

**THE METHODOLOGICAL QUALITY AND EFFECTIVENESS OF  
TECHNOLOGY-BASED SMOKING CESSATION INTERVENTIONS FOR  
DISADVANTAGED GROUPS: A SYSTEMATIC REVIEW AND META-ANALYSIS.**

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## Abstract

**Aims:** To assess the methodological quality and effectiveness of technology-based smoking cessation interventions in disadvantaged groups.

**Method:** Four databases (EMBASE, Cochrane, Medline, and PsycInfo) were searched for studies conducted from 1980- May 2016. Randomised controlled trials that compared a behavioural smoking cessation intervention delivered primarily through a technology-based platform (e.g. mobile phone) with a no-intervention comparison group among disadvantaged smokers were included. Three reviewers assessed all relevant studies for inclusion, and one reviewer extracted study, participant and intervention-level data, with a subset crosschecked by a second reviewer.

**Results:** Thirteen studies targeting disadvantaged smokers ( $n = 4820$ ) were included. Only one study scored highly in terms of methodological rigour on EPOC criteria for judging risk of bias. Of the 13 studies using a technology-based platform, most utilised websites ( $n = 5$ ) or computer programs ( $n = 5$ ), and seven additionally offered nicotine replacement therapy. Technology-based interventions increased the odds of smoking cessation for disadvantaged groups at 1-month (OR 1.70, 95% CI 1.10, 2.63), 3-months (OR 1.30, 95% CI 1.07, 1.59), 6-months (OR 1.29, 95% CI 1.03, 1.62), and 18-months post-intervention (OR 1.83, 95% CI 1.11, 3.01).

**Conclusion:** Few methodologically rigorous studies were identified. Mobile phone text-messaging, computer- and website-delivered quit support showed promise at increasing quit rates among Indigenous, psychiatric and inpatient substance use disorder patients. Further research is needed to address the role technology-based interventions have on overcoming health inequalities to meet the needs of disadvantaged groups.

**Implications:** This review provides the first quantitative evidence of the effectiveness of a range of technology-based smoking cessation interventions among disadvantaged smokers,

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with separate estimates on the basis of intervention type, and cessation outcome measure. Providing cost-effective, easily accessible and real-time smoking cessation treatment is needed, and innovative technology-based platforms will help reach this endpoint. These interventions need to be tested in larger scale randomised controlled trial designs and target broader disadvantaged groups. Data collection beyond 6-months is also needed in order to establish the efficacy of these intervention approaches on long-term cessation rates among disadvantaged population groups.

## INTRODUCTION

Compared to smoking rates in the adult general population,[1, 2] prevalence is higher among socioeconomically disadvantaged groups[3] as characterised by low educational attainment, low incomes, unemployment, and blue collar workers. The long-term unemployed, homeless, mentally ill, ethnic minorities, prisoners, at-risk-youth and single parents are some of the groups that collectively can be defined as ‘disadvantaged’.[4] Among some disadvantaged groups smoking rates are as high as 70% for homeless persons,[5] 50% for Indigenous persons,[6] 50%-83% for prisoners,[7] 40-60% for socioeconomically disadvantaged persons,[8, 9] 30% among African American adults,[10] and 20-30% for those suffering mental illness.[11, 12] Reducing smoking among disadvantaged smokers is central to reducing the burden of disease for these groups, but effective smoking cessation interventions for these populations are currently scant.[13]

Effective approaches to smoking cessation for the general population involve a combination of behavioural support and pharmacotherapy.[14, 15] However, despite established efficacy in the general population,[11] these treatment approaches have proven less effective among disadvantaged smokers and as such, quit rates among these groups remain disproportionately low.[16] One trial of behavioural and pharmacological interventions found that the least

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advantaged smokers were almost three times less likely to quit (5.3%) relative to the most advantaged (14.3%).[17] A contributing factor to lower cessation rates among disadvantaged groups is poor treatment adherence.[13, 17, 18] One method to increase behavioural treatment and medication adherence is the use of technology-based interventions.

