Effect of alternative income assistance schedules on drug use and drug-related harm: a randomised controlled trial

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Summary

Background The synchronised monthly disbursement of income assistance, whereby all recipients are paid on the same day, has been associated with increases in illicit drug use and serious associated harms. This phenomenon is often referred to as the cheque effect. Because payment variability can affect consumption patterns, this study aimed to assess whether these harms could be mitigated through a structural intervention that varied income assistance payment timing and frequency.

Methods This randomised, parallel group trial was done in Vancouver, Canada, and enrolled recipients of income assistance whose drug use increased around payment days. The recipients were randomly assigned 1:2:2 to a control group that received monthly synchronised income assistance payments on government payment days, a staggered group in which participants received single desynchronised monthly income assistance payments, or a split and staggered group in which participants received desynchronised income assistance payments split into two instalments per month, 2 weeks apart, for six monthly payment cycles. Desynchronised payments in the intervention groups were made on individual payment days outside the week of the standard government schedules. Randomisation was through a pre-established stratified block procedure. Investigators and statisticians were masked to group allocation, but participants and front-line staff were not. Complete final results are reported after scheduled interim analyses and the resulting early stoppage of recruitment. Under intention-to-treat specifications, generalised linear mixed models were used to analyse the primary outcome, which was escalations in drug use, predefined as a 40% increase in at least one of: use frequency; use quantity; or number of substances used during the 3 days after government payments. Secondary analyses examined analogous drug use outcomes coinciding with individual payments as well as exposure to violence. This trial is registered with ClinicalTrials.gov, NCT02457949.

Findings Between Oct 27, 2015, and Jan 2, 2019, 45 participants were enrolled to the control group, 72 to the staggered group, and 77 to the split and staggered group. Intention-to-treat analyses showed a significantly reduced likelihood of increased drug use coinciding with government payment days, relative to the control group, in the staggered (adjusted odds ratio 0·38, 95% CI 0·20–0·74; p=0·0044) and split and staggered (0·44, 0·23–0·83; p=0·012) groups. Findings were consistent in the secondary analyses of drug use coinciding with individual payment days (staggered group 0·50, 0·27–0·96, p=0·036; split and staggered group 0·49, 0·26–0·94, p=0·030). However, secondary outcome analyses of exposure to violence showed increased harm in the staggered group compared with the control group (2·71, 1·06–6·91, p=0·037). Additionally, 51 individuals had a severe or life-threatening adverse event and there were six deaths, none of which was directly attributed to study participation.

Interpretation Complex results indicate the potential for modified income assistance payment schedules to mitigate escalations in drug use, provided measures to address unintended harms are also undertaken. Additional research is needed to clarify whether desynchronised schedules produce other unanticipated consequences and if additional measures could mitigate these harms.

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Introduction

Research on the drivers of substance use disorders has shown that this public health challenge is shaped by social and structural factors.[1] Accordingly, there have been repeated calls for social and structural interventions to inform evidence-based action[2] but, to date, little has been done to advance research in this area. Nevertheless, these calls are of enduring importance: as the overdose crisis persists, inter-related medical, social, and structural processes have inequitably distributed the catastrophic burden of drug-related harm, disproportionately affecting populations that are socioeconomically disadvantaged.[3]

Cash transfer benefits, although considered inadequate,[4] are a crucial strategy to mitigate the health effects of extreme poverty, especially for people who use drugs.[5] Nevertheless, an often overlooked pathway connecting

Research in context

Evidence before this study

Long-standing observational evidence, dating back over 25 years, documents increases in drug use and drug-related harm coinciding with income assistance payments that are synchronised each month, whereby all recipients receive a single monthly payment on the same day. We searched PubMed, the Cochrane Library, and Google Scholar without language restrictions using the terms "income assistance", "social assistance", "welfare", "drug use", "addiction", and "cheque/check effect" for articles that report observational, experimental (including randomised trials), or review methods, published between Jan 1, 1995, and Dec 22, 2020. All studies reporting individual or service provider outcomes were considered. Multiple observational studies and a 2011 knowledge synthesis review have described measurable populational harms coinciding with cash transfer benefit payments, including increased drug use, higher risk modes of drug use (eg, injection or syringe sharing), increased health service use (eg, emergency department visits or detoxification service use), service access barriers, interrupted hospital or other care, increased demands on first responders (eg, emergency calls), individual harm (eg, being forced to settle drug debt or negative interactions with police), and individual morbidity and mortality (eg, fatal and non-fatal overdose). No systematic review or meta-analysis has yet been done, most likely because of the wide range of outcomes assessed in these studies. Published research on the topic includes repeated calls to modify the monthly synchronised payment schedules for income assistance, rightly identifying an opportunity to examine whether social policy can be leveraged to mitigate drug use and drug-related harm. When we conceived this study in 2013, to our knowledge, no interventions had tested the effects of varying the timing and frequency of income assistance payments. A single natural experiment had documented delays in drug-related hospital admissions following delayed payments in one jurisdiction in California, USA. Since the start of our trial, additional observational studies have expanded the scope of outcomes linked to synchronised income assistance payments, but we are unaware of any intervention studies relevant to this topic.

Added value of this study

This trial has shown that desynchronisation of income assistance schedules—whereby recipients receive regular payments but not on the same day as each other—and smaller, more frequently disbursed payments might have beneficial effects on community-wide drug harms. Specifically, this study provides experimental evidence that desynchronised monthly payments or desynchronised and smaller, more frequent payments can effectively reduce escalations in drug use that coincide with income assistance payments. However, in the context of monthly synchronised payments that were ongoing in the community, this study also identified the potential for unanticipated increases in harm resulting from modifications to disbursement schedules, drawing attention to the potential for unintentional effects of policy-relevant structural change. This study answers long-standing calls for social and structural interventions to address drug use and drug-related harm.

