Social Science & Medicine 283 (2021) 114194

Contents lists available at ScienceDirect

Social Science & Medicine

journal homepage: www.elsevier.com/locate/socscimed

Effectiveness of clinic-based cardiovascular disease prevention: A randomized encouragement design experiment in the Philippines

Joseph Capuno [a], Aleli Kraft [a], Owen O'Donnell [b,*]

[a] School of Economics, University of the Philippines Diliman, Philippines
[b] School of Economics, School of Health Policy & Management, Erasmus University Rotterdam, Tinbergen Institute, University of Lausanne, Switzerland

ARTICLE INFO

Keywords:
Cardiovascular disease
Hypertension
Prevention
Randomized encouragement design
The Philippines

ABSTRACT

Rationale: Evidence on effectiveness of routine clinic-based cardiovascular disease (CVD) prevention in low- and middle-income countries is lacking. This study aimed to provide evidence on exposure to primary prevention of CVDs obtained through visits to public health clinics in the Philippines that are responsible for operating a widely-adopted CVD risk screening and management protocol.

Method: In a 2018 cluster-randomized experiment in Nueva Ecija province, participants aged 40–70 with no history of CVD in randomly selected communities were offered a money-prize lottery ticket if they visited a public health clinic for a check-up. The induced variation in clinic visits was used to estimate effects of a check-up on exposure to CVD prevention indicators (measurement, diagnosis and medication of physiological CVD risk factors, and medical advice about behavioural risk factors), as well as on health behaviour and predicted 10-year CVD risk score.

Results: Going for a check-up at a public clinic raised a weighted average of effect sizes of the prevention indicators by 0.16 (95% CI 0.06 to 0.26, FWER-corrected p = 0.0218). Disaggregated analyses revealed positive effects on blood pressure measurement and receipt of medical advice, but no significant effect on diagnosis or medication of either hypertension or diabetes/dyslipidaemia. Despite high baseline prevalence of CVD risk factors and increased receipt of medical advice, there were no significant effects after six months on health behaviour, physiological risk factors or CVD risk score.

Conclusion: Getting Filipinos to health clinics responsible for opportunistic CVD risk screening had a muted impact on exposure to CVD prevention and no significant impact on health behaviour and predicted CVD risk. Issuing well-founded protocols may be insufficient to ensure exposure to CVD prevention through routine clinic visits.

1. Introduction

Efforts to improve primary prevention of CVDs in lower- and middle-income countries (LMICs) often rely on opportunistic screening for risk factors at health clinics (WHO, 2013). Most notably, the World Health Organization (WHO) Essential Package of Noncommunicable Disease (NCD) Interventions for Primary Health Care in Low Resource Settings (PEN) sets out protocols for clinics to routinely assess adult patients for CVD risks (WHO, 2010, 2020). In addition to CVD risk assessment and screening, this package specifies criteria for lifestyle counselling, referral, and prescription of medicines, such as antihypertensives and statins, of proven efficacy. Building on this, the HEARTS package (WHO, 2016) aims to strengthen the primary care response to CVDs by stipulating clinical protocols and advocating a risk-based management approach set out in the PEN.

These protocols specify efficacious interventions that modelling suggests are highly cost-effective (Lim et al., 2007; WHO, 2017). Simulations indicate that a package of interventions that includes the PEN-prescribed medicines for prevention and treatment of ischaemic heart disease and stroke would be almost sufficient to hit the Sustainable Development Goals (SDG) target of a one-third reduction in premature mortality from NCDs in the 20 countries (including the Philippines) with the largest NCD burdens (Bertram et al., 2018). There is no evidence, however, on the extent to which a visit to a clinic that should be

* Corresponding author. Dept. of Applied Economics, Erasmus School of Economics, Erasmus University Rotterdam, P.O. Box 1738, 3000, DR Rotterdam, the Netherlands.
E-mail address: odonnell@ese.eur.nl (O. O'Donnell).

https://doi.org/10.1016/j.socscimed.2021.114194
Received 31 August 2020; Received in revised form 5 June 2021; Accepted 30 June 2021
Available online 3 July 2021

routinely following the PEN protocols is effective in raising exposure to CVD prevention.

Observational studies have described (often deficient) PEN implementation (Aye et al., 2020; Wangchuk et al., 2014) and have compared outcomes at PEN and non-PEN clinics (AlHelo and Elessi, 2019). But without inducing random exposure to a PEN clinic, these studies were not capable of identifying effects of visiting such a clinic. A systematic review of experimental study evidence on clinic-based CVD prevention interventions in LMICs found that dissemination of clinical guidelines did not always lead to their implementation and had inconsistent effects on blood pressure, glucose, and lipid outcomes (Lee et al., 2016). A cluster-randomized trial in China and Nigeria found that a precursor of the PEN protocol for CVD risk management was efficacious in reducing the blood pressure of hypertension patients (Mendis et al., 2010). However, more than half of the patients exposed to the package still had uncontrolled hypertension after 12 months, and there was no consistent effect on behavioural risk factors (Mendis et al., 2010). These effects were obtained under experimental conditions that required clinics to follow the protocol. Effectiveness could be even lower where there are gaps in implementation of the protocol (Breda et al., 2019; Peters et al., 2019).

This study aimed to establish by how much a visit to a clinic that should have been screening opportunistically as per the PEN protocol raised exposure to primary prevention of CVD and, consequently, reduced risk factors. In randomly selected communities in one province of the Philippines, we used the conditional offer of a money-prize lottery ticket to induce random variation in visits to public health clinics responsible for operating the PEN. This allowed us to estimate effects of a check-up visit on exposure to CVD prevention processes (tests, diagnoses, medication, and medical advice), health behaviours, CVD risk factors, and predicted CVD risk. We did not aim to evaluate the impact of introducing the PEN. Nor did we seek to identify impediments to its implementation. Rather, we aimed to quantify effects of visiting a clinic operating the package under real-world conditions.

2. CVD prevention in the Philippines

CVDs are the main cause of death in the Philippines, accounting for around one third of all deaths in 2017 – just above the global average (GBDCN, 2018). Between 1990 and 2017, the age-standardized CVD death rate rose by 15 percent in the Philippines, while it fell by 20 percent in Southeast Asia on average (GBDCN, 2018, see Appendix A). CVD mortality risk in the country moved from being equal to the Southeast Asia average at the beginning of this period to 40 percent above the average at the end (GBDCN, 2018). One study projects a doubling of the number of hypertension cases in the next 20 years (Ulep et al., 2020). The Philippines ranks 28th highest worldwide for premature, avertable NCD mortality (Martinez et al., 2020) (see Appendix A). If recent trends were to continue, it would be 2050 (not 2030) before the country would reach the SDG targeted reduction in NCD mortality (Bennett et al., 2018).

One half of all CVD-related deaths in the Philippines in 2018 were of individuals aged 40–69 years (Department of Health, 2020), which is close to the age group we studied (40–70). CVDs accounted for 39 percent of all deaths in this age group ((Department of Health, 2020)). Over a third of Filipinos aged 40–69 were estimated to have elevated blood pressure in 2015 (DOST-FNRI, 2016). About half of them reported no history of hypertension (DOST-FNRI, 2016). Among those who reported a history of hypertension, two thirds reported having been prescribed medication but only 54 percent reported taking medication (DOST-FNRI, 2016).

The demand for primary healthcare is projected to double in the Philippines in the next 20 years (Ulep et al., 2020). Within the public health system, health clinics – Rural Health Units and City Health Centres – are the initial contact points for primary care. While patients, irrespective of income or insurance cover, can get treatment and some medicines from these clinics free of charge, only half of the population can access a clinic within 30 min (Ulep et al., 2020). Many patients with chronic health problems bypass these clinics and seek treatment directly at public and private hospitals (Ulep et al., 2020). Only 4 percent of public health spending on NCDs goes to primary healthcare facilities (Ulep et al., 2020). The primary care benefit (PCB) package, which the Philippine Health Insurance Corporation (aka PhilHealth) introduced in 2012, still accounted for less than 1 percent of all insurance claims in 2017 (Ulep et al., 2020). Nationally, around 10 percent public health clinics do not have a doctor, and 15 percent have no nurse. In the region where this study was undertaken, the rates are lower (8% and 7%, respectively) (Ulep et al., 2020). Clinics often lack patient management targets and performance indicators (Ulep et al., 2020).

The Philippines authorized its version of the PEN in 2012 (Department of Health, 2012a, 2012b). PhilPEN established a protocol – the WHO protocol for the integrated management of diabetes and hypertension, see Appendix B – for progression of all public health clinic patients aged 25+ with no previously established CVD through *risk assessment*, *risk screening* (if at CVD risk, e.g., aged 40+), *referral* (e.g., if hypertensive and aged <40 or with symptoms of heart disease), *prediction* of global CVD risk score, and, depending on that score and individual risk factors, *medication* (of hypertension, diabetes and dyslipidaemia), and/or *lifestyle counselling*. The protocol applies to all patients irrespective of reason for presenting at a public clinic. The clinics operate Hypertension and Diabetes Clubs (HDC) that dispense maintenance medication (DOH, 2016). Clinics are required to provide HDC-registered patients with continuous health education on diet, physical activity, smoking cessation, and alcohol intake (DOH, 2016). Private clinics are not required to follow the PhilPEN protocol. One quarter of the study participants who visited a health clinic in the 30 days prior to the baseline survey attended a private clinic.

