When a Dollar is Not a Dollar: Examining How Timing and Delivery of Government Transfers Influence Household Consumption Decisions

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ABSTRACT

Governments often implement wealth transfers with different policy goals (e.g., boost spending, reduce poverty, etc.) and different distribution methods. Prior research examines the timing (i.e., lump sum or periodic) of these transfers but fails to simultaneously consider delivery method (i.e., standalone or combined with other income). Based on the behavioral life-cycle model, we predict payment timing influences how recipients spend government transfers but that this effect will be muted when transfers are combined with other income (i.e., tax refunds or paychecks). In contrast to the findings of prior research – but consistent with our theory-driven predictions – our results from an experiment using real-world incentives suggest recipients of a periodic transfer spend more of the transfer than recipients of a lump sum transfer when the transfer is paid as a standalone payment(s). We observe no effect of payment timing for combined payments. These results suggest the effect of payment timing is diminished when income is combined with other funds. Accordingly, our findings help to explain theory-inconsistent results of prior research and extend the literature on the behavioral life-cycle model and mental budgeting. Moreover, we provide important insights to policymakers regarding structuring the distribution of government transfers to best achieve specific policy goals.

Keywords: Behavioral life-cycle, mental budgeting, stimulus, subsidy, government transfer

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I. INTRODUCTION

Governments often implement economically significant programs to transfer wealth back to citizens, usually with the intent of either stimulating a troubled economy or subsidizing the budgets of specific households. For example, over $800 billion of economic impact payments (EIPs) were distributed throughout 2020 and 2021 (IRS 2022) to stimulate the economy during the COVID-19 pandemic. Similarly, the Earned Income Tax Credit (EITC)—which is one of the largest social subsidy programs in the US—is expected to distribute over $750 billion over the 10-year period from 2022 to 2031 (U.S. Department of Treasury 2021). Importantly, different government transfer programs have different implicit goals, especially with respect to the desired consumption behaviors of recipients (Taylor 2000; Shapiro and Slemrod 2003). Stimulus transfers are intended to boost the economy and are most effective when transfers result in additional current spending (Taylor 2009). Conversely, subsidy transfers are intended to provide resources to targeted households and are more effective when they result in a mix of spending and savings to cover both current needs and unforeseen future circumstances (Greene 2013).

Although many transfer programs are motivated by a particular policy goal that would suggest a preference for either spending or saving, prior research suggests most programs are ineffectively structured to align with those policy goals. For example, a review of surveys related to the recovery rebate checks (a stimulus transfer) sent to households in 2008 finds most report a spend rate of between 19 percent and 23 percent (Sahm, Shapiro, and Slemrod 2010). Similarly, some commentators on the EITC (a subsidy transfer) argue that it fails to encourage recipients to save for the future (Weber 2016). In this study, we examine how features related to the payment of these types of transfers impact the extent to which the transfers are used in a manner

consistent with intended policy goals. Specifically, we examine the simultaneous impacts of two features of government transfer payments: payment timing and payment delivery method.

Payment timing refers to whether a transfer is paid in a single large lump sum or in multiple smaller (but economically-equivalent in aggregate) periodic payments. Behavioral economics theory predicts periodic payments will result in more spending than lump sum payments (Shefrin and Thaler 1988). Two studies directly test this prediction but do not find support for the theoretical expectations (Coronado, Lupton, and Sheiner 2005; Sahm, Shapiro, and Slemrod 2012). Coronado et al. (2005) finds no impact for payment timing while Sahm et al. (2012) interestingly finds results which directly contradict the implications of the theory they use to motivate examining the effect of payment timing. These studies both use surveys to ask participants to recall if they mostly spent or mostly saved recently enacted government transfers where one of the transfers was paid as a lump sum and another was paid as series of periodic payments. Importantly, both studies examine data in which the periodic transfer was paid as increases in the recipients' paychecks, but the lump sum payment was sent directly to the recipient and not combined with any other funds. While these studies undoubtedly examine payment timing, they do so while also observing differences in how the transfer is delivered. In the current study, we reexamine the impact of payment timing while using a controlled experiment to examine the moderating effect of delivery method.

Payment delivery method describes whether payments are standalone or combined. Standalone payments are sent directly to recipients without being combined with any other funds. For example, EIPs in 2020 and 2021 were primarily paid in the form of direct deposits, paper checks, or prepaid debit cards. Similarly, recipients of the Child Tax Credit (CTC) in 2021 could opt to have a portion of their credit prepaid to them directly on a monthly basis. Combined payments are those that are paid along with other funds. Most combined payments, such as the

EITC, are implemented via a lump sum tax credit that increases the size of a recipient's tax refund. However, some tax credits—as was the case in the examples examined by prior research—are accompanied by reductions in withholdings, which effectively divide the transfer into multiple periodic payments received as slightly larger paychecks instead of larger tax refunds. Combined payment methods are often favored by governments because they rely on existing payment systems and may be more administratively efficient (Hotz and Scholz 2003).

Neither the timing nor delivery method of a government transfer *should* influence how they are used as every dollar is theoretically fungible, but research on mental accounting and mental budgeting suggests cognitive biases lead individuals to treat wealth as though it is not fungible (Thaler 1990). Specifically, mental budgeting describes the psychological processes by which individuals constrain their consumption by budgeting income and tracking expenses in mental accounts (Thaler 1985). Shefrin and Thaler (1988) expand on mental budgeting to propose the behavioral life-cycle model that suggests households budget wealth into broad categories of mental accounts that differ in propensity to consume (versus save). This model strongly indicates lump sum payments are more likely to be budgeted to accounts with a higher propensity to be saved and economically equivalent periodic payments are more likely to be budgeted to accounts with a higher propensity to be spent (Thaler and Shefrin 1981; Shefrin and Thaler 1988). Subsequent research confirms these predictions in settings outside of government transfer payments (e.g., Hymans and Shapiro 1976; Graham and Isaac 2002).

Despite the findings of this literature, the previously mentioned studies of government transfer payments find results inconsistent with the behavioral life-cycle model (e.g., Coronado et al. 2005; Sahm, et al. 2012). We posit differences in payment delivery method likely explain why past studies do not find an effect consistent with the behavioral life-cycle model. When a government transfer is received as a standalone payment, the timing of the payment is likely the

most salient feature of the income which in turn results in a mental budgeting process most similar to the one described by the behavioral life-cycle model. When a government transfer is combined with other sources of income, recipients likely associate the features of that other income with the transfer, which in turn leads to a budgeting process that incorporates more than just the timing of the payment. Accordingly, we predict recipients are more likely to spend transfers received as periodic payments than when they receive economically equivalent lump sum payments, especially when the transfers are delivered as standalone payments.

To test our predictions, we utilize a 2x2 between-subjects multi-round experiment manipulating payment timing (periodic or lump sum) and payment delivery method (standalone or combined) of a government transfer. The experiment consists of 12 rounds (three hypothetical years of four rounds each) wherein participants perform a task to earn an experimental currency. In each round, participants then use their earnings to pay expenses before allocating any remainder to either spending or savings. Importantly, participants' compensation for completing the experiment is designed to mimic real world incentives for saving and spending. Specifically, bonus amount and timing are determined in accordance with allocation to savings (a larger but delayed bonus) or spending (a smaller but immediate bonus).[1] Participants in all conditions receive a government transfer during the third year of the experiment, and the dependent variable is the amount of income allocated to spending during the third year.

Results based on responses from 502 participants recruited from Amazon's Mechanical Turk (MTurk) provide several interesting inferences. We start by replicating the findings of prior research. Consistent with prior findings, we find no evidence that participants who receive a transfer as a smaller increase across multiple paychecks spend more than participants who

[1] This approach to bonuses attempts to replicate the utility a person may receive in real life. Saving generates interest income but defers consumption (thus, a larger delayed bonus) while spending allows for immediate consumption but forfeits the possibility of investment growth (thus, a smaller immediate bonus).

receive a transfer as a larger single direct payment. Next, we examine the conditional effects of payment timing when delivery method is held constant. Consistent with the hypotheses, we find periodic government transfers result in more spending than lump sum transfers, but only when the transfer is paid as a standalone payment and not when combined with other payments. This pattern suggests the behavioral life-cycle model is predictive with respect to government transfers, but combining a government transfer with other funds impedes recipients from using the predicted mental budgeting approach. Finally, we find no significant differences in aggregate spending among participants who receive a transfer combined with their paycheck, combined with their tax refund, and as a separate lump sum payment.

This study makes several important contributions. First, this study contributes to public policy literature by demonstrating the effects of payment characteristics on government transfer program outcomes. Our findings should be particularly important to policymakers as they design future government transfer programs. For subsidy transfer programs, our results suggest policymakers can encourage a mix of spending and saving that is more consistent with policy goals by implementing transfers via lump sum payments or periodic payments paid through larger paychecks. Differentiation between these approaches can be made based on other differences (e.g., administrative burden, temporal congruence between payment and need, etc.).

Perhaps more importantly, our results are the first to suggest government transfer programs will be more efficient at achieving the policy goal of stimulating the economy by promoting spending when paid as standalone periodic payments. Accordingly, recurring direct payments like the Canadian Emergency Response Benefits or enhanced unemployment benefits during the COVID-19 pandemic may have been more effective at stimulating the economy than other transfer programs. Our results are particularly relevant as recent policy proposals (e.g., Sahm 2019) call for future stimulus transfers to be implemented using lump sum payments in

part as a consequence of relying on the findings of prior research (e.g., Sahm et al. 2012) which do not consider the moderating and confounding effect of payment delivery method.

Additionally, we extend prior literature that explicitly examines the impact of government transfer payment timing on consumption behavior. While only two studies attempt to explicitly test this relationship, a broader swath of research and commentator opinions hold the previously unsupported belief that periodic payments would result in more consumption and less saving than lump sum payments. Our results suggest the underlying theory is descriptively accurate, but data limitations (namely, data in which payment timing and delivery method are not isolated) likely obscured findings in those past studies. We leverage the advantage of a controlled experiment to isolate and independently manipulate payment timing and delivery method to identify the moderating role of delivery method. Moreover, by using a simulation with real incentives to save versus spend, we more precisely measure consumption behavior (as compared to a survey based on recalling and reporting past consumption behavior).