Implications of all the available evidence

Social welfare programmes are known to be crucial to reducing the negative effects of poverty on health. Here, we have identified an approach that could help to mitigate the social and structural drivers of drug use and consequent drug-related harm coinciding with synchronised income support payments at an individual, community, or population level. Given the complex findings reported here, the need for experimental research that provides a strong evidence base for the potential intentional and unintentional effects of change is important. Although this research makes clear the consequences of social policy on health for vulnerable populations, careful consideration of how to maximise benefits while minimising harm is paramount.

socioeconomic disadvantages and drug use is the relationship between synchronised government income assistance, whereby all recipients are paid on the same day, and coincident escalations in drug use and associated harm.[5,6] Paying all recipients simultaneously is associated with high-intensity and high-risk drug use, suboptimal access to health services, exposure to violence, and both non-fatal and fatal accidental overdose.[6–9] Described in a review as affecting the distribution but not the overall quantity of drug use,[6] the concentration of drug use and drug-related harm around income assistance payments has serious consequences for people who use drugs, their communities, health systems, first responders, and service providers.

Despite recommendations to alter income assistance schedules, which commonly involve synchronised monthly payments,[4,6] to the best of our knowledge, only a single natural experiment has been done, showing delayed drug-related admission to hospital resulting from an administrative delay in payments.[10] Field experiments among populations that do not use drugs and economic modelling suggest that payment variability has an effect on consumption patterns.[11,12] However, in the absence of controlled research, it is unclear whether alternative disbursement schedules could mitigate problematic patterns of drug consumption and their attendant consequences, while preserving the public health benefits of cash transfers.[4]

We sought to answer calls for social and structural interventions and modified benefit payment schedules through an innovative and experimental structural intervention study seeking to displace the cyclical production of drug-related harm linked to cash transfer benefit payments among socioeconomically marginalised people who use drugs.[6] On the basis of previous literature, we hypothesised

that payment-coincident escalations in drug use result from two inter-related pathways: first, a direct individual pathway from the payments themselves, whereby people increase drug use in response to being paid, similar to consumption patterns in the general population;[11] and second, an indirect social pathway that triggers individual use through socially embedded drivers of drug use[13] that result from all recipients being paid at the same time. Identified a priori as the likely causal pathways that our intervention was seeking to modify,[7] we tested the hypotheses that desynchronising payments from the usual government schedule so that recipients are paid on different days (to mitigate social pathways), or, additionally, splitting standard payments into smaller amounts of money that are disbursed more frequently (to mitigate individual pathways), would reduce escalations in drug use on or around government (synchronised) and individual (desynchronised) payment days. We further hypothesised that as an extension of these reductions in drug use, drug-related harm would similarly decrease. A harm of central concern is exposure to violence, which, in many inner-city communities, is commonly linked to socioeconomic marginalisation.[14] Our analyses of secondary outcomes therefore included a focus on violence victimisation.

In seeking to establish which income assistance schedule produces the least amount of drug-related harm by varying the timing and frequency of income assistance disbursement,[7] this study aimed to develop an evidence base for harnessing modifiable social policy levers to advance public health for socioeconomically marginalised people who use drugs.

Methods

Study design and participants

The impact of Alternative Social Assistance on drug-related harm (TASA) study, known as the Cheque Day Study, was a multi-arm, parallel group, unblinded, superiority randomised controlled trial (RCT). The study was undertaken in Vancouver's Downtown Eastside, a neighbourhood commonly characterised by an open drug use scene, poverty, and community cohesion.[15] Participants were recruited through community-based methods, including word of mouth, advertisements at service providers, street-based recruitment, and recruiting from pre-existing community-based research studies. Participants were eligible for the study if they were 19 years or older, received provincial income transfers (appendix pp 2–3), reported the regular use of illicit drugs other than cannabis and had intensified drug use coinciding with income assistance payments in the previous 6 months, and were willing to change their income assistance payment schedules. Participants were ineligible for the study if they had plans to discontinue assistance receipt or had pending criminal justice system involvement that could lead to incarceration, which results in the suspension of benefit receipt. Full eligibility details are outlined in the appendix (p 4). All participants provided written informed consent. This study was approved by the Providence Health Care–University of British Columbia Research Ethics Board. The study protocol was published previously.[7] A detailed description of study procedures, including updates since protocol publication, are available online.

For a **description of the study procedures** see https://doi.org/10.17605/OSF.IO/UB63F

Randomisation and masking

Volunteer participants were randomly assigned to one of the following: (1) a control group that received monthly synchronised payments on government payment days; (2) a staggered group in which participants received single desynchronised monthly income assistance payments; or (3) a split and staggered group in which participants received desynchronised income assistance payments split into two instalments per month 2 weeks apart. In both experimental treatment groups, payments were received on randomly chosen days (individual payment days) that were outside the week of the government schedule and consistent across the 6 monthly payment cycles. The split and staggered intervention was based on evidence of more extreme consumption fluctuations associated with a longer period between payments.[11] Randomly chosen schedules prevented the creation of secondary synchronised payment days among study participants.

Enrolment was done by the study coordinator (AL) who coordinated all participation, payments, and did quantitative and qualitative interviews. Following screening, the provision of informed consent, and completion of a baseline assessment, study volunteers were randomly assigned to the control and two intervention groups with use of a 1:2:2 allocation ratio through a pre-established stratified block randomisation procedure. Randomly sized blocks of a number divisible by five were stratified by level of income assistance received: low (employable), middle (persons with persistent multiple barriers), or high (persons with disability).[16,17] Allocation was random within each block and established through a computer-generated random number sequence by a statistician with no further involvement in the study. Trial staff were unaware of block sizes. Group assignment was automated within the data collection software (Oracle DBMS 12·1) to ensure allocation concealment and obtained by the research coordinator. Although it was not possible to mask participants or front-line staff because of intervention-specific questions in the study instrument, investigators and study statisticians were masked to group allocation through the assignment of numerical identifiers by the research coordinator. To prevent performance and expectancy biases, trial hypotheses were masked to participants and interviewers.