The Department of Health (DOH) had financial and operational responsibility for the provision of PhilPEN technical assistance and for training clinic staff in the operation of the protocol (DOH, 2012a). Local government units (LGUs) were obliged to adopt PhilPEN in their clinics and to finance any associated costs over and above those covered by DOH and PhilHealth.

The DOH stipulated the equipment and [provided forms clinics needed to conduct risk assessment and patient referral in accordance with the PhilPEN protocols (Department of Health, 2012a). It assigned responsibility to a sub-agency to supply public clinics with three antihypertensives (amlodipine, losartan, and metoprolol) and one diabetes medication (metformin) that the DOH was already committed to financing (Department of Health, 2011). Statins were not supplied (or financed) by the DOH, despite the protocol stipulating that they should be given to all patients with a CVD risk score in excess of 30% and to those with particularly high total cholesterol (≥8 mmol). From 2016, the DOH ordered PhilHealth to include in its PCB package NCD medicines that the DOH did not supply itself to clinics. Accredited clinics that dispensed such medicines would be reimbursed by PhilHealth. Irrespective of the source of finance for medicines available at clinics, they are dispensed to patients without charge. Each clinic submits a list of patients enrolled in the HDC and an inventory of medicines dispensed when making a request to the DOH regional office for replenishment of the clinic's stock of hypertension and diabetes medicines.

All public health clinics in the study province had medical staff who were trained in operation of the PhilPEN protocols, and all the clinics had received related materials, including target client record books. All clinics were equipped to measure body mass index, waist circumference and blood pressure. All could measure blood sugar using a glucometer but not via a fasting plasma glucose test. Only clinics in the cities and larger municipalities were capable of performing blood lipid profile tests on site. The others typically referred patients to local private (fee-charging) laboratories for these tests.

At the time of the study, PhilPEN advocated use of the WHO/International Society for Hypertension (ISH) charts (Mendis et al., 2007) to

identify those at elevated risk of CVDs. These charts tended to categorize a vast majority as low risk despite high prevalence of CVD risk factors (Dugee et al., 2013; Selvarajah et al., 2014). As a result, PhilPEN may well have been failing to deliver primary prevention of CVDs to many who could have benefited from it.

3. Study design

3.1. Evaluation approach

To identify effects of visits to clinics operating PhilPEN on exposure to CVD prevention processes, it would not have been sufficient to observe whether the protocol was followed when patients visited those

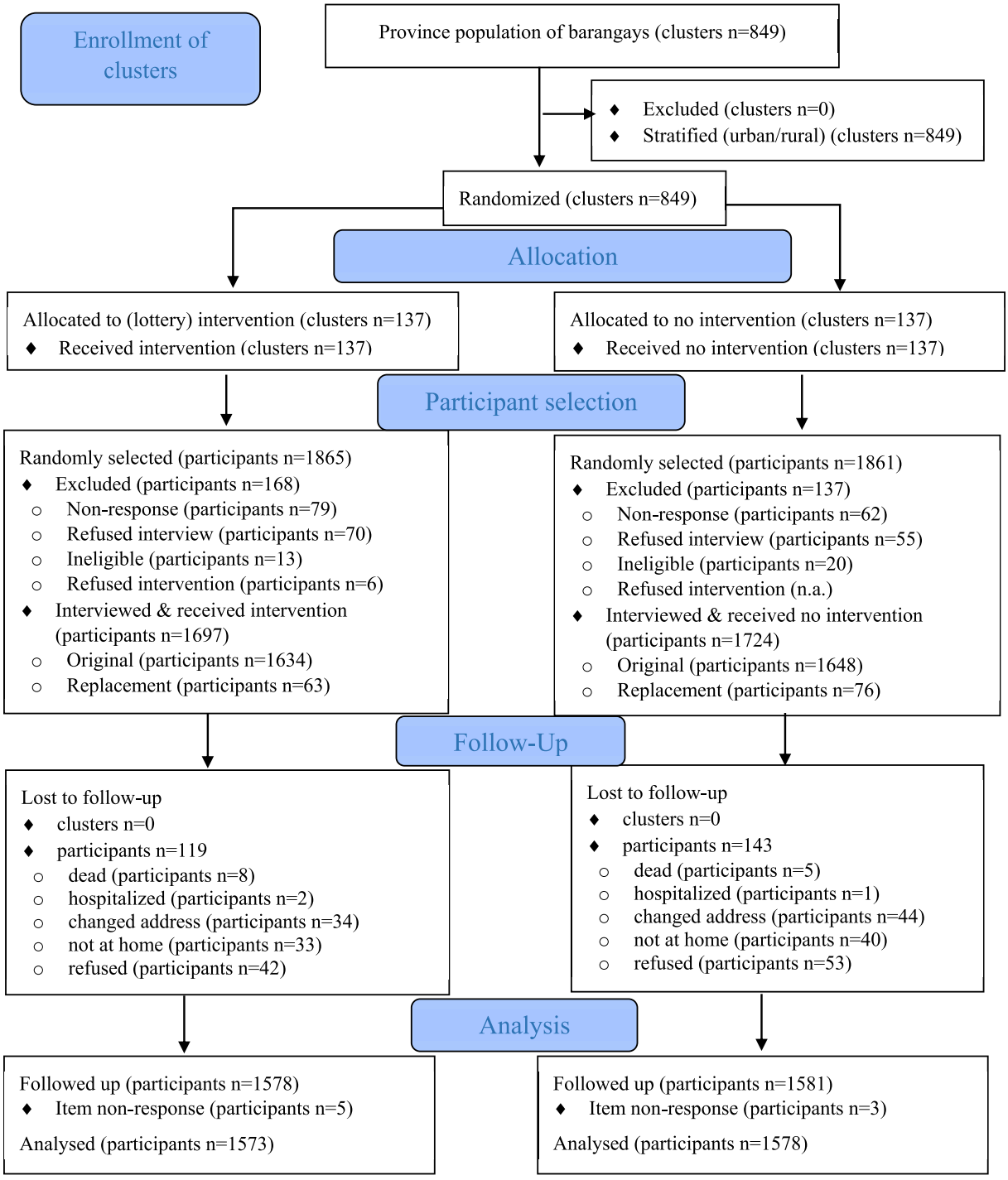

Fig. 1. Participant flow through cluster-randomized experiment. *Notes:* The 849 barangays in Nueva Ecija province were stratified by urban/rural and randomly drawn into the lottery intervention (137) or the control (137). "Replacement participants" were randomly selected to replace others with measurement errors that invalidated the calculated global CVD risk score at baseline. Households/participants that did not respond, refused or were ineligible at baseline were replaced with randomly selected others. All those lost due to item non-response at endline had measured blood pressure above 180 SBP or 120 DBP. Following the study's ethical protocol, the interview was stopped after such a reading and the participant was instructed to seek immediate medical attention. At baseline, any such cases were replaced by another respondent randomly selected from the same household or barangay.

clinics. These patients may have received tests, diagnoses, medicines, and medical advice even if they had not attended public clinics. Further, observation at clinics would not have identified effects on health behaviour and CVD risk factors, which depend on patient adherence to referrals, advice, and medication received from the clinics. Determining the difference that visits to public health clinics operating PhilPEN made to CVD prevention required comparison between individuals who had visited such clinics and other individuals who had not. However, without random variation in clinic attendance, health and other differences between these two groups would have confounded the effects of a visit. To avoid this, we made random offers of a lottery ticket conditional on presentation for a check-up at a public health clinic responsible for operating PhilPEN. We used the induced random variation in visits to such clinics to identify the effects of those visits on exposure to CVD prevention processes, as well as health behaviour and CVD risk factors.

3.2. Sampling and randomization

We conducted a cluster-randomized trial in Nueva Ecija, a landlocked, mainly rural province of 2.15 million people in Central Luzon (CL). We selected this province because of its proximity to Metro Manilla (180 km) and its higher than average prevalence (in 2015) of CVD risk factors – elevated blood pressure (27.4% (CL) vs 23.9% (national), smoking (24.9% vs 23.3%), overweight/obesity (32.7% vs 31.3%), unhealthy diet (94.8% vs 91.4%), physical inactivity (52.2% vs 42.5%), high total cholesterol (51.4% vs 47.2%) (DOST-FNRI, 2015; 2016). Poverty is also slightly higher - 22.6% (Nueva Ecija) vs 21.6% (national).

The province has 849 barangays – the smallest administrative unit in the Philippines, comprising a village or a few blocks in a town or city. We stratified all barangays by their official rural/urban classification, and randomly drew and randomly assigned an equal number (137) to an intervention group or a control group (Fig. 1). We set selection probabilities to achieve required group sizes and to maintain the province split between rural and urban barangays. The intervention group was offered a lottery ticket conditional on going for a check-up at a public health clinic. Randomization at the barangay level meant that there was no scope within a barangay for the lottery incentive to spill over to influence control participants' propensity to visit a clinic. Such contamination was unlikely, although not impossible, between barangays.