Finally, our study contributes to multiple academic literatures. With respect to the literature on the behavioral life-cycle model and mental accounting/budgeting, our results reaffirm the finding that periodic payments are more likely to be budgeted to mental accounts associated with higher propensities to consume. However, our experiment also highlights that this budgeting approach can be interrupted when funds are received concurrently with income from other sources that may be mentally budgeted based on other factors. We also add to the limited accounting literature which examines how households account for and use funds based on the characteristics of the funds (Romich and Weisner 2000; Bobek, Hatfield, and Wentzel 2007; Weber 2016). The timing and delivery characteristics may cause households to account for funds differently, which could have long term implications on the financial well-being and quality of life of a household. Moreover, our study contributes to the literature examining how

behaviors are influenced through tax policy (e.g., Austin, Bobek, LaMothe 2020; Clemons and Shevlin 2016; Falsetta, Rupert, Wright 2013; Stinson, Doxey, Rupert 2021) as combined transfers are most frequently implemented through the income tax system.

II. BACKGROUND AND HYPOTHESIS DEVELOPMENT

Government Transfers, Policy Objectives, and Spending versus Saving

Government transfer programs undeniably result in very large aggregate transfers between governments and individuals. The IRS (2022) reports distributing over $800 billion across three rounds of economic impact payments in 2020 and 2021. Similarly, recovery rebate (stimulus) checks sent out in 2008 totaled $96 billion (Sahm et al. 2010). While very large, these one-time transfers may ultimately be smaller than the cumulative costs of ongoing transfer programs. For example, the U.S. Department of Treasury (2021) indicates the EITC and CTC will cost $769 billion and $312 billion, respectively, over the 10-year period from 2022 to 2031.[2]

Government transfer programs can be enacted with a number of stated policy goals, but the motivation behind a particular program can often be placed into one of two categories: stimulus or subsidy. Stimulus transfers are typically more widely distributed and are primarily intended to boost the economy (i.e., spending), while subsidy transfers are usually targeted programs with narrow qualifications intended to provide assistance to specific households. This is not to say any particular transfer program will easily fit (or always stay) in only one of these two categories. For example, the EITC is currently regarded as the largest anti-poverty (i.e., subsidy) program in the U.S. (Hungerford and Thiess 2013), but some argue at least one original purpose was to stimulate the economy due to recessionary conditions in 1974 (Herbst 2011). In a similar manner, unemployment insurance can act as a subsidy transfer to underemployed

[2] These figures represent the outlay effects of the respective programs and do not include the costs associated with forgone tax revenue (i.e., tax expenditures). The additional estimated costs in forgone tax revenue are $72.6 billion for the Child Tax Credit and $2 billion for the Earned Income Tax Credit (U.S. Department of Treasury 2021).

households in the best of times but also an important economic stabilizer in economic downturns (Gwyn 2022). However, if a given transfer program can be placed into one of these two categories with a well-defined purpose, policymakers may be able to optimize the transfer to achieve a particular goal. In this study, we examine whether policy design choices can influence spending and saving behaviors in accordance with a given policy goal (e.g., stimulus or subsidy).

Prevailing economic theory suggests stimulus transfers are only effective when the transfer is spent rather than saved. Taylor (2009, 550) notes "the macroeconomic theory that rationalizes such temporary rebate payments is that they increase the demand for consumption, stimulate aggregate demand, and thereby help get the economy on a path to recovery." Researchers have extensively examined whether stimulus transfers achieve this goal, and some suggest they are unwarranted (Eichenbaum 1997; Taylor 2000; Feldstein 2003; Taylor 2009). Some studies find evidence that stimulus transfers engender more saving or paying off debt than spending (Shapiro and Slemrod 2009; Coibion, Gorodnichenko, and Weber 2020; Karpman, Maag, Kenney, and Wissoker 2021). Other studies do find evidence that these stimulus transfers promote at least some spending (Broda and Parker 2014; Asebedo, Liu, Gray, and Quadria 2020; Li et al. 2021; Parker, Schild, Erhard, and Johnson 2022), but other evidence suggests a large portion of any such transfer is not spent immediately (Shapiro and Slemrod 2003; Coronado et al. 2005; Johnson, Parker, and Souleles 2006; Johnson, Parker, Souleles 2009; Sahm et al. 2010; Parker, Souleles, Johnson, and McClelland 2013).[3]

In contrast, prior research suggests the policy preference for spending or saving with

[3] In this literature, paying down debt is viewed as a form of savings (Shapiro and Slemrod 1995). From a stimulus perspective, paying debt is equivalent to saving as it does not increase aggregate demand. Moreover, paying down debt has the same impact of increasing net worth as savings (Chambers and Spencer 2008) and debt (especially credit card debt) is a necessary substitute for nonexistent savings among asset poor households (Greene 2013). The extent to which paying of debt and savings are distinguished in this literature is often just for the benefit of avoiding confusion among respondents. In the current study and in our experiment, we avoid this labeling issue and do not distinguish between paying down debt and saving.

respect to a subsidy transfer is more ambiguous. Subsidy transfers are intended to help cover the needs of recipients (Hotz and Scholz 2003; Hamilton, Roll, Despard, Maag, and Chun 2021; Lens, Arriaga, Pisciotta, Bushman-Copp, Spencer, Kronenfeld 2022). However, multiple studies emphasize the importance of subsidizing savings (rather than just daily needs) to help recipients cover unexpected expenses and reduce reliance on the subsidy programs (McKernan, Ratcliffe, and Shanks 2012; Hamilton, Rothwell, Huang, Nam, and Dollar 2020). Moreover, some argue a critical failure of programs like the EITC is that they can sometimes discourage savings (Weber 2016; Hamilton et al. 2020). Others propose using alternative implementations of the EITC that can be designed to encourage more savings (Greene 2013; Halpern-Meekin, Greene, Levin, and Edin 2018). This discussion suggests policymakers may prefer stimulus transfers to encourage more spending while a mix of spending and saving is more desirable for subsidy transfers.

Transfer Payment Characteristics

Despite the tremendous costs of government transfer programs, little consensus exists on the best way to make the actual transfers. Prior research primarily examines whether government transfers are paid as a lump sum or recurring periodic payments. For example, Sahm et al. (2012) examine differences in consumption between the lump sum 2008 recovery rebate checks and the 2009 Making Work Pay credit that was primarily paid through multiple increases to recipients' paychecks. Similarly, other studies examine the potential impact of structuring the EITC and CTC as lump sum versus periodic payments (see Romich and Weisner 2000; Greene 2013; Greenlee, Kramer, Andrade, Bellisle, Blanks, and Mendenhall 2021; Lens et al. 2022).

In some cases, the timing of government transfer payments can be decided by the recipient. For example, the U.S. allowed recipients to decide whether they wanted to receive their EITC as a lump sum or recurring periodic payments from 1979 until 2010 (Government Accountability Office 2007; Holt 2015). Similar programs around the world—such as the Family

Tax Benefit program in Australia and the Working for Families Payment program in New Zealand—continue to allow recipients to decide whether to receive (at least a portion of) their transfers in lump sum or periodic payments. Other programs do not provide such a choice. For example, the Supplemental Security Income Program in the U.S., the Revenu de Solidarité Active in France, the Universal Credit in the U.K., and the Working Family Payment program in Ireland are only paid on a recurring (i.e., periodic) basis while currently the EITC in the U.S. and the similar Working Income Tax Benefit program in Canada are only distributed as lump sums.

Given the importance of spending to achieve the policy goal of a stimulus transfer, prior research examines whether payment timing can significantly impact how transfers are consumed. Based on arguments related to mental accounting and the behavioral life-cycle model (discussed in detail below), this literature predicts recipients will spend a greater portion of a government transfer when it is received as a series of periodic payments rather than as an economically equivalent lump sum. Two studies attempt to directly test this assertion, but both curiously do not find results consistent with theory. First, Coronado et al. (2005) examines the extent to which the Jobs and Growth Tax Relief and Reconciliation Act (JGTRRA) of 2003 stimulated spending. Importantly, provisions of JGTRRA both increased take-home pay (via a reduction in marginal tax rates that manifested as a reduction in withholdings) and authorized a rebate check as a prepayment of an increase in the CTC. Based on a retrospective survey of U.S. taxpayers, the authors find no significant difference in the percentage of households indicating they mostly spent the money when it was received as a periodic increase in their paychecks via a reduction in withholdings (20.6 percent) or as a lump sum advanced rebate check (21.4 percent).

Second, Sahm et al. (2012) investigates whether spending differed between recovery rebates in 2008 and the Making Work Pay Credit in 2009. Importantly, the Making Work Pay Credit was accompanied by reduced withholdings that effectively held tax refunds/taxes due

constant while increasing take-home pay. The recovery rebate checks paid a similar amount of money as a lump sum check sent directly to taxpayers. Contrary to expectations, the survey results showed about half as many households recall mostly spending the money received as reduced withholdings (13 percent) rather than as a rebate check (25 percent). The authors suggest differences in macroeconomic conditions from 2008 to 2009 could have impacted the results, but otherwise offer little explanation for their unexpected findings.

While both studies make important first steps towards understanding how the timing of government transfers affects how they are used, they both suffer from similar limitations. Methodologically, both studies rely on a survey that asks participants to recall whether they "mostly spent," "mostly saved," or "mostly paid off debt" in response to the government transfers. Sahm et al. (2012) acknowledges that, while overcoming limitations inherent to observational cross-sectional data, this survey method likely creates two potential concerns. First, the payment timing could differentially impact the respondents' abilities to accurately gauge how their behaviors were impacted (i.e., recalling how an additional $10 payment was used in each month of a year could be more difficult than recalling how a single $120 payment was used). Second, the survey responses may not have accurately reflected actual behavior perhaps either due to social desirability bias or other motivations to report in a particular way.