See **Online** for appendix

Procedures

Procedures to vary the timing and frequency of income assistance payments were managed through partnership with a local credit union (Pigeon Park Savings, a local branch of VanCity Savings and Credit Union). Participants in the two intervention groups had their income assistance

directly deposited into a credit union account and released on their individual payment days according to the payment schedule set by the randomisation algorithm. Participants were on the intervention for six income assistance cycles, or 26 weeks, which was our criterion for treatment completion. Post-intervention safety follow-up was done 60 days after trial completion. Further details of procedures are in the appendix (pp 4–7).

There was no obvious contamination across intervention groups because payment schedules were followed with minimal deviation. Approved and planned (per protocol) systematic adaptations allowed for access to essential funds for rent or bill payments. Adaptations not anticipated by the study protocol were required when urgent access to funds was needed (eg, to secure housing; n=64) or when government-scheduled payments were delayed because of participant incarceration (n=5). Intervention implementation deviations occurred because of administrative difficulties implementing direct deposit (n=10); delayed payments from the government (n=10); or issues at the credit union (eg, system upgrade or teller error; n=6). To maintain consistency across groups, no support for accessing government funds was provided independent of study intervention implementation.

At baseline assessments, before the participants were randomly assigned, demographic information was collected, as well as data from the past 6 months and past 2 weeks on drug use activities and related exposures, including exposure to violence. At the fortnightly follow-up research visits, data from the past 2 weeks were collected on all measures, including questionnaire items on participant activity and exposures on government payment days and individual payment days (when different). Participant safety, including the monitoring of adverse events (AEs) or serious adverse events (SAEs), was assessed at every follow-up.

Drug use data were gathered with the Timeline Follow Back (TLFB),[18] a reliable, validated instrument that enables collection of daily information on substances used, method of administration (eg, smoked or injected), and estimated street value of drugs used, a proxy for quantity of use.[19] Consistent with intention-to-treat (ITT) principles, those who withdrew from the intervention returned to the government schedule but continued to complete follow-ups, including TLFB data collection.[20]

Participants received honoraria for their participation: CA$30 for the baseline interview and $10 per follow-up, with incentive bonuses after the completion of the first ($10), fifth ($15), ninth ($20), final ($25), and post-study ($15) follow-up interviews for a maximum of $245.

Outcomes

This study's prespecified primary outcome was a binary measure of escalated drug use coinciding with government assistance payment days. Building on earlier studies reporting 25–85% increases in drug use and drug-related harm on or around payment days,[6,7] a study participant's drug use was a priori classified as escalated if, in the first 3 days starting with the government payment day, a participant increased by 40% or more: (1) their average daily frequency of non-cannabis drug use; (2) their average daily quantity of drugs consumed, operationalised as average daily street value; or (3) their average number of non-cannabis substances used, including alcohol and illicit prescription opioid use, compared with all other days of the calendar month, calculated per month. Additional prespecified analyses were done to examine the composite measure components.

As a secondary outcome, we assessed escalated drug use coinciding with individual assistance payment days, operationalised as the primary outcome, but focusing on the 3 days beginning when an individual received a scheduled income assistance payment, regardless of government payment timing (ie, government payment day for the control group, randomised payment days for experimental treatment groups).

An additional important secondary outcome was exposure to violence,[6,8] derived from affirmative responses to the question: "In the past 2 weeks, have any of the following things happened to you?" Response options included having been robbed; threatened with a weapon; punched, slapped, pushed down, or pepper sprayed; beaten up; confined; attacked with a weapon; sexually assaulted; forced to sell sex; or having had an involuntary haircut. Overall exposure to violence was operationalised as a binary measure of any exposure in the 2 weeks before follow-up. We additionally assessed the binary measures of exposure to violence coinciding with government and individual payment days and counts of violent incidents derived from timing-specific and frequency-specific questions. Analyses of other secondary endpoints that pertain to service use, perpetration of violence, non-fatal overdose, and income generation will be undertaken and reported separately.

Participant safety was assessed at each follow-up through standard RCT safety monitoring questionnaires. Safety concerns were classified as AEs or SAEs, such as admission to and treatment in hospital, exposure to violence (assessed separately from the secondary outcome), fatal or non-fatal overdose, or death from other causes. Safety concerns were further classified according to whether there was reasonable possibility that they were caused by the intervention.

Additional measures were relevant as covariates for analyses. Sociodemographic variables included age; gender, categorised as cisgender man or woman, or transgender; and ethnicity, categorised as White people, people of Indigenous ancestry, or non-Indigenous people of colour. Socioeconomic variables included dichotomised variables for high school educational attainment, homelessness, and residency in Vancouver's inner city. We also considered categorical measures of income assistance level as well as substance use disorder treatment status, differentiating between individuals not enrolled in any treatment, those in opioid-assisted therapy (including

methadone maintenance therapy and suboxone treatment), and those receiving other forms of treatment (including detoxification services, residence in a recovery house, attendance at a treatment centre, seeing a counsellor, or participating in a Narcotics Anonymous programme). As a covariate in secondary analyses of violence, frequent substance use was operationalised as 10 or more days of alcohol or non-cannabis illicit drug use in the 14 days before follow-up.

Study oversight

The study was overseen by an independent data safety and monitoring committee that approved a quarterly reporting structure, a prespecified analytical plan, and criteria for study stoppage (appendix pp 5–6).[21] Protocol changes that were recommended by the data safety and monitoring committee and approved by the Research Ethics Board were implemented in June, 2016, and included: (1) modifying the allocation ratio from 1:1:1 to 1:2:2 to ensure sufficient numbers of participants in the intervention groups; (2) increasing the recruitment target to 400 participants; (3) adding secondary feasibility outcomes; and (4) providing emergency fund access equivalent to that available to control group participants. These changes were justified in the context of an RCT in a socioeconomically marginalised population to adapt to higher than anticipated crossover from intervention to control conditions and to support participant safety.

