	0.413	-0.046	-0.040	-0.097
						(0.028)	
12 months, top tercile	260	706	0.273	0.293	-0.020	-0.015	-0.051
						(0.022)	
24 months, top tercile	260	706	0.369	0.357	0.012	0.022	0.062
						(0.027)	
Survey Cohort							
12 months, bottom tercile	115	125	0.393	0.327	0.066	0.095+	0.291+
						(0.049)	
24 months, bottom tercile	115	125	0.439	0.427	0.012	0.032	0.075

						(0.048)	
12 months, middle tercile	125	116	0.272	0.289	-0.017	-0.015	-0.052
						(0.029)	
24 months, middle tercile	126	116	0.360	0.356	0.004	0.001	0.003
						(0.033)	
12 months, top tercile	125	115	0.261	0.249	0.013	0.011	0.044
						(0.031)	
24 months, top tercile	125	115	0.290	0.318	-0.028	-0.029	-0.088
						(0.032)	

Note. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. Early Cohort income tercile ranges are $0-$9,818, $9,819-$17,152, and $17,193-$37,002. Late Cohort income tercile ranges are $0-$8,870, $8,875-$17,188, and $17,189-$44,923. Survey Cohort income tercile ranges are $0-$9,925, $9,926-18,958, and $18,959-$66,188.
[a]Percentage-point difference between GAIN and control groups.
[b]Percent difference between GAIN and control groups.
+p<.10; *p,.05; **p<.01.

Appendix Table A5. ITT Effects for Earnings Instability at 12- and 24-Months Post-Randomization

	GAIN observations	Control observations	GAIN mean	Control mean	Raw mean difference (ITT)[a]	Regression-adjusted difference (ITT)[a]	Effect size (Adjusted ITT/control mean)[b]
Panel A: Full Sample							
Early Cohort							
12 months	960	1473	0.420	0.431	-0.010	-0.011	-0.026
						(0.023)	
24 months	960	1473	0.574	0.582	-0.008	-0.013	-0.022
						(0.025)	
Late Cohort							
12 months	779	2120	0.410	0.445	-0.036	-0.019	-0.042
						(0.023)	
24 months	779	2120	0.530	0.574	-0.045	-0.019	-0.033
						(0.024)	
Survey Cohort							
12 months	365	356	0.447	0.430	0.017	0.008	0.019
						(0.036)	
24 months	365	356	0.584	0.559	-0.025	0.029	0.052
						(0.043)	
Panel B: By income tercile							
Early Cohort							
12 months, bottom tercile	316	495	0.359	0.369	-0.010	0.005	0.014
						(0.042)	
24 months, bottom tercile	316	495	0.546	0.561	-0.015	-0.027	-0.048

						(0.049)	
12 months, middle tercile	319	492	0.504	0.477	0.027	0.035	0.073
						(0.043)	
24 months, middle tercile	319	492	0.665	0.636	0.029	0.035	0.055
						(0.046)	
12 months, top tercile	325	486	0.397	0.446	-0.049	-0.048	-0.108
						(0.035)	
24 months, top tercile	325	486	0.511	0.549	-0.038	-0.032	-0.058
						(0.036)	
Late Cohort							
12 months, bottom tercile	273	694	0.322	0.393	-0.071	-0.042	-0.107
						(0.040)	
24 months, bottom tercile	273	694	0.466	0.566	-0.100	-0.060	-0.106
						(0.045)	
12 months, middle tercile	246	720	0.525	0.528	-0.003	0.010	0.019
						(0.045)	
24 months, middle tercile	246	720	0.672	0.670	0.002	0.016	0.024
						(0.046)	
12 months, top tercile	260	706	0.392	0.413	-0.021	-0.020	-0.048
						(0.033)	
24 months, top tercile	260	706	0.462	0.485	-0.023	-0.007	-0.014
						(0.033)	
Survey Cohort							
12 months, bottom tercile	115	125	0.44	0.459	-0.019	-0.030	-0.065
						(0.066)	
24 months, bottom tercile	115	125	0.625	0.635	-0.010	0.053	0.083

						(0.087)	
12 months, middle tercile	125	116	0.489	0.536	-0.047	-0.072	-0.134
						(0.069)	
24 months, middle tercile	126	116	0.681	0.637	0.044	0.016	0.025
						(0.079)	
12 months, top tercile	125	115	0.410	0.292	0.119*	0.089+	0.305+
						(0.047)	
24 months, top tercile	125	115	0.449	0.398	0.051	0.005	0.013
						(0.049)	

Note. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. Early Cohort income tercile ranges are $0-$9,818, $9,819-$17,152, and $17,193-$37,002. Late Cohort income tercile ranges are $0-$8,870, $8,875-$17,188, and $17,189-$44,923. Survey Cohort income tercile ranges are $0-$9,925, $9,926-18,958, and $18,959-$66,188.