Within each randomly selected barangay, we used random interval sampling: we interviewed the household at a randomly selected location and those at intervals of ten households from that location. We returned to each selected household up to two times if there was no one at home. We replaced each non-response with another randomly selected household from the same barangay. Aiming for 10 households per barangay to be followed to endline, we interviewed around 12 households per barangay at baseline. Lower attrition than anticipated resulted in an average of 11.5 households per barangay at endline (Fig. 1).

Within each household, we interviewed one person reported as a) aged 40–70 years, b) without a diagnosis of diabetes or heart disease, c) not having suffered a heart attack or stroke, d) not currently taking medication for hypertension, and e) not having any health condition that would prevent measurement of height, weight or blood pressure. From the roster of all household members, we randomly selected one person who satisfied the age criterion. Then, we conducted a preliminary screening interview to establish if that person satisfied the other criteria. If the selected person was not at home, an interview was arranged for later. If the selected person could not be interviewed, refused, or did not meet the criteria, we randomly drew another person aged 40–70 from the household roster and repeated the process. If there was no one who satisfied a)-e), then we randomly selected another household from the same barangay. By restricting sample entry to individuals aged 40+, we ensured that all participants were eligible for full CVD risk screening, including tests for blood glucose, urine protein, and, if available, blood lipids, according to the PhilPEN protocol.

3.3. Intervention

At the end of the baseline interview, we told all participants they could go for a check-up at a health clinic where a doctor or nurse could examine them, perform tests, and, if deemed necessary, prescribe medication. In addition, we offered all participants in each intervention group barangay a coupon giving entitlement to enter a lottery with a money prize of ₱5000 (~US$100) conditional on going for a check-up at the public health clinic that served that barangay. The coupon showed an expiry date around 6 weeks after the issue date (Appendix C; Figure C1). The prize was equivalent to approximately 14-days earnings at the regional minimum wage. We told the participants they did not have to pay to enter the lottery, they had a one in ten chance of winning, and there would one winner in their barangay. We instructed them to ask for a medical assessment at the clinic where the coupon would be exchanged for a lottery ticket (Appendix C; Figure C2). We asked them to take photographic ID to the clinic and to send a cellphone text message to the study team afterwards, although neither was a condition for entering a lottery.

At each public health clinic, a designated health worker (nurse or midwife) checked the ID (not necessarily photographic) of anyone turning up with a coupon (see Appendix C; Figure C3). Each coupon was uniquely numbered, carried the participant's name, and stated that the coupon was non-transferable. The health worker recorded the participant's personal details, including age, carried out medical assessment or referred the participant for assessment, and issued the lottery ticket. We randomly drew one winner from all the lottery tickets issued in each barangay (see Appendix C for details). Participants were notified by cellphone text whether they had won. Winners were paid by electronic money transfer or courier.

We asked the health workers to deal with study participants as they would any patient presenting at the clinic for medical assessment. When briefing the clinics, the study team made no mention of PhilPEN. They explained that the study – referred to as the *Nueva Ejica Cardiovascular Health Study* – aimed to improve understanding of people's health behaviour, partly through observing response to the lottery incentive for visiting a clinic. On average, the lottery induced around seven participants per barangay to visit a clinic within a six-week period. Each clinic served two intervention group barangays, on average. Hence, the intervention did not noticeably increase the clinic workload. There were 63 public health clinics that served the 274 study barangays. Almost all of these clinics served both intervention group and control group barangays.

3.4. Timing

We conducted baseline interviews in January–May 2018 (93% January–March, 0.5% May). We conducted endline interviews in September–November 2018, following a schedule designed to leave at least six months from the baseline interview of each participant. The average time between interviews was 32 weeks (7.5 months). All outcomes were measured in the endline survey. For both the intervention and control groups, we used endline survey self-reports to create an indicator of whether each participant had visited a public health clinic since baseline. We excluded visits in the month before the endline interview since these could not have been induced by the lottery offer.

3.5. Outcomes

We pre-specified a number of outcomes along a hypothesized causal chain from a check-up visit to predicted CVD risk. Closest in proximity to a clinic visit, we examined CVD prevention processes specified in the PhilPEN protocol (Appendix B) – *measurement* of blood pressure, blood sugar and cholesterol, *diagnosis* of hypertension, diabetes, dyslipidaemia and heart disease, *medication* of these conditions, *medical advice* about CVD-relevant health behaviours. Table 1 gives the definition of each

Table 1
Outcomes.

Outcome		Definition and measurement
Outcomes are measured for all participants and are self-reported (except S-Zz) at follow-up		
Measurement		
A	Blood pressure (BP)	Had BP measured by medic since baseline
B	Blood sugar or cholesterol	Had blood sugar or cholesterol measured by medic since baseline
Diagnosis		
C	Hypertension	Ever diagnosed as hypertensive by medic
D	Undiagnosed hypertension	Measured systolic BP $\geq$ 140 and/or diastolic BP $\geq$ 90 and never been diagnosed as hypertensive by medic
E	Diabetes, dyslipidaemia or heart disease	Ever diagnosed with diabetes, dyslipidaemia or heart disease by medic
Medication		
F	Hypertension	Taken prescribed medication for hypertension since baseline
G	Diabetes or dyslipidaemia	Taken prescribed medication for diabetes or dyslipidaemia since baseline
Medical advice		Since baseline, received advice from medic to:
H	Quit smoking	quit smoking
I	Less alcohol	drink less alcohol
J	Less salt and fat	eat less salty and fatty foods
K	More fruit, veg. & pulses	eat more fruit, vegetables & pulses
L	Lose weight	lose weight
M	More exercise	do more physical exercise
Health behaviour		
N	Smoker	Currently smokes tobacco
O	Heavy episodic drinker	Drunk at least 4 (female)/5 (male) units alcohol on $\geq$1 occasion in last 30 days
P	Fruit & vegetables	(# days in 7 eats fruit + # days in 7 eats vegetables)/2
Q	No salt	Never adds salt or salty sauce to food
R	Physically active	Physically active for at least 30 min on typical day
CVD risk factors		
S	Systolic BP (SBP)	Average of last two (out of three) measurements on single occasion
T	Hypertension	SBP $\geq$140 and/or diastolic BP $\leq$ 90
U	Body mass index (BMI)	Measured weight (kg)/measured height $(m)^2$
V	Overweight	BMI $\geq$ 25
W	Waist circumference	Measured in cm
X	Central obesity	Waist circumference $\geq$ 80 cm (female)/90 cm (male)
Global CVD risk		
Y	CVD risk score	Percentage chance of heart attack or stroke within 10 years predicted from age, sex, SBP, BMI and smoking using office-based Globorisk
Z	Elevated CVD risk	CVD risk score $\geq$ 10%
Zz	High CVD risk	CVD risk score $\geq$ 20%

Notes: Outcomes in categories *Measurement, Diagnosis*, *Medication, Medical advice* and *Health behaviour* were reported by the participant in the follow-up survey. *CVD risk factors* were measured in that survey. *Global CVD risk* was predicted from measured risk factors (and reported smoking). Blood pressure and anthropometry were measured during the survey. "Medic" refers to a doctor or health worker.

outcome. The medication outcomes, which indicate whether the participant took prescribed medicines since baseline, reflect participant adherence behaviour, as well as clinic actions in prescribing or dispensing medicines. Participants could have obtained tests, diagnoses, medicines, and medical advice through contact with healthcare providers other than a public health clinic. We estimated marginal effects of visiting such a clinic on exposure to CVD prevention.

If visiting a clinic led to diagnosis and/or receipt of medical advice, then participants may have changed their health behaviours. We estimated effects on smoking, alcohol consumption, diet, and exercise (Table 1). Any change in health behaviour, together with medication, may have impacted on physiological CVD risk factors related to blood pressure and anthropometry. We measured both in the endline survey and derived a number of indicators from them (Table 1). Change in behavioural and physiological risk factors would feed through to the predicted risk of CVD: the probability of having a fatal or non-fatal heart attack or stroke within ten years. We predicted this using the office version of *Globorisk* from age, sex, systolic blood pressure (SBP), body mass index (BMI), and smoking status (Ueda et al., 2017) (see Appendix D).

All outcomes except those derived from measured blood pressure, anthropometry, and the CVD risk score were self-reported.

3.6. Power

We powered the study to detect small effects of a clinic visit at 80% power with 5% level of significance (see Appendix E). For continuous outcomes, we defined a small minimum detectable effect (MDE), conventionally, as one fifth of a standard deviation ($d = MDE/std.\ dev. \leq 0.2$) (Cohen, 1992). For binary outcomes, we also followed convention and defined a small MDE as Cohen's $h \leq 0.2$, where h is the absolute difference between the arcsine transformations of the intervention and control group proportions (Cohen, 1992). We established that almost every parameter value assumed in the sample size calculations turned out to be either reasonably close to the respective value realized in the control group, or was biased in a direction that would have resulted in the study having greater power than was planned (Table E1). Using the actual size of the endline analysis sample and the realized (control group) values of parameters, we confirmed that the targeted small MDE ($d \leq 0.2, h \leq 0.2$) was achieved for all outcomes for which ex ante power analysis was performed (Table E2).