Statistical analysis

Detailed in the published protocol,[7] the original sample size calculations allowed for the detection of a 20% difference in rates (between 85% and 65%) of intensified drug use between the control and each intervention group, with a power of 0·8 and a two-tailed α of 0·05, for a total initial sample size of 273 (with 91 individuals in each group). Updated sample size calculations based on the new allocation ratio detailed in the final protocol maintained the same detection power, altered the allocation ratio, and adjusted the rates of intervention withdrawal. The revised sample size calculation increased the recruitment target to 400 (80 in the control group, 160 in each intervention group).

Pre-analysis data modifications are detailed in the appendix (p 7). For interim and final analyses of primary and secondary endpoints, we used ITT,[20] generalised linear mixed models with logit link for binary outcomes.[22] Analyses of the number of incidents of violence used generalised linear models with a negative binomial distribution and adjustment for the number of observations per person as an offset.[23] Study intervention group was the primary variable of interest, with the control group as the reference category. Models were adjusted for sociodemographic characteristics, level of income assistance, whether a participant was already a credit union member at enrolment (to account for potential effects of becoming a member), days since last payment (to account for heterogeneous recall reliability), and whether an observation followed the implementation of the amended protocol. Separate models were developed for the full composite measure and for each of the frequency, quantity, and number of substances components. Time-varying covariates were excluded from models to prevent covariate bias from adjusting for potential treatment modulation.[24] Consistent with standard practice, missing outcome data were initially categorised as an affirmative escalation in drug use, based partially on an assumption that absences might be linked to drug use.[20,25] Prespecified sensitivity analyses tested this assumption and adjusted for time-varying covariates. Additional sensitivity analyses replicated analyses categorising data in two different ways. First, modified per-protocol analyses categorised observations according to whether participants were actively exposed to the intervention or had withdrawn and reverted to the government payment schedule, classifying these as in the control group.[26] Second, we addressed missing data using prespecified standard multiple imputation procedures for longitudinal data (appendix p 6). All analyses were done with R (version 3.5.3). This trial was preregistered on ClinicalTrials.gov (NCT02457949).

Role of the funding source

The funders of the study had no role in study design, data collection, data analysis, data interpretation, or writing of the report.

Results

Between Oct 27, 2015, and Jan 2, 2019, 194 participants were randomised and followed up, providing a total of 2106 observations (median observations per person: 13; IQR 9–14). To our knowledge, there were no violations of eligibility. Participant flow and intervention withdrawal patterns are summarised in figure 1. 45 participants were randomly assigned to the control group, 72 to the staggered group, and 77 to the split and staggered group. The overall completion rate for baseline and scheduled follow-ups was 2106 (80·1%) of 2611 observations, a highly favourable rate compared with other studies with similar populations.[7] 26 participants completed the full 6-month intervention protocol and 89 completed a portion of the intervention protocol (figure 1). 25 (13%) participants did not complete the trial (ie, did not provide observations to the end of the trial period, regardless of treatment status): 16 were lost to follow-up, defined as missing the last three follow-up interviews; five withdrew from the trial; and four participants died during participation. Two additional participants were deceased in the post-trial period for safety monitoring (figure 1). The reported analyses made use of all available data.

Baseline participant characteristics stratified by study group are described in table 1. 89 (46%) participants were women and four (2%) were transgender; 79 (41%) self-identified as having Indigenous ancestry and four (2%) as being non-Indigenous people of colour.