However, we focus on one additional concern that potentially limits the inferences drawn from these past studies: payment delivery method. Both studies discussed above certainly examine differences in consumption behavior between periodic and lump sum government transfer payments. However, the periodic payment in both studies is paid in combination with the recipient's paycheck while the lump sum payment is paid as a standalone check. Importantly, payment delivery method is an independent feature of the transfer that is not necessarily tied to payment timing. For example, both the EITC and CTC have been allowed to be paid (at least in

part) as periodic payments at some point in their respective histories. However, the periodic payments for the EITC were delivered in combination with paychecks (i.e., via reduced withholdings) while periodic payments for the CTC were delivered directly to recipients.

Similarly, lump sum transfers can also either be combined or standalone payments. When the EITC or CTC are paid via a lump sum, they are claimed on the recipient's tax return and are delivered in combination with a tax refund. Other lump sum transfers are delivered directly to the recipients and are not combined with any other funds. For example, all three rounds of economic impact payments in the U.S. in 2020 and 2021 were paid directly to recipients.[4] In this study, we examine how both payment timing and delivery method interact to impact how transfers are used and, consequently, the extent to which transfers achieve intended policy goals. As highlighted in Exhibit 1, government transfer programs have used a variety of combinations of payment delivery method and payment timing, including the combinations we examine in this study.

Mental Accounting, Mental Budgeting, and the Behavioral Life-Cycle Model

Mental accounting is "the set of cognitive operations used by individuals and households to organize, evaluate, and keep track of financial activities" (Thaler 1999, 183). One component of mental accounting concerns how activities are categorized into mental budgets that are used to induce self-control in consumption behavior. Specifically, mental budgeting involves a two-step process in which individuals allocate income and expenditures to mental accounts through budget setting and expense tracking steps, respectively (Heath 1995; Heath and Soll 1996). Prior research suggests these budget setting and expense tracking processes collectively allow individuals to rationally plan out their consumption and subsequently constrain their impulsive

[4] These payments were technically prepayments of a Recovery Rebate Credit received on a subsequent tax return. Many transfer programs involving a standalone payment are structured this way, meaning the transfer ultimately could be received either as a standalone payment (direct check) or as a lump sum combined payment (tax refund). However, the standalone payment is usually intended to be the primary payment method and combined lump sum payments serve as a "backup" to ensure recipients receive the entire transfer even if the lump sum prepayment is calculated incorrectly (usually due to relying on outdated information in the interest of making timely payments).

consumption behaviors (Thaler and Shefrin 1981). Specifically relevant to the current study, Shefrin and Thaler (1988) use the concepts in mental budgeting to build a model of lifetime consumption decisions called the behavioral life-cycle model.

Shefrin and Thaler (1988) developed the behavioral life-cycle model in response to critiques of older models of lifetime consumption decisions including the life-cycle theory of saving (Modigliani and Brumberg 1954; Ando and Modigliani 1963) and the permanent income hypothesis (Friedman 1957). The behavioral life-cycle model posits households budget income across mental accounts that fit into three broad categories: current income, current assets, and future income (Thaler 1990).[5] Shefrin and Thaler (1988) define these mental account categories in terms of differences in marginal propensity to consume (versus save): current income accounts have the highest propensity to be consumed, future income accounts have the lowest propensity to be consumed, and current assets are typically saved but are more easily consumed than future income (Shefrin and Thaler 1988; Thaler 1990). For example, current income accounts consist of funds allocated to everyday current spending such as groceries, utilities, and entertainment while current asset accounts would consist of funds such as vacation, college, and "rainy day" savings that can be drawn upon but are typically reserved for some future purpose. Future income accounts consist of very illiquid sources of wealth that frequently are not even explicitly accounted for such as home equity, career potential, retirement accounts, and future inheritance.

Even before the behavioral life-cycle model had been proposed, research showed different categories of income were saved and consumed at different rates (Holbrook and

[5] While current income, current assets, and future income are frequently referred to as specific mental accounts, we conceptualize them as categories of accounts because Shefrin and Thaler (1988, 615) indicate "this three-account formulation is a great simplification of the actual mental accounting rules. In general, a more realistic model would break up the [current assets] account into a series of subaccounts, appropriately labeled." Other research suggests similar subdivisions of current income accounts is also likely (Beshears, Choi, Laibson, and Madrain 2018). For example, individuals appear to maintain nonfungible mental accounts related to consumption in categories such as entertainment (Heath and Soll 1996) and vehicle fuel (Hastings and Shapiro 2013).

Stafford 1971; Hymans and Shapiro 1976). This stream of literature has been taken as implicit evidence in support of the model. Moreover, subsequent research shows different sources of income lead to different levels of consumption and provides evidence to explicitly suggest income from different sources is often allocated to different categories of mental accounts (Carriker, Langemeier, Schroeder, and Featherstone 1993; Graham and Isaac 2002).

Hypothesis Development

Research based on the behavioral life-cycle model generally agrees on the core principle that income is budgeted to different categories of mental accounts and that these categories differ in terms of marginal propensity to consume (MPC). Moreover, future income accounts are fairly well defined in the literature. These mental accounts are typically distinguished by association with holdings in illiquid assets such as home equity, human capital (i.e., future earnings potential), and retirement accounts (Thaler 1999). Shefrin and Thaler (1988) suggest future income accounts are funded via heuristic rules (e.g., $100 a month goes to retirement) or forced mechanisms (e.g., participation in retirement plans requiring a particular contribution). In contrast, the factors that influence how funds are budgeted to either current income or current assets accounts are far less studied with two notable exceptions: payment timing and source.

With respect to payment timing, prior research concludes there is a relationship between payment timing and how income is budgeted between current income and current asset accounts. This effect is rooted in the impact of payment size: larger payments are more likely to be budgeted to current asset accounts and saved while smaller payments are more likely to be allocated to current income accounts and consumed (Thaler and Shefrin 1981; Shefrin and Thaler 1988; Thaler 1992; Thaler 1994; Beverly, McBride, and Schreiner 2003). Shefrin and Thaler (1988) suggest a primary reason for this treatment is because a large payment enables recipients to engage in a considerable spending binge while still saving most of the payment.

Expanding on the effect of payment size, subsequent research examines whether payment timing also impacts how income is budgeted between mental accounts. Specifically, this literature predicts and finds a series of smaller periodic payments are generally spent to a greater extent than an economically equivalent, but nominally larger, lump sum payment. Consistent with these predictions, Shefrin and Thaler (1998) find MBA students anticipate spending more of a hypothetical bonus that is paid as a series of $200 payments over 12 months rather than as a single $2,400 payment. Chambers and Spencer (2008) use a within-subject experiment and similarly find participants indicate they would allocate more money to savings when receiving a tax refund as an annual payment rather than monthly payments. Thus, prior research seems to suggest lump sum payments (which engender more saving and less spending than periodic payments) are better suited to achieve the policy goals of stimulus transfers while periodic payments are better suited to achieve the policy goals of subsidy transfers.

Coronado et al. (2005) and Sahm et al. (2012) both sought to examine the role of payment timing on how government transfers are spent or saved, but neither finds results consistent with theory. However, both studies rely on data that potentially conflates payment timing and delivery method. The data used in these studies records self-reported recollections of spending and saving in response to real world government transfers that differ in payment timing. Importantly, the real-world lump sum transfers examined in these studies were paid via standalone direct checks while the periodic transfers were paid as larger paychecks such that the periodic payments were combined with other sources of income. As the transfers examined in these studies differ both in payment timing and delivery method, the lack of results consistent with the behavioral life-cycle is difficult to interpret. Accordingly, we consider the role of delivery method as a moderator of the effect of payment timing.

Prior research on the behavioral life-cycle does not appear to explicitly examine the role

of delivery method on allocation of income to current income or current asset accounts but has examined the role of income source. Within a particular government transfer program, the actual source of transfers is held constant (e.g., all payments are from federal/state/local governments), but the perceived source of the transfer may depend on delivery method, which could then impact how the transfer is used. Specifically, we posit that combining a government transfer with funds from other sources (i.e., funds not associated with the government transfer, such as wages or a tax refund) may result in recipients treating the transfer as if it is also from that other source rather than from the government. For example, a periodic combined transfer may be treated as if it is labor income because it is received concurrently as a part of the recipient's paycheck.

With respect to payment source, the literature generally agrees income from some sources is more likely to be allocated to current income accounts and spent while income from other sources is more likely to be allocated to current asset accounts and saved. For example, Shefrin and Thaler (1988, 620) indicate "labor income is encoded into current income (I), while capital income (with the possible exception of dividend income [see Shefrin and Statman 1984]) is encoded into the [current asset] account upon arrival." Similarly, Epley, Mak, and Idson 2006 show recipients spend more income (presumably indicating that the income was budgeted to the current income accounts) when it is described as a bonus rather than as a rebate. Bobek, Hatfield, and Wentzel (2007) find results that suggest tax refunds are mentally budgeted and used in a different manner than income received on paychecks.

The above studies show source clearly impacts how income is allocated between current asset and current income accounts. However, Baker, Nagel, and Wurgler (2007, 272) notes "the underlying psychology behind this sort of mental accounting is an important open question" while studying how dividends are allocated between current income and current asset accounts. Given this dearth of understanding, specifically predicting how the budgeting process is

impacted by source is difficult. Accordingly, we instead focus on how payment delivery method moderates the role of payment timing rather than predicting how it directly influences whether a transfer is allocated to a current income or a current asset account.

We posit the impact of payment timing will be obscured when government transfers are combined with income from other sources. Prior research examining how expenditures (rather than income) are allocated to mental accounts generally concludes individuals develop mental budgeting heuristics (e.g., based on how representative or typical the expenditure is, Heath and Soll 1996) and suggests the budgeting process may occur with minimal thought or effort when the expenditure has been previously experienced and/or occurs frequently (Cohen and Basu 1987; Henderson and Peterson 1992). To the extent combined government transfers are perceived as coming from a source other than the transfer program, existing budgeting heuristics based on source may conflict with or override considerations related to payment timing.