[a]Percentage-point difference between GAIN and control groups.

[b]Percent difference between GAIN and control groups.

*p<.05.

Appendix Table A6. ITT Effects for Benefits Instability at 12- and 24-Months Post-Randomization (SNAP, MA, W2, SSI, CSE, UI)

	GAIN observations	Control observations	GAIN mean	Control mean	Raw mean difference (ITT)[a]	Regression-adjusted difference (ITT)[a]	Effect size (Adjusted ITT/control mean)[b]
Panel A: Full Sample							
Early Cohort							
12 months	960	1473	0.371	0.338	0.033	0.028	0.083
						(0.019)	
24 months	960	1473	0.514	0.502	0.013	0.007	0.014
						(0.025)	
Late Cohort							
12 months	779	2120	0.360	0.384	-0.023	-0.013	-0.034
						(0.021)	
24 months	779	2120	0.533	0.544	-0.01	-0.004	-0.007
						(0.027)	
Survey Cohort							
12 months	365	356	0.373	0.322	0.051	0.046	0.143
						(0.035)	
24 months	365	356	0.499	0.491	0.008	-0.013	-0.026
						(0.050)	
Panel B: By income tercile							
Early Cohort							
12 months, bottom tercile	316	495	0.323	0.314	0.009	-0.012	-0.038
						(0.034)	
24 months, bottom tercile	316	495	0.458	0.488	-0.030	-0.082+	-0.168+

						(0.045)	
12 months, middle tercile	319	492	0.331	0.316	0.015	0.016	0.051
						(0.029)	
24 months, middle tercile	319	492	0.426	0.447	-0.021	-0.014	-0.031
						(0.035)	
12 months, top tercile	325	486	0.458	0.386	0.072*	0.085*	0.220*
						(0.036)	
24 months, top tercile	325	486	0.656	0.571	0.085	0.103*	0.180*
						(0.048)	
Late Cohort							
12 months, bottom tercile	273	694	0.314	0.379	-0.065	-0.060+	-0.158+
						(0.036)	
24 months, bottom tercile	273	694	0.444	0.536	-0.092*	-0.094*	-0.175*
						(0.045)	
12 months, middle tercile	246	720	0.305	0.334	-0.029	-0.020	-0.060
						(0.029)	
24 months, middle tercile	246	720	0.443	0.469	-0.026	-0.021	-0.045
						(0.037)	
12 months, top tercile	260	706	0.462	0.439	0.023	0.039	0.089
						(0.041)	
24 months, top tercile	260	706	0.714	0.627	0.087	0.095+	0.152+
						(0.054)	
Survey Cohort							
12 months, bottom tercile	115	125	0.308	0.331	-0.023	-0.033	-0.100
						(0.052)	
24 months, bottom tercile	115	125	0.405	0.450	-0.046	-0.075	-0.167

						(0.067)	
12 months, middle tercile	125	116	0.385	0.339	0.046	0.064	0.189
						(0.055)	
24 months, middle tercile	126	116	0.536	0.467	0.068	0.096	0.206
						(0.075)	
12 months, top tercile	125	115	0.421	0.296	0.125*	0.112+	0.378+
						(0.067)	
24 months, top tercile	125	115	0.550	0.561	0.011	-0.077	-0.137
						(0.102)	

Note. Models adjust for the full set of covariates presented in Table 2, as well as period and quarter of randomization fixed effects. Early Cohort income tercile ranges are $0-$9,818, $9,819-$17,152, and $17,193-$37,002. Late Cohort income tercile ranges are $0-$8,870, $8,875-$17,188, and $17,189-$44,923. Survey Cohort income tercile ranges are $0-$9,925, $9,926-18,958, and $18,959-$66,188.

[a]Percentage-point difference between GAIN and control groups.

[b]Percent difference between GAIN and control groups.

+p<.10; *p<.05.