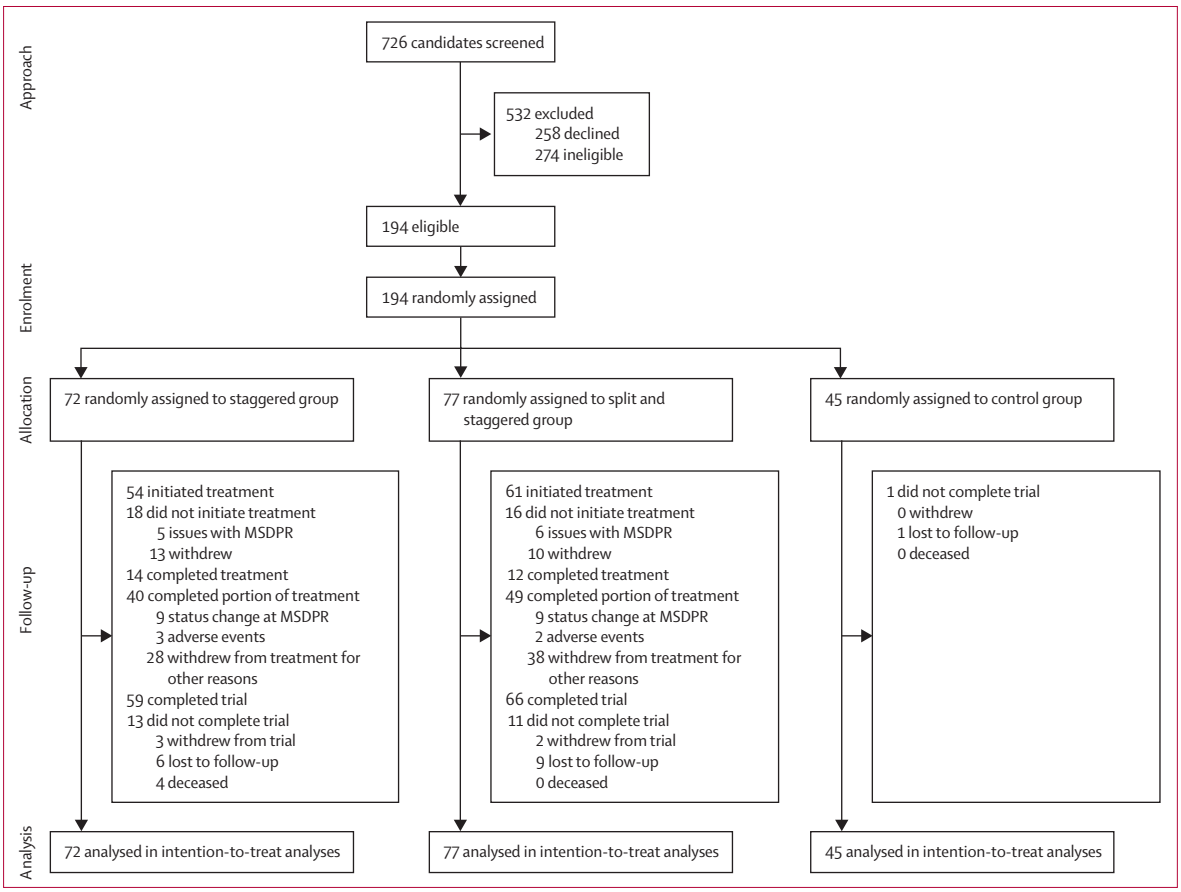

Figure 1: **Consolidated Standards of Reporting Trials trial profile**
Completed treatment refers to participants who were on the study intervention and under active observation for 6 months; completed trial refers to those who provided observations to the end of the trial period (ie, not withdrawn from the trial, lost to follow-up, or deceased), regardless of treatment status. MSDPR=Ministry of Social Development and Poverty Reduction.

Planned interim analyses after a third of the intended sample (133/400 participants) had completed 6 months of follow-up produced a clear signal of intervention efficacy for the primary outcomes and a weaker signal of safety concerns not observed as part of regular monitoring. Therefore, the data safety and monitoring committee recommended the stoppage of new participant recruitment in May 15, 2018. The results reported here are the final results with all study data, including data collected after the completion of interim analyses (ie, for 194 participants).

Table 2 summarises study outcomes. There were self-reports of 858 escalations in substance use on government payment days (51·7% of 1660 observations) among 156 (80%) of 194 individuals, 899 escalations in substance use on individual payment days (53·4% of 1684 observations) among 160 (82%) individuals, and 255 incidents of exposure to violence (12·1% of 2106 observations) among 103 (53%) individuals. The percentage for each variable was calculated using the total number of observations with non-missing values, which differed by variable and group.

Full modelling results are reported in the appendix (pp 8–27). ITT analyses of the primary outcome (figure 2) supported the hypothesis that varying the timing and frequency of income assistance payments would reduce escalations in drug use on government payment days in the staggered group (adjusted odds ratio [AOR] 0·38, 95% CI 0·20–0·74, p=0·0044) and the split and staggered group (0·44, 0·23–0·83, p=0·012) compared with the control group (appendix p 8). Decomposition analyses of the components of the primary outcome indicated that, in both intervention groups, the intervention reduced the frequency of drug use (staggered group 0·46, 0·26–0·81, p=0·0074; split and staggered group 0·54, 0·31–0·93, p=0·028) and the quantity of drug use (staggered group 0·40, 0·21–0·76, p=0·0060; split and staggered group 0·48, 0·26–0·90, p=0·023) relative to the control group. The number of substances used did not differ significantly between groups. Results from most sensitivity analyses were consistent with these findings (appendix pp 9–13); those testing the implications of categorising missing data as an escalation in drug use (appendix p 20) indicated that this assumption attenuated results

and therefore represents a conservative modelling approach.

ITT analyses of our secondary outcome assessing drug use increases coincident with individual payment days (figure 2) also supported our hypotheses. Drug use coinciding with individual payment days was reduced in the staggered group (AOR 0·50, 95% CI 0·27–0·96, p=0·036) and the split and staggered group (0·49, 0·26–0·94, p=0·030) compared with the control group (appendix p 14). Decomposition analyses identified significant reductions for participants in the split and staggered group, but not in the staggered group, in the frequency (0·52, 0·29–0·94, p=0·031) and quantity of drug use (0·53, 0·29–0·96, p=0·036) relative to the control group. The number of substances used did not differ significantly between groups. In sensitivity analyses, modified per-protocol analyses identified a reduced frequency of drug use coinciding with individual payment days in the split and staggered group (0·60, 0·38–0·97, p=0·037) compared with controls (appendix p 15). In ITT models with multiply imputed data and including time-varying covariates, drug use frequency, quantity, and the overall composite measure were reduced in both intervention groups relative to the controls (appendix pp 16–17, 19).

In ITT analyses of exposure to violence (figure 2), contrary to study hypotheses, overall exposure to violence increased in the staggered group (AOR 2·71, 95% CI 1·06–6·91, p=0·037) compared with the control group (appendix p 22). Findings were similar in ITT models with multiply imputed data and including time-varying covariates (appendix pp 24, 27). No significant results were observed for participants in the split and staggered group. Similarly, results were not significant for either intervention group for the incidents of violence coinciding with government income assistance payments, individual income assistance payments, or the frequency of violent incidents, although the small number of reported incidents on individual payment days among active intervention recipients should be noted.

Despite monitoring the occurrence of AEs and SAEs among participants at every study visit, the systematic relationship between the intervention study groups and exposure to violence was not apparent until the completion of interim analyses, and was later confirmed in analyses of the complete dataset. Additionally, 51 unique individuals reported at least one severe or life-threatening AE during the full study period. The most common events involved admission to and treatment in hospital (n=38), overdose requiring the administration of naloxone (n=17), violence resulting in injury (n=15), and eviction (n=1; table 3). Six participants died (four during the follow-up period, two during the post-trial safety monitoring period). No AEs or SAEs were directly attributed to the study intervention; one AE, exposure to violence for a participant in the split and staggered group, was indirectly associated with the intervention.