In contrast, standalone payments cannot be misperceived as coming from different sources as they come directly from the government. As such, we posit transfers delivered as standalone payments are not as strongly associated with existing mental budgeting heuristics based on source because they are received with less frequency than other sources of income and recipients have likely had less of a chance to develop such heuristics. Moreover, the perceived source of the transfer should be consistent across payment timing for standalone transfers (i.e., their source is clear because they are not combined with other funds), while combined periodic and lump sum government transfers may be perceived as coming from sources other than the transfer program depending on what they are combined with.

Collectively, the discussion above leads us to hypothesize that the effect of perceived payment delivery method may conflict with the effect of payment timing when government transfers are combined with other income. We specifically predict periodic government transfer

payments are more likely to be allocated to current income accounts and spent while lump sum government transfer payments are more likely to be allocated to current asset accounts and saved, and this payment timing effect will manifest more prominently when government transfers are standalone rather than combined. Stated formally:

> **H1: After controlling for payment delivery method, individuals will spend a larger amount of a government transfer when it is paid via multiple periodic payments as compared to an economically equivalent lump sum payment.**
>
> **H2: The effect of payment timing will be stronger for standalone government transfers than for combined government transfers.**

III. METHODS

To test our hypotheses, we conduct a 2 x 2 between-participants experiment in which we randomly assign real U.S. taxpayers solicited through Amazon Mechanical Turk into four treatment conditions. Participants complete a multi-round simulation that is divided into three "years" of four rounds each for a total of 12 rounds. In each round, participants earn Lira (£), the experimental currency, by completing an earnings task and then use the experimental currency to pay for various living expenses (rent, utilities, groceries, and miscellaneous expenses) for that round. At the end of each round, participants decide how to allocate any Lira that remains after paying the living expenses between savings and spending. In years one and two, participants complete a similar process of earning Lira, paying expenses, and allocating remaining Lira between spending and savings for each round within the year.[6] In the third year of the simulation, participants are given a government transfer of £900 in addition to their regular earnings. We manipulate payment timing (multiple small payments or one lump sum payment) and payment delivery method (standalone or combined). Participants receiving periodic payments get either four separate checks in the standalone condition or four withholding decreases in the combined

[6] While the process is the same for each round and year, we do slightly vary the amount of earnings and cost of expenses between years to add realism to the simulation.

condition, each time resulting in an extra £225 (or £900 total). Participants in the lump sum conditions receive either one separate check for £900 in the standalone condition or a one-time increase of £900 in their tax refund in the combined condition.

Overview and Procedures

Appendix 1 provides an overview and additional specific details about the experiment. After answering three screening questions, participants are informed they will complete a multi-round simulation.[7] Prior to starting the first round, participants are provided with background information about the experiment, including information about the tasks they will complete and how they will be compensated. Next, they are provided with a brief overview of the study and their compensation. Specifically, participants are informed that they will complete a multi-round simulation and will receive a $2.00 flat fee for participating in the study along with a bonus of no less than $2.00 based on their decisions during the simulation. Moreover, participants are told they must correctly respond to all the attention check questions to receive their bonus. Bonuses (if earned) could range from $2.30 to $4.60. Next, participants are told the simulation will involve an earnings task in which they accumulate Lira and then use the Lira to pay for living expenses and taxes prior to allocating any remaining Lira between spending in savings. After reading this information, participants are required to correctly complete a two-question simulation quiz to confirm they read the information carefully. If they miss either question, they are given one opportunity to review the material and repeat the quiz before they are dismissed from the study.

[7] For the first two screening questions, participants had to indicate they are a U.S. citizen or a permanent resident and at least 18 years old. For the final screening question, potential participants were presented with an image of a 4x4 grid of integers between 1 and 10 (inclusive) and were asked to select all odd numbers that are not on the bottom row of the grid. This question was designed to screen out "bots" (i.e., non-human respondents), individuals without sufficient understanding of the English language to interpret the question, and inattentive individuals. In addition, we required participants to have completed at least 1,000 HITs with a 95 percent or better approval rating and be on the Cloud Research approved participants list. Finally, due to the nature of the tasks in the experiment, we prevented any participants from attempting to complete the study using a mobile device.

After completing the simulation quiz, participants receive detailed information about how their bonus is computed. Participants are told the total of the Lira they allocate to spending and savings during the simulation will be converted to U.S. dollars at the conclusion of the study. Lira allocated to spending are converted to dollars at a rate of $0.75 for every £1,000 and this bonus is paid within 24 hours of the completion of the study while Lira allocated to savings are converted to dollars at a rate of $1.50 for every £1,000 and this bonus is paid 30 days after the completion of the study. This bonus scheme is intended to replicate the real-world incentives for spending and savings: spending results in immediate gratification while savings generates a return but delays gratification.[8] After this information is presented to the participants, they then answer four comprehension check questions about the calculation of their bonus. As with the simulation quiz, participants who respond incorrectly are given one opportunity to review the instructions and repeat the comprehension quiz prior to being dismissed from the study.

Finally, participants are required to complete a simplified practice round to ensure they understand how to complete the various tasks in the simulation. In the practice earnings task, participants are provided instructions and are required to complete a single slider task in which they set a slider to a predetermined number (the actual earnings task in the simulation consists of two such sliders). The selectable values on the slider range from 0 to 100. Once the participant successfully completes the practice earnings task, they next complete a practice expense payment task. In this task, participants are informed of their after-tax net earnings from the task and provided with a list of expenses they must pay (rent, utilities, groceries, and miscellaneous). Participants then pay the expenses by entering the amount of each expense into a corresponding

[8] We drew from experimental economics to create an incentive structure that incorporated both the delayed gratification associated with saving and the economic results of their decisions. Different parameterization could have influenced the proportion of saving to spending; however, the current study is interested in how government transfers influence spending patterns and focuses on differences across conditions. Any effect of parametrization of bonuses should be held constant across conditions.

box on the screen and are not allowed to continue until the total of the expenses is correct. Next, participants enter the total of the expenses in a second set of boxes to compute how much remaining Lira they have at the end of the practice round (by entering the total of expenses, the experimental instrument automatically calculates their remaining Lira). To avoid confusion related to their bonus, we do not ask participants to actually allocate their remaining Lira between spending and savings for the practice task.

Following the completion of the practice tasks, participants begin the simulation in earnest. The total simulation consists of 12 rounds divided across three years.[9] In each round, participants complete the earnings task, the expense payment tasks, and then allocate any remaining Lira between spending and savings.[10] Beyond repeating these tasks across the 12 rounds, the simulation contains three key features. First, at the end of each year, participants are provided with a summary of the year including the total of their allocations as well as information about their annual tax filing. Specifically, participants have taxes withheld on their behalf during the earnings task that exceeds their annual tax liability and are informed that they will receive a relatively small tax refund in the first round of the following year (all tax refunds—except those that include a government transfer—are between £114 and £123). Second, after round four of year two, participants are informed that they will receive a £900 government transfer in year three. Finally, all participants receive their government transfer in year three as described in the next section. To conclude the simulation, participants view a summary of year three and then make one last allocation decision with respect to the tax refund they would have

[9] We do not disclose the total number of rounds to participants.

[10] To add realism and to prevent participants from anchoring on particular allocation amounts, the amount earned and expenses incurred vary slightly form round to round. All participants earn the same total amount of Lira from the earnings task and pay the same amount of expenses each round and throughout the entire study. Moreover, quantities in the simulation are targeted such that earnings from the earnings task (though not necessarily total income in a particular round, which depends on condition) exceed total expenses in each round by between 100 and 200 Lira.

received in the first round of year four (there is no actual year four in the simulation). After making this final allocation decision, participants respond to manipulation and attention check questions and a post experimental questionnaire.

Manipulations, Dependent Variable, and Manipulation/Attention Checks

The manipulations are introduced after year two and take place in year three. Participants are all told "Towards the end of Year 2, the government passed legislation authorizing a tax-free government transfer payment to individuals like you. You qualify and will get a £900 government transfer." After reading this, all participants are required to acknowledge that they will receive the £900 transfer in year three to confirm their awareness of the transfer. Participants are then randomly assigned to conditions and receive additional information specific to their condition. Participants in the lump sum conditions receive their transfer as a single £900 payment that is either standalone or combined with their tax refund for year two (paid in round one of year three). Participants in the periodic conditions receive their transfer in a series of four £225 payments that are either standalone or combined with their earnings from the earnings task (paid in each round of year three). Accordingly, all participants are informed of their transfer at the same time and receive their transfer entirely within year three.

Our dependent variable is *Y3SPEND*, which is the amount of Lira each participant allocates to spending in the third year of the simulation. To control for the innate spending and savings preferences of each participant, we also use *Y2SPEND* as a control variable. *Y2SPEND* is the amount of Lira each participant allocates to spending in the second year of the simulation.[11] To ensure the manipulations were salient to participants, we ask two manipulation check questions. First, participants are required to indicate whether they received the transfer as a lump

[11] The results of the experiment are inferentially unchanged if we instead control for each participant's allocation to spending in the first year, or the average allocated to spending in the first two years. Moreover, the results are inferentially unchanged if we instead use percentage of income spent during year three as the dependent variable.

sum payment or as a series of four periodic payments. Second, participants are required to indicate whether the transfer was received as a separate check/check or combined with their tax refund/paycheck. Additionally, we ask an attention check question in which participants are directly asked to select "slightly agree" to show they are reading the questions carefully.

IV. RESULTS

Participants

Participants were recruited using the MTurk platform and Cloud Research to complete the study.[12] A total of 22 participants responded incorrectly to one or both manipulation check questions and none of the participants failed the attention check question. These participants were not removed from the dataset, but all conclusions drawn from the data are inferentially unchanged if these participants are removed. The final sample contains 502 observations.[13] On average, participants took just over 26 minutes to complete the experiment and the average bonus awarded was $3.96 in addition to the flat fee of $2.00 (thus, average total compensation was $5.96, or about $13.75 per hour) A summary of the participants' demographic data is presented in Table 1, Panel A.