Technology-based interventions include internet sites, computer programs, mobile phone text-messaging ('texts') and smartphone applications ('apps') known as mobile-health technology (m-Health), and other electronic aids.[19] M-Health and internet-based interventions are effective for a number of health conditions[20-24] including smoking in general population samples.[19, 25-27] A comprehensive systematic review investigating the effectiveness of technology-based (computer, internet, mobile phone, or other electronic aids) interventions in adult smokers reported relative risk ratios of 1.32, 95% confidence interval (CI) for prolonged abstinence and 1.14, 95% CI for point prevalence abstinence, indicating technology-based interventions increases the likelihood of cessation compared to generic self-help materials. Despite widespread access to the internet and mobile phone technology among disadvantaged groups,[28-31] the efficacy of technology-based interventions for smoking cessation among disadvantaged smokers is largely unknown.

Disadvantaged groups present unique challenges when implementing methodologically rigorous interventions.[32] They have lower levels of education and literacy,[33] lower levels of health literacy,[34] and limited finances.[13] Tailoring interventions to an appropriate reading age, provision of free or subsidized treatment, and reimbursing for out-of-pocket expenses can overcome some of these challenges.

A systematic review examining the impact of tobacco control interventions on socioeconomic inequalities in smoking found studies targeting disadvantaged groups were limited by weak study designs.[35] Furthermore, a review examining research output in smoking cessation for

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low-SES and disadvantaged population groups found no significant increases in intervention research output between 2000-2004 and 2008-2012.[36] Although this last review identified no increases in the number of smoking cessation trials for disadvantaged groups, it is not known if there has been a shift towards technology-based interventions, or whether methodological quality has improved.

To date, no systematic review has assessed the methodological quality of these technologies, or their effectiveness at reducing smoking rates for disadvantaged smokers. This systematic review aimed to assess the methodological quality and effectiveness of technology-based smoking cessation interventions in disadvantaged groups.

## Method

### Search strategy

A systematic search, following the *Preferred Reporting Items for Systematic Review and Meta-Analyses* (PRISMA) approach,[37] is summarised in Figure 1. The Cochrane, EMBASE, MEDLINE, and PsycINFO databases were searched from January 1980 to May 2016, using terms pertaining to smoking, cessation, computer/mobile phone/email/, and disadvantaged groups [vulnerable populations or socioeconomic status or homeless persons or mental health or prisoners or juvenile delinquency or indigenous/Maori/Inuit/north American Indian (the search strategy is provided in supplementary materials Tables 2 to 5). Search results from each database were combined. Reference lists of included studies were used to identify further studies.

### Inclusion and exclusion criteria

Included studies were required to test a smoking cessation intervention among a low-SES or disadvantaged smoking population that was primarily delivered on a technology-based platform (e.g., mobile phone, internet, etc.), and met the Effective Practice and Organisation

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of Care (EPOC) study design criteria: randomised controlled trial (RCT); controlled clinical trial (CCT); controlled before and after study (CBA); and interrupted time series (ITS).[38]

No restrictions were placed on definitions of smoking, and studies were required to examine tobacco smoking abstinence. Prior research has identified an overall scarcity of intervention research targeting disadvantaged groups.[13, 36, 39] In order to include as many studies while reasonably concluding that the intervention was targeting a disadvantaged group, studies that included a sample of 45% or greater for a specified disadvantaged group (or less if data was provided for their respective disadvantaged group) were included. Low-SES was defined by low income and/or low levels of educational attainment, and disadvantaged groups included homeless persons, Indigenous and native persons, prisoners, at-risk-youth, persons with a mental illness, persons using substances or diagnosed with a substance use disorder, and persons with a disability or chronic illness such as HIV-positive persons.[40]

Studies were excluded if: they assessed smokeless tobacco or electronic cigarettes; the primary intervention was an 'old technology' (telephone counselling or VHS video), or conventional mass media campaigns, because their efficacy has been established;[41, 42] the intervention targeted smoking prevention or uptake; the outcome data was not reported in a useable form; or cessation was not reported as an outcome (e.g. reduction in cigarette consumption or changes in motivation to quit). Cost-effectiveness and secondary analysis papers were excluded if they did not report new information from the primary outcomes paper. If data was reported in a non-useable form, the authors of the study were contacted to request data.