	Control group (n=45)	Staggered group (n=72)	Split and staggered group (n=77)
Sociodemographic characteristics			
Age	45 (37–53)	43 (34–51)	45 (39–51)
Gender			
Men	20 (44%)	41 (57%)	40 (52%)
Women	22 (49%)	31 (43%)	36 (47%)
Transgender	3 (7%)	0	1 (1%)
Ethnicity			
White	25 (56%)	41 (57%)	45 (58%)
People with Indigenous ancestry	20 (44%)	29 (40%)	30 (39%)
Non-indigenous people of colour	0	2 (3%)	2 (3%)
Socioeconomic characteristics			
Educational attainment			
<High school	20 (44%)	35 (49%)	44 (57%)
≥High school	25 (56%)	37 (51%)	33 (43%)
Housing status*			
Housed	26 (58%)	34 (47%)	52 (68%)†
Homeless	19 (42%)	38 (53%)	25 (33%)†
Income assistance type*			
Employable	8 (18%)†	17 (24%)†	19 (25%)
Persistent multiple barriers	7 (16%)†	2 (3%)†	4 (5%)
Disability	30 (67%)†	53 (74%)†	54 (70%)
Drug use-related characteristics			
Drug use patterns*			
Any opioid	26 (58%)	54 (75%)	60 (78%)
Daily opioid	14 (31%)	34 (47%)	31 (40%)
Any stimulant	42 (93%)	67 (93%)	72 (94%)
Daily stimulant	15 (33%)	24 (33%)	31 (40%)
Any alcohol	28 (62%)	34 (47%)	33 (43%)
Daily alcohol	3 (7%)	5 (7%)	4 (5%)
Any cannabis	24 (53%)	37 (51%)	40 (52%)
Daily cannabis	9 (20%)	18 (25%)	22 (29%)
Substance use disorder treatment status*			
No treatment	18 (40%)	26 (36%)	21 (27%)
Opioid-assisted treatment‡	19 (42%)	35 (49%)	44 (57%)
Other treatment§	8 (18%)	11 (15%)	12 (16%)

Data are n (%) or median (IQR). *Describes activities or exposures in the 6 months before the baseline interview. †Percentages do not add up to 100% because of rounding. ‡Opioid-assisted treatment included methadone maintenance therapy and suboxone treatment. §Other treatment included detoxification services, residential treatment, counselling, 12-step programmes, or other non-substitution-based forms of treatment.

Table 1: **Baseline characteristics of the intention-to-treat sample stratified by study group (n=194)**

Discussion

In this study, we have shown that a structural change to the timing and frequency of income assistance payments effectively mitigates escalations in drug use coinciding with government as well as individual payment days. Specifically, monthly and twice-monthly split payments

	As randomised (intention to treat)				Effective study group (modified per protocol)			
	Control group* (580 observations)	Staggered group (711 observations)	Split and staggered group (815 observations)	Total	Control group* (1442 observations)	Staggered group (345 observations)	Split and staggered group (319 observations)	Total
Government payment coincident								
Drug use frequency	314/488 (64·3%)	240/532 (45·1%)	304/640 (47·5%)	858/1660 (51·7%)	684/1195 (57·2%)	84/233 (36·1%)	90/232 (38·8%)	858/1660 (51·7%)
Drug use quantity	339/488 (69·5%)	269/534 (50·4%)	341/642 (53·1%)	949/1664 (57·0%)	766/1196 (64·0%)	97/235 (41·3%)	86/233 (36·9%)	949/1664 (57·0%)
Number of drugs used	194/489 (39·7%)	187/534 (35·0%)	205/642 (31·9%)	586/1665 (35·2%)	449/1197 (37·5%)	70/235 (29·8%)	67/233 (28·8%)	586/1665 (35·2%)
Individual payment coincident								
Drug use frequency	315/488 (64·5%)	271/544 (49·8%)	313/653 (47·9%)	899/1685 (53·4%)	686/1195 (57·4%)	116/245 (47·3%)	97/245 (39·6%)	899/1685 (53·4%)
Drug use quantity	343/488 (70·3%)	299/546 (54·8%)	368/655 (56·2%)	1010/1689 (59·8%)	771/1196 (64·5%)	128/247 (51·8%)	111/246 (45·1%)	1010/1689 (59·8%)
Number of drugs used	189/489 (38·7%)	201/546 (36·8%)	201/655 (30·7%)	591/1690 (35·0%)	443/1197 (37·0%)	84/247 (34·0%)	64/246 (26·0%)	591/1690 (35·0%)
Exposure to violence								
Overall exposure	51/580 (8·8%)	107/710 (15·1%)	97/815 (11·9%)	255/2105 (12·1%)	162/1442 (11·2%)	54/344 (15·7%)	39/319 (12·2%)	255/2105 (12·1%)
Government payment coincident	10/578 (1·7%)	26/702 (3·7%)	22/807 (2·7%)	58/2087 (2·8%)	29/1429 (2·0%)	15/342 (4·4%)	14/316 (4·4%)	58/2087 (2·8%)
Individual payment coincident	10/580 (1·7%)	12/711 (1·7%)	11/815 (1·3%)	33/2106 (1·6%)	29/1442 (2·0%)	1/345 (0·3%)	3/319 (0·9%)	33/2106 (1·6%)

Data are n/N (%). A total of 194 participants provided 2106 observations. Percentages are provided as the percentage of the total number of observations with non-missing values for each variable, which differed by variable and group. *Totals between government and individual payment days for control group participants differed slightly because of minor protocol deviations (eg, incarceration or the ministry withholding payments) that delayed government scheduled payments.