As shown in Table 1, most participants indicated they were between 25-44 years of age with income less than $74,999. Half of the participants were male, while 48 percent indicated they are female (two percent indicated other or prefer to not respond), and 67 percent have completed a college degree. While not identical to the U.S. population, these values appear to be consistent with prior tax research using MTurk participants (e.g., Austin, Bobek, and LaMothe

[12] A total of 763 individuals clicked the link to participate in the study. Of these potential participants, a total of 224 participants were screened out from participating: two for not meeting either the age or residency requirements, 31 for using a mobile device, and 191 failed to correctly complete the CAPTCHA challenge. Additionally, 21 participants were dismissed for failing to correctly answer both questions about the simulation (the simulation quiz) and an additional 16 participants were dismissed from the study for failing to correctly answer all four questions about how their bonus was determined (the comprehension quiz).

[13] Prior to collecting data, this study was submitted to and approved by the Institutional Review Board.

2020). In addition, we also collected three additional variables with information pertinent to the current research question. The majority of participants (87 percent) indicated that they received a stimulus payment in either 2020 or 2021 while 33 percent of participants indicated that they received the EITC on their most recent tax return which suggests most participants in our study have at least some experience dealing with government transfers. Finally, 68 percent of participants indicated that they received a tax refund on their most recent tax return, suggesting tax refunds are not an unusual occurrence for participants in our sample.

Initial Analysis

Prior to testing our hypotheses, we present descriptive analysis of our data. Table 1, Panel B provides an overview of the spending behavior of participants in each condition both in terms of the average amount and percentage of remaining Lira participants spend in each of the three years of the simulation. Panels C and D present the average amount and percentage, respectively, of Lira participants in each condition allocated to spending in each round (including from the tax refund that would have been paid in year four round one). Figure 1 displays the same information as Panel C graphically. As shown in Table 1, the amount of Lira spent across conditions was relatively constant in both year one and year two. Untabulated ANOVA analyses indicate the amount spent by participants did not differ across conditions in year one or year two ($F = 0.13$, df = 3, $p = 0.973$ and $F = 0.11$, df = 3, $p = 0.957$; respectively).[14] Further untabulated analysis indicates the amount allocated to spending by participants did not differ across conditions in any single round in years one or two (lowest $p = 0.619$ for year one round one).

As a final preliminary analysis, we examine the general change in Lira allocated to spending over time. From year one to year two, we find participants significantly increase Lira allocated to spending by an average of £76 ($t = 18.40$, $p < 0.001$, untabulated). After receiving

[14] Unless otherwise noted, all tests are two-tailed.

the government transfer, participants significantly increased the total Lira allocated to spending by an average of £215 (t = 16.89, p < 0.001, untabulated). Interestingly, the increase in spending only accounts for approximately 24 percent of the amount of the government transfer payment. Additionally, this amount is similar to findings of past research which examines real world transfers (e.g., Broda and Parker 2014; Coibion et al. 2020; Karpman et al. 2021; Parker et al. 2022; Shapiro and Slemrod 2009). Moreover, the percentage of Lira allocated to spending significantly decreased after participants received the government transfer (t = -6.85, p < 0.001, untabulated). These findings suggest participants may have largely mentally allocated their government transfers to current asset accounts. While not necessarily predicted, this finding is consistent with the behavioral life-cycle model which suggests windfalls and bonuses are generally spent at a lower rate than regular income.

Hypothesis Testing

The hypotheses predict 1) *Y3SPEND* will be significantly larger in the periodic condition than in the lump sum condition, and 2) the difference in *Y3SPEND* will be significantly larger in the standalone condition than in the combined condition. The pattern of means for the dependent variable are displayed graphically in an interaction plot in Figure 2. To formally analyze the hypotheses, we conduct an ANCOVA analysis in which the dependent variable is *Y3SPEND*, and the independent variables are *TIME*, *DELIVERY*, and the two-way interaction between *TIME* and *DELIVERY*. *TIME* is an indicator for whether participants are in the lump sum or periodic condition and *DELIVERY* is an indicator for whether participants are in the standalone or combined condition. As previously discussed, we also include *Y2SPEND* as a covariate. Table 2, Panel A displays the means for *Y3SPEND* while Table 2, Panel B displays the results of the ANCOVA analysis. Unsurprisingly, *Y2SPEND* is very significantly associated with the dependent variable (F = 3,217.74, p < 0.001). Results from the ANCOVA analysis are not

consistent with H1 as the main effect of *TIME* is insignificant ($F = 1.03$, $p = 0.311$) even after controlling for *DELIVERY*. However, the two-way interaction between *TIME* and *DELIVERY* is significant ($F = 5.97$, $p = 0.015$). This significant interaction, along with the pattern of means presented in Figure 2/Table 2, Panel A, is consistent with the second hypothesis. Moreover, a significant interaction indicates the main effects should not be interpreted directly. Accordingly, we use tests of simple effects reported in Table 2, Panel C to evaluate the hypotheses.

As a starting point for the tests of simple effects, we first estimate a post-hoc test intended to replicate the results of previous studies that examine the role of payment timing on the use of government transfers. Recall Coronado et al. (2005) and Sahm et al. (2012) both use data that confounds payment timing and delivery method as the lump sum government transfers were made as standalone payments while the periodic government transfers were combined with paychecks. Accordingly, we first compare the average Lira allocated to savings between the combined periodic condition and the standalone lump sum condition. Consistent with prior research, we find participants in the combined periodic condition allocate £18 more to spending than participants in the standalone lump sum condition. However, the post hoc test indicates this difference is not statistically distinguishable from zero ($t = -0.07$, $p = 0.945$). This result replicates the findings of Coronado et al. (2005) and importantly highlights the potential conflating or counteracting effects inherent to examining a simultaneous manipulation of payment timing and payment delivery method.

Next, we examine the simple effects of government transfer payment timing conditioned on payment delivery method. Consistent with H1, we find payment timing is significantly associated with spending when payment delivery method is held constant as a standalone payment ($t = 2.45$, $p = 0.007$, one-tailed). Participants in the periodic/standalone condition spend £490 on average while participants in the lump sum/standalone condition spend only £421, a

difference of £69 or approximately 7.67 percent of the transfer. While this amount could seem somewhat trivial, this increase would have translated to over $63 billion in additional spending and economic stimulus across the $800 billion of economic impact payments in the U.S. during the COVID-19 pandemic. However, we find no evidence of a payment timing effect when delivery method is held constant as a combined payment (t = -1.01, p = 0.314) as participants in the periodic/combined condition spend £439 on average while participants in the lump sum/combined condition spend £434 on average.[15] Results collectively provide strong support for H2 and indicate the effect of payment timing is moderated by payment delivery method.

To ensure the robustness of our analyses, we examine whether controlling for any of the demographic variables reported in Table 1, Panel A significantly changes any of the results. Untabulated simple correlations reveal several of the demographic variables are significantly related to *Y3SPEND* including gender (p = 0.098), age (p = 0.049), income (p = 0.002), experience filing tax returns (p = 0.051), whether or not the recipient received an Earned Income Tax Credit on their most recent federal income tax return (p < 0.001), and whether or not the recipient received a tax refund on their most recent federal income tax return (p = 0.013). Moreover, all of these except for gender (p = 0.172) were significantly correlated (all p < 0.100) with the change in spending from year 2 to year 3 (suggesting the correlation with *Y3SPEND* would persist even when controlling for *Y2SPEND*). All of these variables except for refund status are negatively correlated with the change in spending from year one to year two, suggesting older, wealthier individuals with more tax filing experience and those who receive the Earned Income Tax Credit spend less of the government transfer they receive. Participants who reported recently receiving a tax refund rather than owing additional taxes spend more of the government transfer. However, results for the hypotheses tests are inferentially unchanged when

[15] We conduct a two-tailed test because the difference in means is directionally inconsistent with the hypothesis.

these demographic variables are included as covariates in our ANCOVA analysis.

Finally, we reexamine the results after considering whether participants responded only to the bonus structure and not the simulation. Participants could wait 30 days effectively elect to double their bonus by saving all remaining Lira while participants who wanted to accelerate their bonus could spend all remaining Lira. Given this bonus structure, participants who spend or save all remaining Lira may have been reacting to the bonus structure rather than the simulation. Untabulated analysis indicates 123 participants (24.5 percent) chose to always save while 24 participants (4.78 percent) chose to always spend. However, untabulated analysis indicates the result of the hypothesis tests are inferentially unchanged if these participants are removed.

Split-Phase Analysis of Year Three

In the interest of more deeply examining our findings, we separately examine the behaviors of our participants in two distinct phases of year three. The first phase occurs in round one wherein participants in the lump sum conditions receive a larger amount of Lira to allocate than participants in the periodic conditions. The second phase occurs in rounds two through four wherein participants in the periodic conditions receive a larger amount of Lira to allocate than participants in the lump sum conditions who have already received their entire government transfer. We note one particularly important consideration for this analysis is that the payment timing manipulation represents a payment size effect within each of these phases.[16] While the total Lira available to participants to allocate in year three is constant across conditions, the amount of Lira available differs between conditions within each phase (round 1 vs. rounds 2-4). As such, we examine the average percentage of Lira spent in each phase to control for

[16] Due to the nature of the experiment, *TIME* more accurately represents a payment size manipulation in the context of the split-phase analysis because the total amount of government transfer received within each phase is not the same across conditions. Specifically, the *TIME* manipulation compares participants who received a large transfer versus a small transfer in the first phase (i.e., round 1), and compares participants who receive multiple small transfers versus no transfers in the second phase (i.e., rounds 2 through 4).

differences in amount of Lira available to allocate as the dependent variable (*Y3R1SPEND%* and *Y3R234SPEND%*, respectively).