### **Data extraction**

The titles and abstracts of all identified papers were assessed independently for relevance by three reviewers (VCB, JI, and RJC) with a third reviewer (RJC) used to resolve any discrepancies in coding between the two initial coders. All relevant studies were assessed

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independently in full against the inclusion and exclusion criteria by the same three reviewers (VCB, JI and RJC).

Data extraction was conducted by one of the review authors (VCB) and was independently verified by two other review authors (EAS and RJC) to reach consensus. A data extraction form was developed based on guidance literature,[43] and included: details of the study, sample baseline characteristics e.g. nicotine dependence measure, the intervention and control group comparator, equity tailoring (coded based on study materials being at an appropriate reading age or culturally appropriate etc.), and primary outcomes at each follow-up assessment (number of participants abstinent (cases) and non-abstinent (non-cases) in the intervention and comparison groups). Where papers included multiple comparisons, intervention arms or separate trials, these were treated as independent studies for the purposes of both data extraction and analysis.[44]

### **Risk of bias**

Included studies were evaluated for methodological strength using the Cochrane Collaboration risk of bias tool.[44] Studies were examined for seven risk of bias items: randomised sequence generation, allocation concealment, blinding of participants and assessors, methods of addressing incomplete outcome data, potential selective reporting, and any other bias. Risk of bias was assessed by the first author and independently checked by two other authors (RJC and EAS) to reach consensus. For each item the study was judged to be either 'low risk', 'high risk', or 'unclear', and each study given an overall rating of methodological quality by scoring the number of 'low risk' items out of seven. A score between 1 to 3 items was deemed poor; 4 to 5 moderate; and 6 or more deemed high.

### **Measures of treatment effect/outcome measures**

Biochemical verification is recommended for special populations,[45] however, its use among these groups has declined.[36] The precision of biochemical verification determines the accuracy of self-reported smoking abstinence and is the preferred reporting of smoking abstinence in high-impact journals.[46] Since outcome measures are not standardised,[45] we qualitatively described the outcome measures used and analysed cessation by outcome measure (e.g. self-report vs. biochemically validated) at each available time-point. Smoking cessation outcomes included either point prevalence or continuous abstinence, with biochemically validated abstinence preferred over self-report measures. In studies using more than one measure, the most conservative outcome was included in analyses. Although the Russell Standard criteria of at least 6-months follow-up is considered the gold standard for assessing smoking cessation outcomes[47], we collected and analysed absolute quit rates from immediate post-intervention up to 18-months post-intervention in order to capture both short and long term changes to smoking behaviour, and to examine if intervention efficacy attenuated over time. Studies could use any method for accounting for missing data due to participant drop-out however, in studies where multiple methods were used we extracted data as intention-to-treat (ITT).

### **Data analysis**

Data were analysed as odds ratios (OR) with 95% confidence intervals (CI) using the Mantel-Haenszel fixed-effect method in MetaXL, version 5.1.[48] Statistical heterogeneity was assessed using the  $I^2$  statistic and associated p-value. Where significant heterogeneity was detected ( $p < .05$ ), a random effects model was used.[44]

Where the study comprised both disadvantaged and non-disadvantaged smokers, we qualitatively reported on absolute quit rates at 6-months final follow-up between intervention and control arms between groups. We conducted pre-planned analyses of combined



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intervention effects, subgroup analyses of intervention type (computer-delivered, mobile text-messaging, websites, DVDs and video- telephony), and smoking cessation outcome measure (self-reported versus biochemically validated). To meaningfully analyse data across the immediate-post to 18-months follow-up period, cessation outcome data was categorized as: 1-month follow-up (data collected between one to two-months); 3-month follow-up (data collected between 10-weeks to four months); 6-month follow-up (data collected between five to six months); 12-month follow-up (data collected at 12 months); and 18-month follow-up (data collected at 18-months). Due to inconsistencies and incomplete reporting, secondary measures of changes in cigarette consumption, nicotine dependence, and number of quit attempts could not be meta-analysed.