Table 2: **Self-reported cumulative instances of government payment-coincident and individual payment-coincident increases in drug use and exposure to violence among 194 participants, stratified by study group, 2015–19**

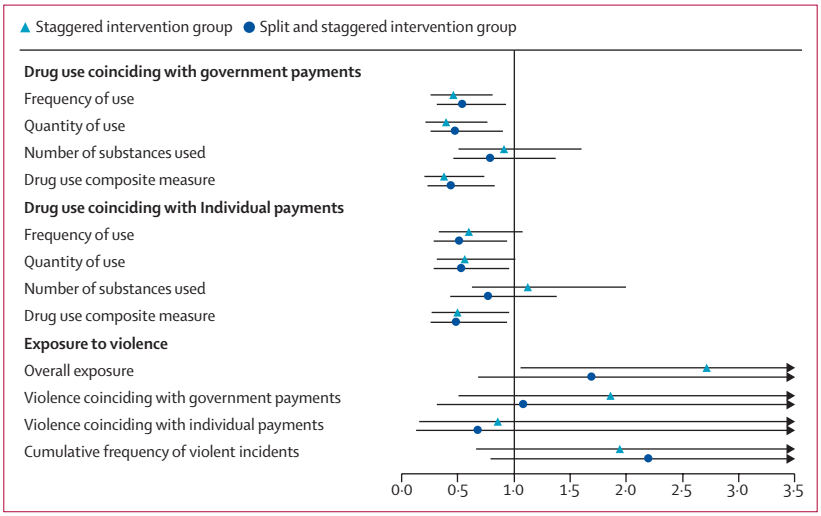

Figure 2: **Multivariate, generalised linear mixed model analyses of drug use and violence coinciding with income assistance payments among people who use illicit drugs (n=194)**
Intention-to-treat analysis of the effects of varying the timing and frequency of income assistance payments on: (1) escalations of drug use coinciding with government payment days; (2) escalations of drug use coinciding with individual payment days; and (3) exposure to violence among people who use illicit drugs. The control group is the reference category for all analyses.

that were desynchronised from regular government schedules significantly reduced the payment-coincident frequency and quantity of drug use. However, we unexpectedly also identified unintended negative effects: overall exposure to violence increased for people receiving desynchronised monthly payments in some analyses. Our results suggest that variation in payment timing and frequency can modify the individual and social pathways that increase triggers for drug use, but might negatively affect vulnerability to violence. Importantly, our findings do not justify the drug testing of benefit recipients nor the retrenchment or withdrawal of benefits for people who use drugs, practices that are largely deemed unfair, immoral, and objectionable.[27] Instead, this study has identified the cash benefit disbursement design as a potentially important and underused tool to promote public health for people who use drugs, with substantial potential to intervene in the social and structural contexts in which drug use and drug-related harm are produced and reinforced.

Past modelling and observational research identifying the benefits of staggered, more frequent payments[8,11,12] suggest potential applications to drug-related outcomes. Our results substantiate this potential, showing that changing payment schedules can change patterns of drug use. Importantly, we additionally identified potential negative consequences of these changes. To the best of our knowledge, this is the first experimental study seeking to use social policy levers to structurally modify the nexus between government payments and drug use among

	As randomised (intention to treat)				Effective study group (modified per protocol)			
	Control group (580 observations)	Staggered group (711 observations)	Split and staggered group (815 observations)	Total	Control group (1442 observations)	Staggered group (345 observations)	Split and staggered group (319 observations)	Total
Admission to and treatment in hospital for physical health	8 (1·4%)	11 (1·5%)	12 (1·5%)	31 (1·5%)	19 (1·3%)	7 (2·0%)	5 (1·6%)	31 (1·5%)
Admission to and treatment in hospital for mental health	1 (0·2%)	0	6 (0·7%)	7 (0·3%)	7 (0·5%)	0	0	7 (0·3%)
Exposure to violence	2 (0·3%)	4 (0·6%)	9 (1·1%)	15 (0·7%)	9 (0·6%)	3 (0·9%)	3 (0·9%)	15 (0·7%)
Non-fatal overdose (moderate to severe)*	3 (0·5%)	7 (1·0%)	7 (0·9%)	17 (0·8%)	14 (1·0%)	2 (0·6%)	1 (0·3%)	17 (0·8%)
Fatal overdose	0	2 (0·3%)	0	2 (0·1%)	0	2 (0·6%)	0	2 (0·1%)
Death (excluding overdose)	0	2 (0·3%)	0	2 (0·1%)	1 (0·1%)	1 (0·3%)	0	2 (0·1%)
Other (eg, eviction)	0	0	1 (0·1%)	1 (0·1%)	0	0	1 (0·3%)	1 (0·1%)
Total	14 (2·4%)	26 (3·7%)	35 (4·3%)	75 (3·6%)	50 (3·5%)	15 (4·3%)	10 (3·1%)	75 (3·6%)

Data are n (%). A total of 194 participants provided 2106 observations. Percentages are provided as a percentage of the total observations in each group. Differences in incidents of exposure to violence between monitoring for adverse events and serious adverse events and reported secondary outcomes are attributable to the different questionnaire instruments used to collect these data. Instances where exposure to violence or non-fatal overdose resulted to admission to or treatment in hospital are classified according to primary exposure (violence or overdose) to avoid double counting. *Overdose requiring the administration of naloxone.

Table 3: **Summary of adverse events and serious adverse events captured by safety monitoring procedures among 194 participants, stratified by study group, 2015–19**

people who use drugs. In light of our complex findings, this study illustrates the importance of experimental research in anticipating the intended and unintended effects of social and other policy reforms.

How does desynchronising and splitting income assistance payments migitate the so-called cheque effect? The prima facie explanation is instrumental: the absence of a benefit payment on government payment days prevents increased consumption on those days. Yet, despite considerable reductions in drug use coinciding with government payment days among intervention participants, escalations in drug use were not altogether eliminated, implicating social influences on drug use that remain even in the absence of individual payments. A range of mechanisms, including imitation, socially conditioned reinforcement, social facilitation, and proximity to drug use environments,[13] might explain why escalations in drug use coinciding with government payment days were not eliminated. Additionally, in analyses of drug use patterns on individual payment days when money was available for intervention participants, we nevertheless recorded a decreased frequency and quantity of drug use, suggesting the mitigation of social triggers for use. This result further reinforces our premise that modifying structurally regulated influences on substance use by not paying all income assistance recipients simultaneously or in one lump sum holds considerable promise for the reduction of drug-related harm.

However, our findings of increased exposure to violence among participants in the staggered group necessitate caution. These results point to the potentially negative effect of structural differentiation, whereby the differentiated timing of money receipt for a subset of people in an otherwise synchronised system results in some members of the community having money when others do not. The increased economic activity that accompanies government payments often involves settling drug debts and corresponding expectations that people will have money, potentially increasing the risk of violence for intervention participants.[28] Independent of drug debt cycles, such structural differentiation might result in intervention participants being targeted. In short, standing out in this context has potentially harmful consequences.