Results are presented in Table 3. Panels B and C shows results related to *Y3R1SPEND%* are largely consistent with both hypotheses: we observe a significant effect of *TIME* ($F = 6.38$, $p = 0.012$) which is moderated by *DELIVERY* ($F = 4.14$, $p = 0.042$). Follow up tests confirm a significant *TIME* effect when transfers are standalone ($t = 3.23$, $p < 0.001$) but not when they are combined ($t = 0.35$, $p = 0.729$). That is, participants spent a significantly larger percentage of the transfer in Year 3, Round 1 when it was a standalone periodic transfer versus a standalone lump sum transfer, but no spending difference existed when transfers were combined. We also find no effect of *DELIVERY* when payments are lump sum ($t = 0.93$, $p = 0.351$) but a marginally significant effect when payments are periodic ($t = 1.95$, $p = 0.052$). These results suggest the moderation effect of delivery method operates by impacting how periodic payments are mentally budgeted, but not impacting how lump sum payments are budgeted.

Results in Table 3, Panels D and E examine behaviors in rounds two through four of year three when participants in the periodic conditions continue to receive government transfers but participants in the lump sum condition do not. In this analysis, we again observe a significant effect of *TIME* ($F = 17.84$, $p < 0.001$), but the simple effect estimates in Panel E indicate the effect is in the opposite direction. This is somewhat unsurprising as participants in the periodic conditions are now receiving larger payments than participants in the lump sum conditions and the size effect would be in the opposite direction. We again observe a significant interaction effect ($F = 5.53$, $p = 0.019$), which now is accompanied by a marginally significant effect of *DELIVERY* ($F = 3.14$, $p = 0.077$). Follow up tests in Table 3, Panel E suggest an interesting interaction pattern in the last three rounds of year three: the *TIME* effect is now significant for the combined conditions ($t = 4.64$, $p < 0.001$) instead of the standalone conditions ($t = 1.33$, $p =$

0.185). These results suggest participants receiving a standalone periodic transfer behave in a manner similar to participants who receive no government transfer, while participants receiving a combined periodic transfer do behave differently. Finally, we observe no effect of payment delivery method when payments are lump sum (t = -0.41, p = 0.682) but a marginally significant effect when payments are periodic (t = 2.92, p = 0.004), which again suggests the moderation effect of delivery method operates by impacting how periodic payments are mentally budgeted.

Additional Comparative Analysis and Discussion of Policy Goals

Given the practical significance of our study to policymakers, we conduct two additional analyses to further examine our results. First, given the relatively similar means for average Lira allocated to spending across the periodic combined condition and the two lump sum conditions, we conduct an untabulated one-way ANCOVA with just these three conditions, an indicator for condition, and *Y2SPEND* as a covariate. Results from this analysis indicate the Lira allocated to spending does not differ significantly across these three conditions (F = 0.58, p = 0.561, untabulated). Moreover, untabulated pairwise comparisons within this ANCOVA indicate none of the three conditions differ significantly from each other. Second, we compute a post-hoc contrast to compare the average Lira allocated to spending in the periodic standalone condition to that of the remaining three conditions based on the ANCOVA estimated in Table 2, Panel B. In this test, we find participants in the periodic standalone condition allocated significantly more Lira to spending than participants in the other three conditions (t = 2.65, p = 0.008, untabulated).

These results collectively suggest policymakers may be able to structure government transfer programs to match policy goals. In particular, the results suggest transfer programs may best achieve the policy goal of stimulating the economy by structuring the transfer payments as standalone periodic checks sent directly to recipients. Interestingly, none of the examples of stimulus programs listed in Exhibit 1 are distributed using periodic standalone payments.

Stimulus programs in the U.S. have primarily relied on standalone lump sum payments and sometimes combined periodic payments. In contrast, periodic combined payments and lump sum payments may be better suited to achieve the policy goals associated with subsidy programs. As the use of government transfers across these three timing and delivery method combinations are not significantly different, policy makers could instead differentiate these based on other characteristics. For example, periodic combined payments may be a better fit with policy objectives than lump sum payments because funds are distributed more frequently to recipients.

V. CONCLUSION

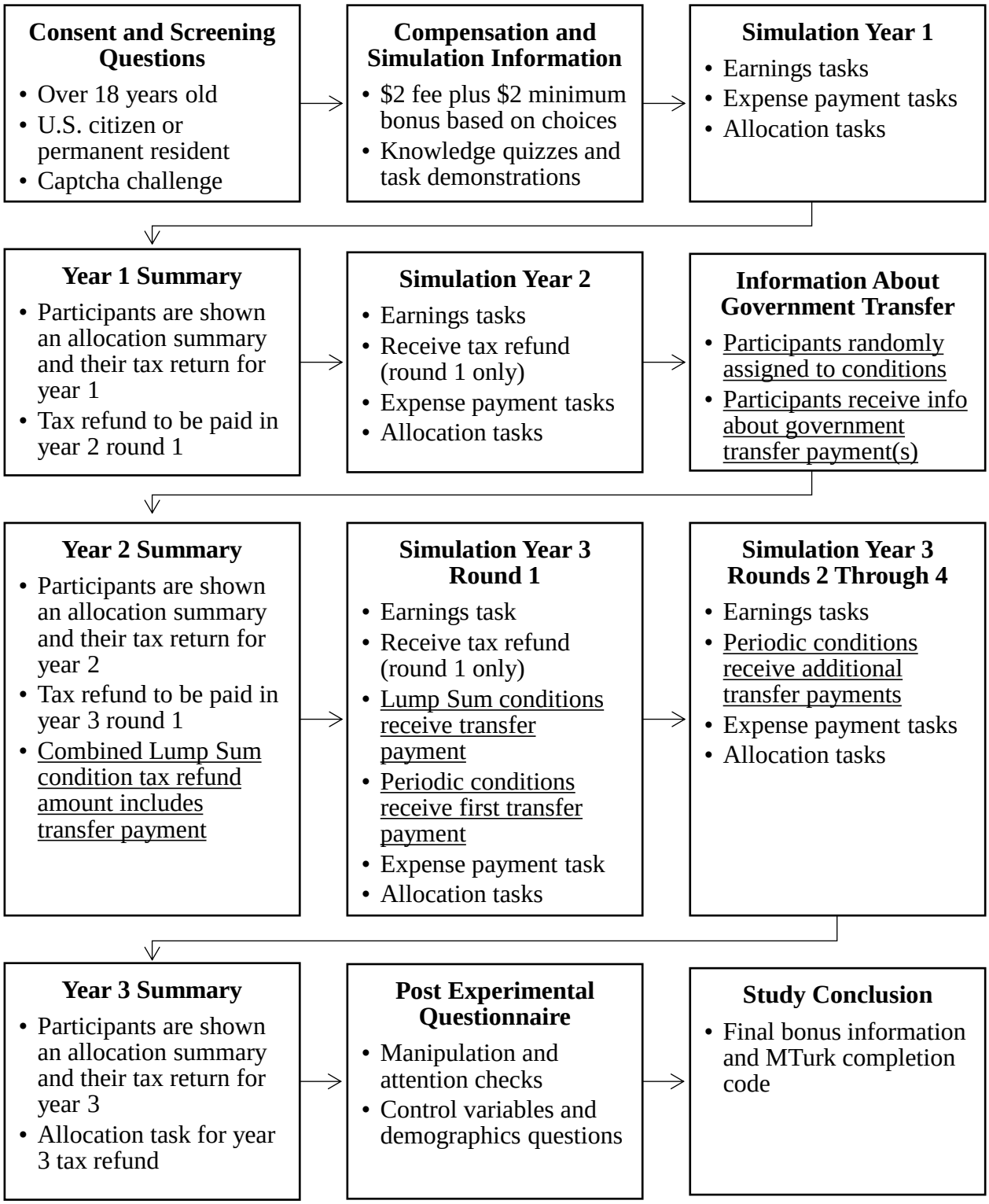
Based on the results of our experiment, we conclude timing of government transfers can impact the way recipients of the transfers use those funds, but the impact of timing is moderated by delivery method. Specifically, we find evidence that recipients of government transfers tend to spend a greater portion of transfers that are paid as periodic payments but only when those payments are delivered as standalone payments and are not combined with other sources of income. While prior studies make similar predictions, our results suggest those studies may fail to find theoretically consistent results because they do not account for the moderating role of delivery method on the effect of timing as it pertains to the use of government transfers.

This study contributes to both theory and practice. First, we contribute to theory on mental accounting and the behavioral life-cycle model by showing theory-consistent results despite the counter-theoretical results of prior research. Moreover, we extend the behavioral life-cycle model by highlighting a moderator that eliminates the role of payment timing – an effect that is well established in the literature. Second, this study provides insight into the effects of payment characteristics on the societal impact of government transfers, which should be of immense interest to policymakers. Governments transfer billions of dollars to individuals every year, and these transfers can often be linked with implicit goals related to spending and savings

by the recipient. Stimulus transfers are only effective to the extent they engender an increase in current spending while subsidy transfers are generally more effective when they engender an increase in both spending and savings. Although transfer programs are widespread across both federal and state governments around the world, payment characteristics are far from uniform. Our results provide powerful insights to policymakers who can use this variation in payment characteristics to target the policy goals of various transfer programs more effectively.