With the exception of a few outcomes, the study had the power to detect the small effects it was intended to test for. One caveat is that the ex ante power analysis did not adjust for multiple testing of effects on many outcomes, although we did take this into account in the analyses (section 4.2).

4. Data analysis

4.1. Identification and estimation

The study aimed to estimate effects of visiting a public health clinic responsible for operating PhilPEN on exposure to CVD prevention processes, health behaviour, and CVD risk factors. This was achieved by identifying the variation in each outcome that was associated only with public health clinic visits that were induced by the random lottery offer. We estimated models with the following general structure,

$$Y_{1i}^{*} = \rho VISIT_i + \lambda Y_{0i} + \mathbf{X}_{0i}\boldsymbol{\beta} + \varepsilon_i \quad (1)$$

$$VISIT_i^{*} = \gamma LOTTERY_i + \theta Y_{0i} + \mathbf{X}_{0i}\boldsymbol{\delta} + u_i \quad (2)$$

where Y_{1i}^{*} is a latent outcome at endline that either corresponds to an observed continuous outcome, $Y_{1i} = Y_{1i}^{*}$, or determines an observed binary outcome by its sign, $Y_{1i} = 1[Y_{1i}^{*} > 0]$, where $1[]$ is the indicator function. $VISIT_i$ is a binary indicator of having visited a public health clinic between the baseline interview and one month before the endline interview. This is determined by (2), with either $VISIT_i = VISIT_i^{*}$ or $VISIT_i = 1[VISIT_i^{*} > 0]$ depending on the estimator used. $LOTTERY_i$ is an indicator of belonging to the randomly selected intervention group that was offered a lottery ticket conditional on visiting a clinic for a check-up. We controlled for the baseline value of the outcome (Y_{0i}) and a vector of baseline covariates $(\mathbf{X}_0)$ that included demographics, health indicators, family CVD history, healthcare utilization, health insurance, urban/rural, education, employment, and "wealth" quintile groups. The latter were formed from principal components analysis of housing conditions, water supply, sanitation, household durables, assets, and receipt of formal and informal transfers. For any outcome reported for the period between baseline and endline, the "baseline value" corresponded to ever having experienced that outcome. ε_i and u_i are error terms, each assumed to be correlated across participants within a barangay.

For continuous outcomes, we set $Y_{1i} = Y_{1i}^{*}$ and estimated (1) by two-stage least squares (2SLS), with $VISIT_i$ instrumented by $LOTTERY_i$ using (2) (with $VISIT_i^{*}$ replaced by $VISIT_i$). The resulting estimate of ρ is the estimated local average treatment effect (LATE) of a clinic visit – the effect on those who would be induced by the lottery offer to visit a clinic for a check-up (Angrist and Pischke, 2009, p.155).

For binary outcomes, we assumed ε_i and u_i to be jointly normally distributed, estimated a bivariate probit model in which (1) and (2) are the linear latent indices and obtained the averaged marginal effect of $VISIT_i$ on the probability of a positive outcome, which corresponds to an estimate of the average treatment effect (ATE) under the distributional assumption (Angrist and Pischke, 2009, p.p. 199–204). It is important to recognize that we were not simply estimating the probability of having blood pressure measured, hypertension diagnosed, etc., conditional on visiting a clinic. These events could have occurred without going to a public health clinic. We estimated the extent to which a check-up visit to such a clinic, which was responsible for operating PhilPEN, raised the probability of experiencing CVD prevention practices that are specified in the protocol of that package.

4.2. Inference

Standard errors were adjusted for clustering at the level of randomization to the intervention – the barangay – and control was made for stratification through inclusion of an indicator of urban/rural in models.We took two approaches to dealing with the multiple comparisons problem. The first was to aggregate outcomes into three summary indices of *clinic-centred* outcomes (Table 1, outcomes *a-c* and *e-m*), *health behaviour* outcomes (Table 1, outcomes *o-r*), and the global 10-year CVD risk score (Table 1, outcome *y*). Each of the first two indices was a weighted average of the effect sizes of the outcomes entering that index (see Appendix F). The third index was calculated from CVD risk factors using the Globorisk algorithm (Ueda et al., 2017). The effect of a check-up clinic visit on each summary index was estimated by 2SLS. Our second approach was to estimate effects on the separate outcomes listed in Table 1 and to correct for multiple comparisons through control of the False Discovery Rate (FDR) using the Benjamini et al. (2006) two-stage procedure to produce sharpened q-values (Anderson, 2008).

Table 2
Balance of analytical sample on baseline outcomes.

		Baseline mean [SD]		H0: (1)=(2)	Normalized
		Control	Intervention	p-value	difference
		(1)	(2)	(3)	(4)
Measurement					
a	Blood pressure (BP)	0.805	0.803	0.929	0.005
b	Blood sugar or cholesterol	0.267	0.310	0.043	−0.096
Diagnosis					
c	Hypertension	0.036	0.045	0.190	−0.046
d	Undiagnosed hypertension	0.245	0.263	0.277	−0.041
e	Diabetes, dyslipidaemia or heart disease	0.021	0.029	0.157	−0.053
Medication					
f	Hypertension	0.025	0.028	0.575	−0.020
g	Diabetes or dyslipidaemia	0.015	0.018	0.417	−0.030
Medical advice					
h	Quit smoking	0.089	0.076	0.235	0.045
i	Less alcohol	0.056	0.044	0.130	0.055
j	Less salt and fat	0.221	0.226	0.776	−0.012
k	More fruit, veg. & pulses	0.304	0.322	0.395	−0.040
l	Lose weight	0.080	0.091	0.373	−0.037
m	More exercise	0.054	0.067	0.168	−0.054
Health behaviour					
n	Smoker	0.271	0.272	0.933	−0.003
o	Drinker	0.249	0.254	0.814	−0.011
p	Fruit & vegetables	4.05 [2.37]	3.98 [2.44]	0.416	0.043
q	No salt	0.272	0.319	0.069	−0.102
r	Physically active	0.613	0.673	0.075	−0.125
CVD risk factors					
s	Systolic BP (SBP)	125.2 [20.8]	125.1 [20.3]	0.934	0.003
t	Hypertension	0.264	0.284	0.235	−0.046
u	Body mass index (BMI)	23.0 [5.11]	23.3 [4.94]	0.165	−0.061
v	Overweight	0.291	0.315	0.213	−0.052
w	Waist circumference	85.9 [12.5]	85.9 [12.0]	0.943	−0.003
x	Central obesity	0.497	0.510	0.540	−0.025
Global CVD risk					
y	CVD risk	10.98 [7.79]	10.98 [8.20]	0.999	−0.000
z	Elevated CVD risk	0.471	0.451	0.270	0.040
zz	High CVD risk	0.117	0.125	0.524	−0.023
n clusters		137	137		
n participants		1578	1573		
F-statistic joint significance				1.463	(n = 3151)

Notes: Columns (1) and (2) give means in Control group (not offered lottery ticket) and Intervention group (offered lottery ticket) at baseline in sample used to produce estimates, i.e. after attrition and item non-response at endline. Standard deviations [SD] of continuous variables in brackets. Column (3) gives p-values from t-tests of equal means. Column (4) gives difference in means divided by the square root of the sum of the group variances. F-statistic is for test of joint significance of all the outcomes used to explain the intervention indicator. Variable definitions in Table 1. For any outcome that was defined for the period between baseline and endline, this table shows the mean at baseline for ever having experienced the outcome.

5. Results

5.1. Preliminary analyses

Out of 1865 and 1861 households approached in the intervention and control groups, respectively, 168 and 137, respectively, did not respond, refused, or were ineligible (Fig. 1). There were 1697 participants in the intervention group and 1724 in the control group at baseline. Enumerator errors invalidated the (anthropometry) data of some participants (63 in intervention group, 76 in control group). Each was replaced by someone randomly selected from the same barangay. Attrition and item non-response at endline resulted in the loss of 124 (7.3%) participants from the intervention group and 146 (8.5%) from the control group ($p = 0.2139$).

The intervention and control groups in the analysis sample were very well balanced. There was only one significant (5% level) group difference in the baseline mean values of the 27 outcomes (Table 2) – less than would be expected by chance. For the one outcome with a significant difference – whether blood sugar or cholesterol had been measured – the magnitude of the normalized difference was well below the 0.25 threshold often taken as indicative of imbalance (Imbens and Rubin, 2015). This was also true for each of the two health behaviour outcomes – never adding salt to food and daily physical activity – with significantly different means at the 10% level. The intervention and control groups differed significantly (5% level) in the baseline mean values of only two out of 23 covariates, and the magnitude of the normalized difference exceeded 0.1 (but not 0.25) for only one (public health clinic visits) of these covariates (Table 3). Around two-thirds of the sample was female, almost three quarters rural dwelling, around two fifths had no more than elementary education, and more than two thirds had public health insurance (Table 3).