## Results

### Summary of search results

The results of the search and selection process are presented in the PRISMA flowchart (See Figure 1). A total of 6345 articles were identified and 13 studies met the inclusion criteria. In eleven studies [49-59] the study sample wholly comprised disadvantaged participants, and in two studies [60, 61] the sample comprised both non-disadvantaged and disadvantaged participants, and the results of the disadvantaged group could be isolated and used for the analyses. Of the six authors who were contacted for further information, five responded and additional data was added to the analysis.

### Study characteristics

The total 13 studies comprised 8623 participants, of which 4802 were classified as disadvantaged and used in the analyses. There were between 100 and 2142 participants per study. Participants were on average 40.5 years, and 54.5% were female. At baseline, participants smoked on average 16.9 cigarettes per day and had an average Fagerstrom Test

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for Nicotine Dependence (FTND) score of 4.8 (SD=2.2). Follow-up periods ranged from immediate post-intervention (within one week of the intervention ending) to 18 months. Nine studies were conducted in the United States,[49-51, 53-59] two studies were conducted in the UK,[52, 61] and one study was conducted in New Zealand.[60] Characteristics of the included studies are provided (see supplementary materials Table 6).

### **Disadvantaged groups**

Four studies targeted groups classified as low-SES: one study targeted “blue collar” workers (defined as heavy equipment operators),[50] one study targeted rural American smokers (64.5% of the sample had incomes <200% of the federal poverty level),[59] and two studies targeted low-SES smokers.[52, 61] Three studies targeted smokers with a mental illness,[51, 57, 58] two studies targeted African American smokers,[54, 56] two studies targeted HIV positive smokers,[53, 55] one study targeted substance use disorder patients,[49] and one study targeted Indigenous smokers in New Zealand (Maori).[60]

### **Intervention characteristics**

Five studies used websites to deliver cessation support targeted at low-income smokers[50, 52, 61] and HIV positive smokers.[53, 55] Five studies used a computer program to deliver cessation advice and support to substance dependent smokers,[49] smokers with a mental illness,[51, 57, 58] and predominantly African American (81.8%) pregnant smokers.[54] One study examined a 60-minute culturally specific cessation DVD for African American smokers[56] and one study used integrated video-telephony for rural low-SES smokers.[59] The sole study[60] to focus on Indigenous smokers was conducted in New Zealand and examined the effectiveness of mobile phone text-message cessation support for Maori and non-Maori smokers.

Nine studies tailored their intervention and study materials to their respective disadvantaged group with the aim of reducing health inequalities.[50, 51, 53, 54, 56-58, 60, 61] Equity

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tailoring included: providing study materials at an appropriate reading age; providing culturally appropriate written materials, images, or DVD; and providing appropriately qualified staff (e.g. psychologists being available for smokers with a mental disorder). Eight studies provided participant reimbursements for completing study tasks.[49-51, 55-57, 59, 60]

### **Provision of pharmacotherapy**

Of the 13 included studies, eight studies offered NRT.[49-51, 53, 55, 57-59] Five studies offered up to 10-weeks of monotherapy e.g. patch-only,[51, 53, 55, 57, 58] and two offered combination therapy[49, 50] and one study provided bupropion if NRT monotherapy was not effective.[51] All pharmacotherapy treatment was provided by study staff, five studies provided NRT free of charge/study-supplied,[49, 50, 55, 57, 58] two studies did not explicitly state if NRT was provided for free,[51, 53] and one study had staff assist income-eligible participants to apply for pharmacotherapies from the pharmacy assistance program but medication was not provided free of charge.[59]

### **Cessation outcome measures**

Four studies relied on self-reported quit status: two studies reported self-reported 7-day point prevalence quit rates at 1 and 6 month follow-up[50] and 6-week follow-up.[60] One study reported on self-reported 4-week continuous abstinence at two month follow-up[52] while another study reported self-reported quit status at 1-month follow-up (e.g. yes/no).[56] Six studies used carbon monoxide (CO) confirmed 7-day point prevalence quit rates at 1-month and 10-week follow-up;[49] 3 months;[51, 53, 55, 57, 58] 6 months;[51, 53, 57, 58] 12 months;[51, 53, 57, 58] and 18 month follow-up.[51, 58] Two studies used saliva cotinine verified sustained abstinence at 6-month[61] and 12-months[59] and one study used urinary cotinine at 10 week follow-up.[54]