APPENDIX 1
Experimental Materials

Panel A: Summary of Experimental Procedures



Panel B. Summary of Income and Expenses by Round

	1-1	1-2	1-3	1-4	2-1	2-2	2-3	2-4	3-1	3-2	3-3	3-4	4-1
Earnings (gross)	£1,830	£1,900	£1,840	£1,850	£1,880	£1,960	£1,950	£1,920	£1,960	£1,960	£2,040	£2,060	£0
Income Taxes Withheld	-£201	-£209	-£202	-£204	-£207	-£216	-£211	-£211	-£218	-£216	-£224	-£227	£0
Earnings (net of taxes)	£1,629	£1,691	£1,638	£1,646	£1,673	£1,744	£1,739	£1,709	£1,742	£1,744	£1,816	£1,833	£0
Rent	-£800	-£800	-£800	-£800	-£850	-£850	-£850	-£850	-£900	-£900	-£900	-£900	£0
Utilities	-£229	-£238	-£230	-£231	-£235	-£245	-£240	-£240	-£248	-£245	-£255	-£258	£0
Groceries	-£261	-£271	-£263	-£264	-£269	-£280	-£274	-£274	-£283	-£280	-£291	-£294	£0
Miscellaneous	-£238	-£205	-£227	-£249	-£173	-£180	-£231	-£166	-£172	-£129	-£217	-£210	£0
Remaining Earnings	£101	£177	£118	£102	£146	£189	£144	£179	£139	£190	£153	£171	£0
Tax Refund (no government transfer)	£0	£0	£0	£0	£114	£0	£0	£0	£119	£0	£0	£0	£123
Lump Sum Stimulus	£0	£0	£0	£0	£0	£0	£0	£0	£900	£0	£0	£0	£0
Remaining Income	£101	£177	£118	£102	£260	£189	£144	£179	£1,158	£190	£153	£171	£123
Periodic Stimulus	£0	£0	£0	£0	£0	£0	£0	£0	£225	£225	£225	£225	£0
Remaining Income	£101	£177	£118	£102	£260	£189	£144	£179	£483	£415	£378	£396	£123

Panel B displays numbers used in the earnings task, expense payment task, and income allocation task for each round for participants in the lump sum and periodic conditions. These figures are invariant across *DELIVERY* conditions as only how the funds are received changes between the standalone and combined conditions.

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EXHIBIT 1
Payment Delivery Method and Payment Timing of Selected Transfer Programs

	Lump Sum	Periodic
Combined	**Tax Refund** • Earned Income Tax Credit (Subsidy) • Child Tax Credit (Subsidy) • Working Income Tax Benefit (Canada) (Subsidy)	**Larger Paychecks** • Making Work Pay Credit (Stimulus) • Earned Income Tax Credit (1979 to 2010) (Subsidy)
Standalone	**Direct Check** • Economic Stimulus Act Rebate Checks (Stimulus) • 2020/2021 Economic Impact Payments (Stimulus)	**Direct Checks** • Advance Child Tax Credit (2021) (Subsidy) • Supplemental Security Income Program (Subsidy) • Revenu de Solidarité Active (France) (Subsidy) • Universal Credit (U.K.) (Subsidy) • Working Family Payment Program (Ireland) (Subsidy)

Exhibit 1 lists and categorizes examples of various government transfer programs based on the timing (lump or periodic) and delivery method (combined or standalone) used to distribute payments. Moreover, the exhibit also notes whether each program is regarded as a stimulus or subsidy program.

FIGURE 1
Average Lira Allocated to Spending by Condition and Round

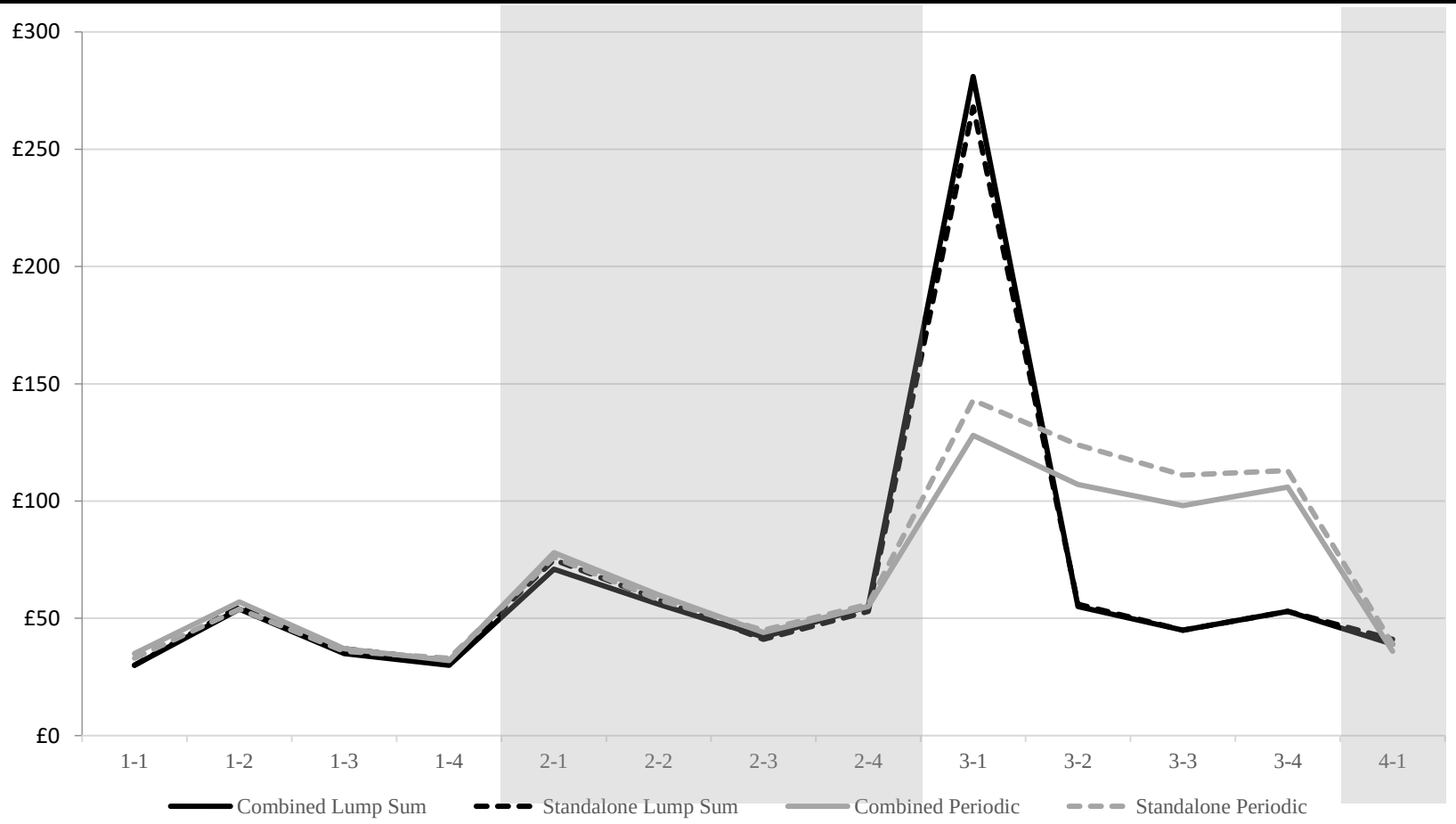

Figure 1 graphically depicts the averages of the amount of Lira allocated to spending for each of the 12 rounds as well as for the tax refund from year three (round "4-1") in the experiment, by condition. These averages are also reported in Table 1 Panel C. Participants received tax refunds in rounds 2-1, 3-1, and 4-1. Participants in the Combined Lump Sum condition received a single transfer payment in round 3-1 as part of their tax refund while participants in the Standalone Lump Sum condition received a single transfer payment in round 3-1 as a separate check. Participants in the Combined Periodic condition received their transfer divided across four payments in each round of year three (rounds 3-1 through 3-4) paid as a part of their paychecks from the earnings task while participants in the Standalone Periodic condition received their transfer divided across four payments in each round of year three paid as separate checks. All participants received information about the government transfer and how it would be paid after rounds 2-4.

FIGURE 2

Interaction Plot for *Y3SPEND*

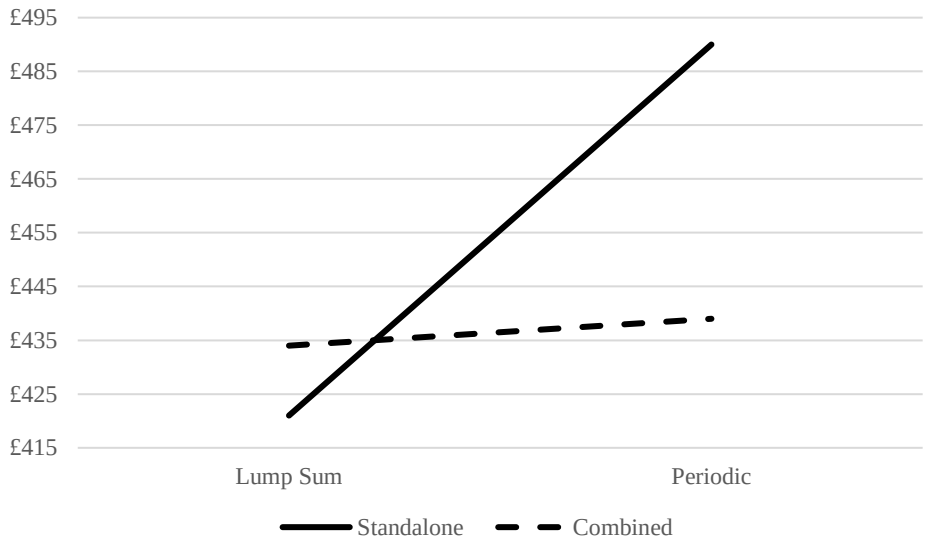

Figure 2 graphically depicts the average total amount participants allocated to spending in year three by condition. Participants in the Combined Lump Sum condition received a single transfer payment in round 3-1 as part of their tax refund while participants in the Standalone Lump Sum condition received a single transfer payment in round 3-1 as a separate check. Participants in the Combined Periodic condition received their transfer divided across four payments in each round of year three (rounds 3-1 through 3-4) paid as a part of their paychecks from the earnings task while participants in the Standalone Periodic condition received their transfer divided across four payments in each round of year three paid as separate checks.