5.2. Effects on aggregated outcomes

Table 4 presents 2SLS estimates of effects of a check-up clinic visit on the three summary indices of clinic-centred, health behaviour, and global CVD risk outcomes. The first-stage estimates in the bottom panel indicate that the conditional offer of a lottery ticket had a large effect on clinic attendance. It raised the probability of visiting a clinic between baseline and endline by around 47 percentage points (pp) from a control group probability of 14 percent. The F statistics confirm that the lottery offer was a strong instrument. The estimate in column (1) of the top panel indicates that a clinic visit induced by the lottery offer raised the weighted average of the effect sizes of the clinic-centred outcomes by 0.16 (95% CI 0.06 to 0.26). This positive effect on exposure to CVD prevention through a combination of biomarker measurement, risk factor diagnosis and medication, and medical advice remains significant at the 5% level after family-wise error rate (FWER) correction for multiple testing (Romano & Wolf, 2005, 2016).

Using a summary index of measured CVD risk factors – blood pressure/hypertension, BMI/overweight and waist circumference/central obesity – instead of the global CVD risk score, and moving smoking to the health behaviour summary index, did not change the conclusion that visiting a clinic raised exposure to clinic-centred CVD prevention process indicators but did not appear to have any impact on CVD-related health behaviour or physiological risk factors (Appendix G, Table G1).

Table 3
Balance of analytical sample on baseline covariates.

	Baseline mean [SD]		H_0: (1)=(2)	Normalizeddifference
	Control	Intervention	p-value	
	(1)	(2)	(3)	(4)
Male	0.342	0.319	0.251	0.049
Age (years)	52.4 [8.88]	52.2 [8.67]	0.529	0.024
Married or cohabiting	0.810	0.790	0.178	0.051
Urban	0.270	0.257	0.815	0.028
Worked in last 7 days	0.570	0.582	0.643	−0.023
Education, highest level				
<elementary	0.143	0.124	0.215	0.055
Elementary	0.297	0.266	0.110	0.070
some high school	0.187	0.167	0.196	0.052
high school graduate	0.261	0.305	0.029	−0.098
College	0.112	0.138	0.106	−0.078
Wealth quintile group				
Poorest	0.215	0.193	0.300	0.055
2nd poorest	0.206	0.200	0.750	0.014
Middle	0.205	0.196	0.634	0.021
2nd richest	0.197	0.194	0.859	0.008
Richest	0.177	0.216	0.085	−0.099
Arthritis, rheumatism, osteoporosis	0.183	0.170	0.483	0.035
Angina, cancer, lung or neurological disease	0.082	0.090	0.456	−0.028
SF-20 health-related quality of life	84.7 [17.2]	85.2 [17.1]	0.446	−0.047
Relative with hypertension, cholesterol or diabetes	0.676	0.668	0.687	0.018
Public health insurance (PhilHealth) cover	0.681	0.680	0.962	0.002
# visits to public health clinic last 6 months	0.25 [1.04]	0.18 [0.68]	0.014	0.114
Outpatient visit last 30 days	0.081	0.077	0.726	0.016
Inpatient admission last 12 months	0.014	0.022	0.121	−0.058
n clusters	137	137		
n participants	1578	1573		
F-statistic joint significance			1.391 (N = 3151)	

Notes: Columns (1)–(4) as in notes to Table 2. F-statistic is for test of joint significance of all the covariates used to explain the intervention indicator. *Wealth quintile groups* formed from first principal component of house ownership, materials, size, amenities and state of repair, water source, sanitation, household durables, e.g. television, car, etc., ownership of assets (houses, land, agricultural, business, financial), receipt of remittances and conditional cash transfer. *Arthritis* etc., refers to reported diagnosis of any of these conditions. *Angina* etc. Refers to symptoms of angina identified from the Rose Questionnaire or diagnosis of cancer, lung disease, asthma, COPD, neurological or psychiatric disorder/disease. *SF-20* is the mean of the scores on the six components of SF-20 (Stewart et al., 1988; Hays et al., 1995). *Relative* is a parent or sibling. A public health clinic is a rural health unit, city health center or barangay health post. Number (#) of visits to a public health clinic is censored at 4.

Table 4
Effects of check-up clinic visit on aggregated outcomes.

	Clinic-centred	Health behaviour	Global CVD risk
	(1)	(2)	(3)
LATE	0.1562	0.0337	0.0055
(SE)	(0.0508)	(0.0648)	(0.2567)
Naïve p-value	0.0021	0.6038	0.9829
FWER p-value	0.0218	0.7500	0.9840
First stage	0.4732	0.4741	0.4746
(SE)	(0.0216)	(0.0216)	(0.0215)
F-statistic	489.4	490.1	493.2
n clusters	274	274	274
n participants	3151	3151	3151

Notes: First row gives 2SLS estimates of effects of a clinic visit. Column (1) and (2) are for weighted averages of effect sizes of outcomes a-c + e-m and o-r in Table 1, respectively. Control group means of these outcomes are zero by construction. Column (3) is for predicted 10-year CVD risk. Control group mean of this outcome is 10.9. FWER p-value is adjusted for multiple testing using the Romano and Wolf (2005, 2016) implemented in Stata® using rwolf (Clarke et al., 2019) with 10,000 bootstrap replications. Naïve p-value is unadjusted. First stage is OLS estimate of effect of lottery offer on probability of a clinic visit. F-statistic is the robust Kleibergen-Paap (2006) Wald rk F statistic of instrument strength. Standard errors in parentheses are adjusted for clustering at the level of randomization. All models include the baseline values of the outcome and the covariates listed in Table 3, except that age and sex are controlled through sex-specific 5-year age group indicators.

Estimates were also highly robust to instrumenting visits with their predicted probabilities from a probit model for the first stage (Table G2, panel A) and to not controlling for covariates and the baseline outcome (Table G2, panel B).

5.3. Effects on specific outcomes

Table 5 presents estimated effects on specific outcomes. Effects on continuous outcomes are LATEs estimated by 2SLS. Effects on binary outcomes are ATEs estimated by bivariate probit. Without correction for multiple testing, visiting a clinic for a check-up was estimated to significantly (10% level) raise seven of the twelve outcomes that entered the clinic-centred summary index. Specifically, there were significant effects on having blood pressure measured by a medic (17 pp), being diagnosed with hypertension (3.5 pp), and receiving all but one type of medical advice about CVD-related health behaviour (2.9–12.8 pp). However, after correcting for multiple testing, only the effects on blood pressure measurement and advice to eat a healthier diet remained significant (10% level).

With regard to the health behaviour outcomes, there was a significant (10% level) positive effect only on abstinence from adding salt to cooked food. While the effect of 8.5 pp was large relative to the control group mean (22 pp), its significance was not robust to correcting for multiple testing. There was no effect on physiological CVD risk factors. The point estimate of the effect on elevated CVD risk is negative and would imply a 2 pp reduction in the probability of the risk exceeding 10%, or a 4% relative reduction in the probability under the counterfactual of no clinic visit. However, this effect was not remotely significant, while there was a positive estimated effect on the probability of having high CVD risk that was significant without adjusting for multiple testing.

The estimates were robust to a) estimating effects on binary outcomes by 2SLS following the same procedure that was used for continuous outcomes, b) estimating effects on all outcomes by 2SLS with a clinic visit instrumented by the predicted probability from a probit, and c) not controlling for the baseline outcome and covariates (Appendix G, Tables G4a and G4b). Exceptions were that the 2SLS estimates of the effects on having blood pressure measured, and on receipt of medical advice to quit smoking, eat a healthier diet, and lose weight all remained significant at the 5% level after correction for multiple testing.

6. Discussion

6.1. Interpretation and implications

The estimated effects on aggregated outcomes suggest that going to a public health clinic for a check-up increased exposure to CVD prevention through some combination of measurement, diagnosis, and medication of risk factors, as well as medical advice. A check-up was estimated to increase the weighted average of effect sizes of clinic-centred prevention indicators by 0.16 of a standard deviation. Disaggregated analyses pinpointed increased likelihoods of having blood pressure measured (by 17 pp) and receiving advice on healthy living (2.9–12.8 pp) as responsible for the effect. Even then, most of the medical advice estimates were only on the margins of significance after adjusting for multiple testing.

There is also no evidence that going for a check-up raised the likelihood of having blood sugar or cholesterol measured, or that it increased diagnosis and medication of diabetes or dysplipidemia. All the public health clinics within the study province had glucometers to test blood sugar. Around 10% of all participants who visited a public clinic between the baseline and endline reported having their blood sugar measured at the clinic, while another 9% were referred for a blood sugar test (see Appendix Table G5). Only the public clinics in the cities and large municipalities of the province were equipped to perform lipid tests. The others, like most public health clinics in the Philippines (Ulep et al., 2020), would have to fulfill the PhilPEN protocol regarding measurement of cholesterol (Appendix B) by referring patients to private laboratories. Only around 6% of study participants who visited a public clinic were referred for a cholesterol test (Appendix Table G5). The lack of any effect of a clinic visit on the likelihood of having blood sugar or lipids measured implies that those who were tested at a clinic, or who were referred for a test, would have been tested anyway, or that these patients did not follow through on the referral, possibly because they were deterred by charges they would incur at a private clinic.