Although further research is needed, the risk of violence might decrease if the intervention were implemented on a wider scale. The study participants represent a small subsample of the population of people who use drugs and receive income assistance, and knowledge of the study was not widespread. Creditors' expectations for repayment on government payment days might be less strongly held in the presence of system-wide variation in payment dates. Furthermore, high rates of violence in the study context occurring independently of the study[14] might be linked to low levels of income assistance, which increased marginally during data collection in 2017 but had otherwise not increased since 2010. Reducing poverty by increasing income assistance rates might make predatory violence less common. Importantly, violence is one of numerous potential drug-related harms linked to synchronised payments. Analyses of other secondary endpoints outside the scope of this report will help to determine whether the complex patterns here are reflected in other outcomes.

These findings have important implications for policy change. Recognising the central importance of cash transfer benefits in mitigating the harms of poverty among people who use drugs, any reform efforts would need to include measures to mitigate foreseeable negative consequences. Notably, modern banking systems and the increasingly digitised social assistance infrastructure in

many contexts suggest that reform would not be required for recipients to whom the rationale for change does not apply or would not be beneficial. Such payment systems could provide customisable income assistance schedules and individualised social care that are adaptable to individual financial management practices, preventing the unintended consequences of one-size-fits-all approaches and facilitating improved health and social outcomes, as well as scalability. Crucially, as was reinforced by widespread stakeholder involvement during the study (appendix p 28), any consideration of changes should meaningfully involve people who use drugs and incorporate their preferences. Furthermore, changes should be applied without collecting drug use information, because such data could be used inappropriately to discriminate against or stigmatise recipients.[27]

We note several limitations. The study was implemented in a single context. This limitation might restrict the generalisability and external validity of the results, which we anticipate will be particularly but not uniquely relevant to contexts in which sizeable populations of people who use drugs receive cash transfer benefits. The so-called cheque effect has been identified in many other contexts,[6,9,10] and flexibly altering payment schedules might have benefits well beyond populations of people who use drugs.[11] Generalisability might also be limited by selection effects present in all voluntary experimental studies whereby the study sample comprises individuals willing to undertake the study intervention. Although the frequency of intervention withdrawal in our sample is important to note, the presence of strong and consistent results across multiple sensitivity analyses is indicative of the intervention's success in modifying drug use patterns. Notwithstanding, planned qualitative process evaluations will be crucial to assess why the intervention was or was not beneficial for different participants. Additionally, the study data are self-reported and might be subject to response bias, particularly that linked to socially desirable reporting, which might have been amplified by the inability to mask assessors. Nevertheless, reports of high reliability and validity[29] have met this common concern for research among people who use drugs, and we used the reliable and valid TLFB[18] to minimise these limitations, with no known reason why self-reporting would differ across study groups. Although there is potential for measurement error, the use of detailed daily measurements of drug use derived from the TLFB increased the precision of our estimates.

Our study results and limitations suggest potentially important future research directions. Evaluating the effects of policy reform on public health requires a change in policy to study, that relevant outcomes are monitored, and that research documents the pathways linking policies with health outcomes.[30] Numerous barriers to much needed action to address the social determinants of health of people who use drugs have been identified.[3] This experimental study establishes the capacity to test potential social policy reform without implementing changes at a population level, with potential application to policy-relevant, preventive, and upstream determinants of health. This advance is particularly important when considering the need to anticipate and evaluate the unintended effects of policy reform. In light of the substantial numbers of participants who withdrew from the intervention, implementing a study in which participants choose their intervention schedule could provide insight into how aligning benefit receipt with participant preference affects key outcomes.

Seeking to respond to long-standing calls for social and structural interventions to address the drivers of drug-related harm,[1] this study provides evidence of the effects of changing income assistance payment schedules. Our complex findings call for a more nuanced understanding of how individuals will respond to social and structural change, and the need to consider measures that mitigate the unintended effects of policy reform. This study shows a promising approach to the development of robust evaluations of structural changes that are potentially scalable, and that are directly relevant to the upstream determinants of health. This trial additionally emphasises the importance of exploring whether reform to a wide range of cash transfer benefit systems, which aim to mitigate the harms of poverty but nevertheless shape drug use behaviour, can address the disproportionate burden of problematic drug use and drug-related harm among socioeconomically marginalised people who use drugs.

Contributors

LR designed and oversaw the study as principal investigator. AL supervised data collection activities and coordinated the study. LR, AL, JCC, and EN accessed and verified the data and JCC and EN did the statistical analysis. LR wrote the first draft of the report. TK, EW, M-JM, and BDLM acted as coinvestigators and provided the scientific input during the development and conduct of the study. JS provided integral research design and statistical advice. AL, JCC, EN, M-JM, BDLM, JS, EW, and TK provided editorial feedback on the report. All authors had full access to all the data reported in the study. The corresponding author had final responsibility for the decision to submit for publication.

Declaration of interests

The University of British Columbia has received an unstructured grant from NG Biomed to support M-JM. EW is the chief medical officer of Numinus Wellness. All other authors declare no competing interests.

Data sharing

Trial data and the study data dictionary will not be made publicly available because of the sensitivity of distributing data gathered from marginalised participants engaged in criminalised behaviour. In exceptional circumstances, the sharing of data will be considered by application. In cases where requests to access study data are approved, the data dictionary and deidentified participant data, including only variables relevant to the application, will be provided. Additional documentation, including the study protocol, prespecified statistical analysis plan, consent form, and other related study documents will be available. Data will be available effective Jan 1, 2022, indefinitely by application to the study principal investigator and corresponding author at bccsu-lr@bccsu.ubc.ca. Access criteria for the data include release only to researchers with investigator support after approval of a proposal with a signed data access agreement and upon proof of completion of the requisite data protection protocols.

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