TABLE 1
Demographic and Descriptive Statistics

Panel A. Sample Demographics

	Sample (n=502)	US Population
Gender		
Male	50%	49%
Female	48%	51%
Other	1%	
Prefer Not to Respond	1%	
Age		
Less than 25	3%	9%
25 to 34	29%	19%
35 to 44	37%	17%
45 to 54	16%	17%
55 to 64	10%	17%
65 or older	5%	21%
Education		
Less than high school	1%	11%
High school	11%	27%
Some college courses	21%	20%
College graduate	51%	29%
Post-graduate degree or courses	16%	13%
Income		
Less than $25,000	14%	18%
$25,000 to $49,999	25%	21%
$50,000 to $74,999	22%	17%
$75,000 to $99,999	16%	13%
$100,000 or more	20%	31%
Prefer not to respond	3%	
Received Stimulus in 2020 or 2021		
Yes	87%	
No	10%	
Prefer not to respond	3%	
Received EITC (most recent tax return)		
Yes	33%	
No	50%	
I don't know/remember	17%	
Received Refund (most recent tax return)		
Yes, Refund of Less Than $2,000	52%	
Yes, Refund of Greater Than $2,000	16%	
No, Owed Additional Taxes	25%	
I don't know/remember	7%	

Panel A provides demographic information for participants who completed the experiment. Frequencies reported for the US Population are from the US Census Bureau 2020 estimates. US Population frequencies for age and education are based on the population over age 20 and 25, respectively.

TABLE 1 (Continued)

Panel B. Average Amount (Percentage) Allocated to Spending by Condition and Year (n=502)

	Year 1	Year 2	Year 3
Combined Lump Sum	£150 (30%)	£224 (29%)	£434 (26%)
Standalone Lump Sum	£154 (31%)	£228 (30%)	£421 (25%)
Combined Periodic	£161 (32%)	£237 (31%)	£439 (26%)
Standalone Periodic	£155 (31%)	£235 (30%)	£490 (29%)
Overall	£155 (31%)	£231 (30%)	£446 (27%)

Panel B displays the average amount of remaining income allocated to spending in each year within each condition. Allocation from year 4 (i.e., from the tax refund from year 3) is excluded. Participants in the Combined Lump Sum condition received a single transfer payment in round 3-1 as part of their tax refund while participants in the Standalone Lump Sum condition received a single transfer payment in round 3-1 as a separate check. Participants in the Combined Periodic condition received their transfer divided across four payments in each round of year three (rounds 3-1 through 3-4) paid as a part of their paychecks from the earnings task while participants in the Standalone Periodic condition received their transfer divided across four payments in each round of year three paid as separate checks.

Panel C. Average Amount Allocated to Spending by Condition and Round (n=502)

	1-1	1-2	1-3	1-4	2-1	2-2	2-3	2-4	3-1	3-2	3-3	3-4	4-1
Combined Lump Sum	£30	£54	£35	£30	£71	£56	£42	£55	£281	£55	£45	£53	£39
Standalone Lump Sum	£30	£55	£37	£32	£75	£58	£41	£53	£268	£56	£45	£53	£41
Combined Periodic	£35	£57	£37	£32	£78	£60	£44	£55	£128	£107	£98	£106	£36
Standalone Periodic	£33	£54	£36	£33	£76	£58	£45	£56	£143	£124	£111	£113	£39
Overall	£32	£55	£37	£32	£75	£58	£43	£55	£205	£86	£75	£81	£39

Panel C displays the average amount of remaining income allocated to spending in each round within each condition.

Panel D. Average Percentage Allocated to Spending by Condition and Round (n=502)

	1-1	1-2	1-3	1-4	2-1	2-2	2-3	2-4	3-1	3-2	3-3	3-4	4-1
Combined Lump Sum	30%	31%	30%	29%	27%	30%	29%	31%	24%	29%	29%	31%	32%
Standalone Lump Sum	30%	31%	31%	31%	29%	31%	28%	30%	23%	29%	29%	31%	33%
Combined Periodic	35%	32%	31%	31%	30%	32%	31%	31%	27%	26%	26%	27%	29%
Standalone Periodic	33%	31%	31%	32%	29%	31%	31%	31%	30%	30%	29%	29%	32%
Overall	32%	31%	31%	31%	29%	31%	30%	31%	26%	29%	29%	29%	32%

Panel D displays the average percentage of remaining income allocated to spending in each round within each condition.

TABLE 2
Primary Analysis

Panel A. Mean (s.d.) [n] of *Y3SPEND* by Condition

	DELIVERY		
TIME	**Combined**	**Standalone**	**Total**
Periodic	£439 (£466) [125]	£490 (£493) [127]	£465 (£479) [252]
Lump Sum	£434 (£475) [125]	£421 (£471) [125]	£428 (£472) [250]
Total	£436 (£469) [250]	£456 (£482) [252]	£446 (£476) [502]

Panel A displays the average amount of remaining income allocated to spending in year three by condition.

Panel B. ANCOVA for Effect of *TIME* and *DELIVERY* on *Y3SPEND*

Source	**df**	**Mean Square**	***F*-statistic**	***p*-value**
TIME	1	31,303	1.03	0.311
DELIVERY	1	37,632	1.24	0.267
TIME*DELIVERY	1	181,601	5.97	0.015
Y2SPEND	1	97,844,645	3,217.74	< 0.001
ERROR	497	30,408		

Panel B displays the results from an ANCOVA analysis in which *Y3SPEND* is the dependent variable and the independent variables are *TIME* (Lump Sum/Periodic), *DELIVERY* (Standalone/Combined), and the two-way interaction between *TIME* and *DELIVERY*. The amount of Lira allocated to spending in year 2, *Y2SPEND*, is used as a covariate to control for each participant's individual propensity to spend.

Panel C: Contrasts and Follow Up Test

Test	**Estimate**	***t*-statistic**	***p*-value**
Combined Periodic vs Standalone Lump Sum	-1.52	-0.07	0.945
Standalone Periodic vs Standalone Lump Sum	53.84	2.45	0.007[a]
Combined Periodic vs Combined Lump Sum	-22.24	-1.01	0.314

[a]One-tailed *p*-value (two-tailed *p*-values are reported for tests with estimates that are directionally inconsistent with the hypothesized effect)

Panel C displays the results from two planned contrasts and a follow up test based on the ANCOVA analysis reported in Panel B. The first contrast estimates the simple effect of TIME while holding DELIVERY constant as standalone and the second contrast estimates the simple effect of TIME while holding DELIVERY constant as combined. The follow up text approximates prior findings by comparing the Combined Periodic condition (i.e., government transfer paid as in increase in take-home pay) against the Standalone Lump Sum condition (i.e., government transfer paid as one separate check).

TABLE 3
Split-Phase Analysis

Panel A. Mean of Percentage of Lira Spent by Condition

	Y3R1SPEND%	***Y3R234SPEND%***
Combined Lump Sum	24%	30%
Standalone Lump Sum	23%	30%
Combined Periodic	27%	26%
Standalone Periodic	30%	30%
Overall	26%	29%

Panel A displays *Y31SPEND%* (the percentage of remaining income allocated to spending in round one of year three) and *Y3R234SPEND%* (the average percentage of remaining income allocated to spending in rounds two though four) by condition.

Panel B. ANCOVA for Effect of *TIME* and *DELIVERY* on *Y3R1SPEND%*

Source	**df**	**Mean Square**	***F*-statistic**	***p*-value**
TIME	1	0.115	6.38	0.012
DELIVERY	1	0.009	0.51	0.477
TIME*DELIVERY	1	0.075	4.14	0.042
Y2SPEND%	1	33.152	1,823.38	< 0.001
ERROR	497	0.018		

Panel B displays the results from an ANCOVA analysis in which *Y3R1SPEND%* (percentage of Lira spent in round 1 of year 3) is the dependent variable and the independent variables are *TIME* (Lump Sum/Periodic), *DELIVERY* (Standalone/Combined), and the two-way interaction between *TIME* and *DELIVERY*. *Y2SPEND%* (percentage of Lira allocated to spending in year 2) is used as a covariate to control for each participant's individual propensity to spend.

Panel C: Contrasts and Follow Up Test on *Y3R1SPEND%*

Test	**Estimate**	***t*-statistic**	***p*-value**
Standalone Periodic vs Standalone Lump Sum	0.055	3.23	0.001
Combined Periodic vs Combined Lump Sum	0.006	0.35	0.729
Combined Lump Sum vs Standalone Lump Sum	-0.016	-0.93	0.351
Combined Periodic vs Standalone Periodic	0.033	1.95	0.052

Panel C displays the results from follow-up tests based on the ANCOVA analysis reported in Panel A. The first test estimates the simple effect of TIME while holding DELIVERY constant as standalone and the second test estimates the simple effect of TIME while holding DELIVERY constant as combined. The third test estimates the simple effect of DELIVERY while holding TIME constant as lump sum and the final test estimates the simple effect of DELIVERY while holding TIME constant as periodic.

TABLE 3 (Continued)

Panel D. ANCOVA for Effect of *TIME* and *DELIVERY* on *Y3R234SPEND%*

Source	df	Mean Square	*F*-statistic	*p*-value
TIME	1	0.145	17.84	< 0.001
DELIVERY	1	0.025	3.14	0.077
TIME*DELIVERY	1	0.045	5.53	0.019
Y2SPEND%	1	38.146	4,695.33	< 0.001
ERROR	497	0.008		

Panel D displays the results from an ANCOVA analysis in which *Y3R234SPEND%* (average percentage of Lira spent in rounds 2 through 4 of year 3) is the dependent variable and the independent variables are *TIME* (Lump Sum/Periodic), *DELIVERY* (Standalone/Combined), and the two-way interaction between *TIME* and *DELIVERY*. *Y2SPEND%* (percentage of Lira allocated to spending in year 2) is used as a covariate to control for each participant's individual propensity to spend.

Panel E: Contrasts and Follow Up Test on *Y3R234SPEND%*

Test	Estimate	*t*-statistic	*p*-value
Standalone Periodic vs Standalone Lump Sum	-0.015	-1.33	0.185
Combined Periodic vs Combined Lump Sum	-0.053	-4.64	< 0.001
Combined Lump Sum vs Standalone Lump Sum	-0.004	-0.41	0.682
Combined Periodic vs Standalone Periodic	0.033	2.92	0.004

Panel E displays the results from follow up tests based on the ANCOVA analysis reported in Panel D. The first test estimates the simple effect of TIME while holding DELIVERY constant as standalone and the second test estimates the simple effect of TIME while holding DELIVERY constant as combined. The third test estimates the simple effect of DELIVERY while holding TIME constant as lump sum and the final test estimates the simple effect of DELIVERY while holding TIME constant as periodic.