In principle, clinics were supplied with diabetes medication and should have dispensed it without charge, irrespective of the patient's health insurance cover. While the PhilPEN protocol stipulates prescription of statins to patients found to be at high CVD risk, the DOH implementation of the programme did not include supplying clinics with these medicines.

Notwithstanding the fact that effects on most types of medical advice fell just short of significance after correcting for multiple comparisons, the pattern of results suggests that visiting a clinic for a check-up did raise the likelihood of receiving some kind of advice on healthy living. There was little or no evidence of any consequent change in smoking, drinking, diet or exercise, however. A reduced likelihood of adding salt to cooked food was a possible exception to the lack of effect on health behaviour. It is unlikely that these null effects were attributable to a lack of time (6 months) for behaviour to change in response to advice issued at clinics and to repeated health education provided to patients who registered with Health and Diabetes Clubs following risk screening. A more plausible explanation, besides the sheer difficulty of ever inducing change in health habits, is that the effects on receipt of medical advice, while large relative to the very low base levels, were small in the context of highly prevalent unhealthy behaviours. For example, more than a quarter of the control group smoked. Yet going for a check-up was estimated to raise the probability of receiving medical advice to quit smoking by less than 4 pp.

Given the lack of effects on health behaviours and a muted, if any, effect on use of antihypertensives, it is understandable that there were no effects on physiological CVD risk factors and predicted CVD risk. Reasonably tight confidence interval estimates suggest that effects on the separate risk factors were not left undetected due to lack of power. Bringing adults aged 40+ into contact with clinics charged with

Table 5
Effects of a check-up clinic visit on specific outcomes.

		Effect	(SE)	Naïve p-value	FDR q-value	Control mean	Wald IV strength
		(1)	(2)	(3)	(4)	(5)	(6)
Measurement							
a	Blood pressure (BP)	0.1699	(0.0597)	0.0048	0.0640	0.4233	404.9
b	Blood sugar or cholesterol	−0.0028	(0.0243)	0.9091	1.0000	0.1071	402.0
Diagnosis							
b	Hypertension	0.0354	(0.0220)	0.0846	0.2420	0.0488	403.5
d	Undiagnosed hypertension	−0.0108	(0.0295)	0.7146	1.0000	0.2376	404.9
e	Diabetes, dyslipidaemia or heart disease	−0.0122	(0.0126)	0.3341	0.6970	0.0425	403.3
Medication							
f	Hypertension	0.0221	(0.0171)	0.1727	0.3640	0.0336	407.3
g	Diabetes or dyslipidaemia	−0.0065	(0.0093)	0.4737	0.9010	0.0247	403.7
Medical advice							
h	Quit smoking	0.0357	(0.0177)	0.0182	0.1170	0.0241	404.5
i	Less alcohol	0.0173	(0.0115)	0.1121	0.2900	0.0184	404.9
j	Less salt and fat	0.0684	(0.0315)	0.0243	0.1260	0.1071	402.7
k	More fruit, veg. & pulses	0.1280	(0.0408)	0.0008	0.0220	0.1229	401.5
l	Lose weight	0.0464	(0.0203)	0.0119	0.1060	0.0336	404.3
m	More exercise	0.0287	(0.0163)	0.0610	0.2250	0.0368	402.6
Health behaviour							
n	Smoker	0.0019	(0.0179)	0.9172	1.0000	0.2643	407.6
o	Heavy episodic drinker	0.0066	(0.0249)	0.7895	1.0000	0.2028	406.7
p	Fruit & vegetables	−0.0322	(0.1587)	0.8394	1.0000	4.3403	492.5
q	No salt	0.0850	(0.0508)	0.0866	0.2420	0.2218	405.2
r	Physically active	0.0599	(0.0757)	0.4311	0.8590	0.5311	402.4
CVD risk factors							
s	Systolic BP (SBP)	−0.4381	(0.9676)	0.6507	1.0000	123.99	491.0
t	Hypertension	0.0036	(0.0292)	0.9028	1.0000	0.2592	404.0
u	Body mass index (BMI)	−0.0393	(0.1030)	0.7029	1.0000	22.977	488.5
v	Overweight	0.0089	(0.0186)	0.6314	1.0000	0.2858	403.3
w	Waist circumference	−0.0839	(0.4755)	0.8599	1.0000	84.938	491.8
x	Central obesity	0.0106	(0.0203)	0.6056	1.0000	0.4708	405.8
Global CVD risk							
z	Elevated CVD risk	−0.0225	(0.0245)	0.3559	0.6970	0.4683	404.3
zz	High CVD risk	0.0349	(0.0187)	0.0449	0.1870	0.1096	403.6

Notes: Column (1) gives bivariate probit estimates of ATEs of a clinic visit on binary outcomes (a-r, t, v, x-zz) and 2SLS estimates of LATEs of a clinic visit on continuous outcomes (s, u, w). In both cases, a clinic visit was (effectively) instrumented with the randomized conditional offer of a lottery ticket. Column (2) gives standard errors adjusted for clustering at the level of randomization of the instrument. All models also included the baseline values of the outcome and the covariates listed in Table 3, except that age and sex are controlled through sex-specific 5-year age group dummies. Column (3) gives unadjusted p-values. Column (4) gives Benjamini et al. (2006) sharpened q-values that adjusted for multiple testing by controlling the False Discovery Rate (Anderson, 2008). Column (5) gives the mean of the outcome in the control group at endline. Column (6) gives the Wald test statistic for instrument strength. For binary outcomes, this is $\sim\chi^2(1)$. For continuous outcomes, it is the robust Kleibergen-Paap (2006) Wald rk F statistic. For all models, sample size is 276 clusters and 3151 individuals. See Appendix E, Table E3 for estimates of the effect of the lottery offer on clinic visit probability.

conducting opportunistic CVD screening appears to have been insufficient to reduce the risk of CVD.

The null effects on health behaviour, risk factors, and predicted CVD risk are consistent with the conclusions of a systematic review of evaluations of PEN-like multiple CVD risk factor interventions targeted mostly on high-risk populations in LMICs (Uthman et al., 2015). This found insufficient evidence to draw conclusions about effects on health behaviour, as well as on blood glucose and cholesterol, and only imprecise evidence of reductions in BP, BMI and waist circumference. Effects in general populations may be expected to be even more muted. Our findings are also consistent with a systematic review of evidence on clinic-based CVD prevention in LMICs that found that dissemination of clinical guidelines did not always lead to their implementation, and it had inconsistent effects on blood pressure, glucose, and lipid outcomes (Lee et al., 2016). This study adds to evidence that exposure to PEN-like protocols does not induce change in health behaviour (Mendis et al., 2010).

There is evidence from Central America, South-East Asia, and South Asia for the effectiveness of community-based, as opposed to clinic-based, primary prevention of CVD using outreach health workers (Jafar et al., 2009, 2020; Schwalm et al., 2019). Bringing CVD prevention closer to the population through sending low-cost paramedics with only rudimentary training into communities may be more effective than relying fully trained medics offering diagnosis, medication, and advice when someone makes a one-off visit to a clinic.

6.2. Limitations

We designed the study to have power to detect small effects. Ex post analysis suggested that it achieved the targeted power for most outcomes (Appendix E). But for some outcomes with low baseline prevalence, effects that were absolutely small could be considered relatively large. Given this, we are hesitant to conclude from insignificant effects on diagnosis and medication that going for a check-up did not raise the likelihood of being diagnosed with hypertension and receiving medication for it. The point estimate of the effect on hypertension diagnosis (3.5 pp) is almost three quarters of the low (reported) diagnosis rate in the control group. The ex post calculation of the MDE indicated that, even without correcting for multiple testing, the study was only powered to detect an effect on hypertension diagnosis almost as large as the control group mean. Similarly, while the estimated effect on medication of hypertension was not significant, particularly after correcting for multiple comparisons, the point estimate of 2.2 pp is two thirds of the control group mean (3.7 pp), and the MDE exceeded that mean (Appendix E; Table E2).

Small absolute effects on diagnosis and medication of hypertension would not, however, amount to a substantial improvement in CVD prevention from a low baseline. Even ignoring the lack of significance of the estimated effect on undiagnosed hypertension, naïve interpretation of the point estimate would suggest that a 24 percent probability of living with undiagnosed hypertension would have been reduced by only

1 pp as a result of going to a clinic for a check-up. Despite the high rate of undetected and uncontrolled hypertension, the magnitude of an insignificant point estimate would suggest, if its uncertainty were overlooked, that a check-up raised the probability of being prescribed antihypertensives by just over 2 pp. This would have increased the fraction of the population taking this medication to only around 5.5 percent, whilst 26 percent were estimated to be hypertensive.

Hypertension is preferably diagnosed from blood pressure measurements on multiple occasions. By using blood pressure measured (three times) on a single occasion at each of baseline and endline, we may have overestimated both hypertension prevalence and the rate of undiagnosed hypertension at the time of each survey. However, this measurement error need not have biased our estimates of the marginal effects of a clinic visit on these outcomes, particularly given that we controlled for the baseline value of the respective outcome.

For the study's randomized encouragement design to have identified effects of a check-up clinic visit on CVD prevention outcomes, those outcomes must not have varied with receipt of the lottery offer conditional on clinic attendance. This identification assumption would have been violated if clinics were to have treated patients with a lottery voucher differently from others presenting for a check-up. The direction of bias that would result from what would essentially be a Hawthorne effect is not clear. On the one hand, the Cardiovascular Health Study label and the fact that we instructed participants to ask for a medical assessment may have led clinic staff to apply the PhilPEN protocol more diligently than otherwise. In that case, we have overestimated the exposure to CVD prevention processes that typically results from visiting clinics responsible for operating PhilPEN. On the other hand, the clinics may have been more dismissive of patients they perceived as simply being interested in entering a lottery. In that case, we have underestimated the effectiveness of opportunistic screening based on the PhilPEN protocol. We aimed to minimize the risk of any such bias by instructing clinics to operate as normal. Also, we told them that the purpose of the study was to understand the health behavior of participants, not to evaluate the operation of PhilPEN. Comparison of the CVD prevention processes experienced by intervention and control group patients on any visit to a public health clinic between the baseline and endline revealed relatively few differences (Appendix G; Table G5). This does not lend support to a potential criticism that the study found only a muted effect of a check-up clinic visit on exposure to CVD prevention because clinics were more relaxed in following the PhilPEN protocol for study participants induced by the lottery offer.

Except for measured blood pressure, BMI and waist circumference, and the outcomes derived from these measures, all other outcomes were self-reported in the endline survey and were potentially subject to recall error. However, any such error would bias the estimated effect of a clinic visit on the outcome only if it differed systematically between the intervention group and the control group. There was no a priori reason to expect this. And there was no evidence of it from the intervention-control group comparison of recalled CVD prevention processes experienced at a clinic visit (Table G5). Random reporting error of similar magnitude in both groups would add noise and reduce the effective power to detect effects on reported outcomes. This is a further reason why insignificant effects on hypertension diagnosis and medication could possibly be false negative results.

The study's encouragement design could both potentially limit the extent to which its results generalize. The effects estimated on aggregated outcomes (Table 4) are local to the population who would be induced by a lottery to visit a public health clinic for a check-up. If the effects of a clinic visit on these compliers differ from effects on non-compliers, then the study did not estimate average effects in the population from which the sample was drawn. Intervention group participants were told to take photographic ID with them to clinics and to send a cellphone message afterwards. Although these were not conditions for entering the lottery, individuals without photo ID or access to a cellphone may have been discouraged from following up on the lottery offer. Fortunately, almost all adults in the study location had either voter ID or senior citizen ID, more than three quarters (76%) of the intervention group owned a cellphone and less than a tenth (9.5%) did not give the study team a cellphone number. Further, provided the joint normality assumption was valid, bivariate probit delivered estimates of average effects on individual outcomes (Table 5). Consistency between these estimates and estimates of local effects on the same outcomes obtained from 2SLS (Table G4a) suggests that any heterogeneity in the effects was limited.

The study was conducted in only one province of the Philippines. We are not aware of any difference between Nueva Ecija and other provinces in the operation of PhilPEN at public health clinics, however. Findings may not generalize to other countries.

7. Conclusion

This study demonstrated that going for a check-up at a public health clinic responsible for conducting opportunistic CVD risk screening using a protocol that is widely adopted throughout the developing world increased the exposure of Filipinos to CVD prevention processes only modestly. They were more likely to have had their blood pressure measured and to have received medical advice on unhealthy habits, but their probability of living with undiagnosed and uncontrolled hypertension fell little, if at all. They did not change behaviours that would lower exposure to CVD risk factors. Issuing clinics in the Philippines with well-founded protocols for CVD risk screening and management was not sufficient to ensure that predominantly poor individuals got markedly more of the diagnoses and medicines that are known to be effective in preventing cardiovascular diseases.

Authors' contributions

JC, AK and OO'D conceived the research objective and designed the study. JC and AK obtained approval from authorities, supervised the fieldwork and took responsibility for data collection and management. OO'D designed and conducted the data analysis, and drafted the paper. JC, AK and OO'D edited the draft. JC, AK and OO'D approved the final version of the paper.

Trial Registration

ClinicalTrials.gov NCT03512691, AEA Registry for Social Science experiments AEARCTR-0002867.

Data sharing

Data and code are available from the corresponding author.

Acknowledgements

We acknowledged the funder and research assistance in a separate file submitted to Editorial Manager. Here is the text from that file.Acknowledgements: Thanks to three referees for detailed comments, and also to Kayleen Calicdan, Benedict Evangelista Jr., Kristine Gloria, Jenny Kudymowa and Precious Montilla for excellent research assistance. The study was funded by the Swiss Agency for Development and Cooperation / National Science Foundation Programme for Research on Global Issues for Development, grant 400640_160374 (PI: Jürgen Maurer). The authors declare no conflicting interests.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.socscimed.2021.114194.

References

AlHelo, R., Elessi, K., 2019. Implementation of world health organization package of essential noncommunicable disease interventions for cardiovascular risk management in gaza/Palestine: a retrospective record review study. Dubai Med. J. 2, 153–157. https://doi.org/10.1159/000503992.

Anderson, M.L., 2008. Multiple inference and gender differences in the effects of early intervention: a reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects. J. Am. Stat. Assoc. 103 (484), 1481–1495.

Angrist, J.D., Pischke, J.-S., 2009. Most Harmless Econometrics: an Empiricist's Companion. Princeton University Press, Princeton, NJ.

Aye, L.L., Tripathy, J.P., Maung Maung, T., Oo, M.M., Nwe, M.L., Thu, H.M.M., Ko, K., Kaung, K.K., 2020. Experiences from the pilot implementation of the package of essential non-communicable disease interventions (PEN) in Myanmar, 2017-18: a mixed methods study. PloS One 15, e0229081.

Benjamini, Y., Krieger, A., Yekutieli, D., 2006. Adaptive linear step-up procedures that control the false discovery rate. Biometrika 93 (3), 491–507.

Bennett, J.E., Stevens, G.A., Mathers, C.D., Bonita, R., Rehm, J., Kruk, M.E., Riley, L.M., Dain, K., Kengne, A.P., Chalkidou, K., Beagley, J., Kishore, S.P., Chen, W., Saxena, S., Bettcher, D.W., Grove, J.T., Beaglehole, R., Ezzati, M., 2018. NCD Countdown 2030: worldwide trends in non-communicable disease mortality and progress towards Sustainable Development Goal target 3.4. Lancet 392, 1072–1088.

Bertram, M.Y., Sweeny, K., Lauer, J.A., Chisholm, D., Sheehan, P., Rasmussen, B., Upreti, S.R., Dixit, L.P., George, K., Deane, S., 2018. Investing in non-communicable diseases: an estimation of the return on investment for prevention and treatment services. Lancet 391, 2071–2078. https://doi.org/10.1016/S0140-6736(18)30665-2.

Breda, J., Wickramasinghe, K., Peters, D.H., Rakovac, I., Oldenburg, B., Mikkelsen, B., Shao, R., Varghese, C., Collins, T., Wall, K., Farrington, J., Townsend, N., 2019. One size does not fit all: implementation of interventions for non-communicable diseases. BMJ 367, l6434.

Clarke, D., Romano, J.P., Wolf, M., 2019. The Romano-Wolf Multiple Hypothesis Correction in Stata. IZA, Bonn. IZA DP 12845.

Cohen, J., 1992. A power primer. Psychol. Bull. 112 (1), 155–159.

Department of Health, 2011. Implementation Guidelines on the DOH Complete Treatment Pack (COMPACK) to Ensure Sustainable Access to Essential Drugs and Medicines for the Marginalized Sector, Administrative Order No. 2011-0013. Department of Health, Manila.

Department of Health, 2012a. Implementing Guidelines on the Instutionalization of Philippine Package of Essential NCD Interventions (PHIL PEN) on the Integrated Management of Hypertension and Diabetes for Primary Care Facilities, Administrative Order No. 2012-0029. Department of Health, Manila.

Department of Health, 2012b. Operations Manual on the Philippine Package of Essential NCD Interventions (PHIL PEN) on the Integrated Management of Hypertension and Diabetes for Primary Care Facilities. Department of Health, Manila.

Department of Health, 2016. Implementing Guidelines on the Organization of Health Clubs for Patients with Hypertension and Diabetes in Health Facilities, Administrative Order 2016-0014. Department of Health, Manila.

Department of Science and Technology - Food and Nutrition Research Institute (DOST-FNRI), 2015. Philippine Nutrition Facts and Figures 2013. In: 8th National Nutrition Survey Clinical and Health Survey. FNRI, Metro Manila.

Dugee, O., Oum, S., Buckley, B.S., Bonita, R., 2013. Assessment of total cardiovascular risk using WHO/ISH risk prediction charts in three low and middle income countries in Asia. BMC Publ. Health 13, 539.

Department of Health, 2020. The 2018 Philippine Health Statistics. Department of Health, Metro Manila. https://doh.gov.ph/sites/default/files/publications/2018%20Philippine%20Health%20Statistics.pdf.

Department of Science and Technology - Food and Nutrition Research Institute, 2016. Philippine Nutrition Facts and Figures 2015: Clinical and Health Survey. FNRI Metro Manila. http://enutrition.fnri.dost.gov.ph/site/uploads/2015_CLINICAL_AND_HEALTH_SURVEY.pdf.

Global Burden of Disease Collaborative Network, 2018. Global Burden of Disease Study 2017 Results. Institute for Health Metrics and Evaluation (IHME), Seattle, WA accessed July 27, 2020. http://ghdx.healthdata.org/gbd-results-tool?params=gbd-api-2017-permalink/c0318bd0400091a066f37f3d35c63a96.

Hays, R.D., Sherbourne, C.D., Mazel, R., 1995. User's Manual for the Medical Outcomes Study (MOS) Core Measures of Health-Related Quality of Life. RAND Corporation, Santa Monica, CA. https://www.rand.org/pubs/monograph_reports/MR162.html.

Imbens, G.W., Rubin, D.B., 2015. Causal Inference in Statistics, Social, and Biomedical Sciences: an Introduction. Cambridge University Press, New York, NY.

Jafar, T.H., Hatcher, J., Poulter, N., Islam, M., Hashmi, S., Qadri, Z., Bux, R., Khan, A., Jafary, F.H., Hameed, A., Khan, A., Badruddin, S.H., Chaturvedi, N., Hypertension Research Group., 2009. Community-based interventions to promote blood pressure control in a developing country: a cluster randomized trial. Ann. Intern. Med. 151 (9), 593–601. https://doi.org/10.7326/0003-4819-151-9-200911030-00004.

Jafar, T.H., Gandhi, M., de Silva, H.A., Jehan, I., Naheed, A., Finkelstein, E.A., Turner, E. L., Morisky, D., Kasturiratne, A., Khan, A.H., Clemens, J.D., Ebrahim, S., Assam, P.N., Feng, L., COBRA-BPS Study Group, 2020. A community-based intervention for managing hypertension in rural south Asia. N. Engl. J. Med. 382 (8), 717–726. https://doi.org/10.1056/NEJMoa1911965.PMID:32074419.

Kleibergen, F., Paap, R., 2006. Generalized reduced rank tests using the singular value decomposition. J. Econom. 133, 97–126.

Lee, E.S., Vedanthan, R., Jeemon, P., Kamano, J.H., Kudesia, P., Rajan, V., Engelgau, M., Moran, A.E., 2016. Quality improvement for cardiovascular disease care in low- and middle-income countries: a systematic review. PloS One 11, e0157036.

Lim, S.S., Gaziano, T.A., Gakidou, E., Reddy, K.S., Farzadfar, F., Lozano, R., Rodgers, A., 2007. Prevention of cardiovascular disease in high-risk individuals in low-income and middle-income countries: health effects and costs. Lancet 370 (9604), 2054–2062. https://doi.org/10.1016/S0140-6736(07)61699-7.

Martinez, R., Lloyd-Sherlock, P., Soliz, P., Ebrahim, S., Vega, E., Ordunez, P., McKee, M., 2020. Trends in premature avertable mortality from non-communicable diseases for 195 countries and territories, 1990-2017: a population-based study. Lancet Glob. Health 8, e511–e523.

Mendis, S., Lindholm, L.H., Mancia, G., Whitworth, J., Alderman, M., Lim, S., Heagerty, T., 2007. World Health Organization (WHO) and International Society of Hypertension (ISH) risk prediction charts: assessment of cardiovascular risk for prevention and control of cardiovascular disease in low and middle-income countries. J. Hypertens. 25, 1578–1582.

Mendis, S., Johnston, S.C., Fan, W., Oladapo, O., Cameron, A., Faramawi, M.F., 2010. Cardiovascular risk management and its impact on hypertension control in primary care in low-resource settings: a cluster-randomized trial. Bull. World Health Organ. 88 (6), 412–419.

Peters, D.H., Peters, M.A., Wickramasinghe, K., Osewe, P.L., Davidson, P.M., 2019. Asking the right question: implementation research to accelerate national non-communicable disease responses. BMJ 365, l1868.

Romano, J.P., Wolf, M., 2005. Exact and approximate stepdown methods for multiple hypothesis testing. J. Am. Stat. Assoc. 100 (469), 94–108.

Romano, J.P., Wolf, M., 2016. Efficient computation of adjusted p-values for resampling-based stepdown multiple testing. Stat. Probab. Lett. 113, 38–40.

Schwalm, J.-D., McCready, T., Lopez-Jaramillo, P., Yusoff, K., Attaran, A., Lamelas, P., Camacho, P.A., Majid, F., Bangdiwala, S.I., Thabane, L., Islam, S., McKee, M., Yusuf, S., 2019. A community-based comprehensive intervention to reduce cardiovascular risk in hypertension (HOPE 4): a cluster-randomised controlled trial. Lancet 394, 1231–1242.

Selvarajah, S., Kaur, G., Haniff, J., Cheong, K.C., Hiong, T.G., van der Graaf, Y., Bots, M. L., 2014. Comparison of the Framingham Risk Score, SCORE andWHO/ISH cardiovascular risk prediction models in an Asian population. Int. J. Cardiol. 176, 211–218.

Stewart, A.L., Hays, R.D., Ware, J.E., 1988. The MOS short-form general health survey: Reliability and validity in a patient population. Medical Care 26, 724–735.

Ueda, P., Woodward, M., Lu, Y., Hajifathalian, K., Al-Wotayan, R., Aguilar-Salinas, C.A., Ahmadvand, A., Azizi, F., Bentham, J., Cifkova, R., Di Cesare, M., Eriksen, L., Farzadfar, F., Ferguson, T.S., Ikeda, N., Khalili, D., Khang, Y.-H., Lanska, V., León-Muñoz, L., Magliano, D.J., Margozzini, P., Msyamboza, K.P., Mutungi, G., Oh, K., Oum, S., Rodríguez-Artalejo, F., Rojas-Martinez, R., Valdivia, G., Wilks, R., Shaw, J. E., Stevens, G.A., Tolstrup, J.S., Zhou, B., Salomon, J.A., Ezzati, M., Danaei, G., 2017. Laboratory-based and office-based risk scores and charts to predict 10-year risk of cardiovascular disease in 182 countries: a pooled analysis of prospective cohorts and health surveys. Lancet Diabetes Endocrinol. 5, 196–213. https://doi.org/10.1016/S2213-8587(17)30015-3.

Ulep, V., Uy, J., Casas, L.D., 2020. Primary Health Care for Non-communicable Diseases in the Philippines. Philippine Institute for Development Studies. https://pidswebs.pids.gov.ph/CDN/PUBLICATIONS/pidsdps2039.pdf.

Uthman, O.A., Hartley, L., Rees, K., Taylor, F., Ebrahim, S., Clarke, A., 2015. Multiple risk factor interventions for primary prevention of cardiovascular disease in low- and middle-income countries. Cochrane Database Syst. Rev. 8, CD011163. https://doi.org/10.1002/14651858.CD011163.pub2.

Wangchuk, D., Virdi, N., Garg, R., Mendis, S., Nair, N., Wangchuk, D., Kumar, R., 2014. Package of essential noncommunicable disease (PEN) interventions in primary health-care settings of Bhutan: a performance assessment study. WHO South-East Asia J. Publ. Health 3 (2), 154–160.

World Health Organization, 2010. Package of Essential Noncommunicable Disease Interventions for Primary Care in Low Resource Settings. WHO, Geneva.

World Health Organization, 2013. Global Action Plan for the Prevention and Control of Noncommunicable Diseases 2013–2020. WHO, Geneva.

World Health Organization, 2016. HEARTS: Technical Package for Cardiovascular Disease Management in Primary Care. WHO, Geneva. https://apps.who.int/iris/bitstream/handle/10665/252661/9789241511377-eng.pdf;jsessionid=0A8BDE969B795431D73050FC58124180?sequence=1.

World Health Organization, 2017. Tackling NCDs: 'best Buys' and Other Recommended Interventions for the Prevention and Control of Noncommunicable Diseases. WHO, Geneva accessed July 29, 2020. https://apps.who.int/iris/bitstream/handle/10665/259232/WHO-NMH-NVI-17%C2%B79-eng.pdf?sequence=1&isAllowed=y.

World Health Organization, 2020. WHO Package of Essential Noncommunicable (PEN) Disease Interventions for Primary Health Care. World Health Organization, Geneva. Licence: CC BY-NC-SA 3.0 IGO. https://www.who.int/publications/i/item/who-package-of-essential-noncommunicable-(pen)-disease-interventions-for-primary-health-care.