### **Control group characteristics**

The majority of studies used standard care self-help quit support comparator groups with the exception of two studies. One study[60] provided text-messages thanking control participants for their participation, and another study provided a cessation DVD aimed at the general population.[56] Self-help resources were provided on either static websites,[52, 61] via a list of resources,[49, 51, 53, 55, 57, 58] using telephone quit services,[50, 59] or provided by the regular healthcare professional.[54]

### **Risk of bias**

We assessed all included studies for risk of bias (see supplementary materials Figure 4 and 5). Only one study scored six or more 'low risk' items and was deemed to be methodologically rigorous;[61] eight studies scored at least four 'low risk' items and were deemed to be moderately rigorous;[49, 51, 52, 54, 55, 58-60] and four studies scored no more than three 'low risk' items and were deemed to be poor on measures of methodological rigour.[50, 53, 56, 57] The majority of studies were at low risk of bias for sequence generation,[49, 51, 53-61] incomplete outcome data,[49-52, 55, 57-59, 61] selective reporting,[49-52, 54-56, 60, 61] and approximately half of the studies were at low risk of bias for allocation concealment.[52, 57-61] None of the studies clearly reported blinding of participants and personnel, and thus were all rated as having an unclear risk of bias for this domain. Three studies were deemed high risk of bias for incomplete outcome data,[53, 54, 60] three studies were considered high risk for selective reporting,[53, 57, 58] and two studies were considered high risk for other bias including uneven sample sizes and significant differences in baseline characteristics per condition.[53, 57]

### **Effectiveness of technology-based interventions**

Table 1 shows the results of the meta-analysis using all available data for all interventions and intervention types, at each follow-up.

## Overall

Quit rates for disadvantaged samples revealed technology-based interventions increased the odds of cessation at 1-month follow-up (OR 1.70, 95% CI 1.10, 2.63) however, heterogeneity was moderate ( $I^2 = 62\%$ ,  $Q=18.2$ ,  $p<.01$ ); 3-months follow-up (OR 1.30, 95% CI 1.07, 1.59) with low heterogeneity ( $I^2 = 4\%$ ,  $Q=9.4$ ,  $p>.05$ ); 6-months follow-up (OR 1.29, 95% CI 1.03, 1.62) with low heterogeneity ( $I^2 = 0\%$ ,  $Q=4.9$ ,  $p>.05$ ); and 18-months follow-up (OR 1.83, 95% CI 1.11, 3.01) with low heterogeneity ( $I^2 = 25\%$ ,  $Q=1.3$ ,  $p>.05$ ).

## Outcomes by intervention type

One study used mobile phone text-messaging and increased the odds of cessation for their respective disadvantaged group at 1-month follow-up (OR 2.81, 95% CI 1.58, 4.99) with low heterogeneity ( $I^2 = 0\%$ ,  $Q=0$ ,  $p= n/a$ ). Computer programs increased the odds of cessation for disadvantaged groups at 3-months (OR 2.04, 95% CI 1.25, 3.34) with low heterogeneity ( $I^2 = 0\%$ ,  $Q=3.6$ ,  $p>.05$ ), 12-months (OR 1.68, 95% CI 1.06, 2.68) with low heterogeneity ( $I^2 = 0\%$ ,  $Q=0.1$ ,  $p>.05$ ), and 18-months follow-up (OR 1.83, 95% CI 1.11, 3.01) with low heterogeneity ( $I^2 = 25\%$ ,  $Q=1.3$ ,  $p>.05$ ), and websites increased the odds of cessation for disadvantaged groups at 6-months follow-up (OR 1.37, 95% CI 1.01, 1.85) with low heterogeneity ( $I^2 = 0\%$ ,  $Q=.33$ ,  $p>.05$ ). Forest plots depicting results of 6-months cessation and 12-months cessation by intervention type are shown in Figure 2 and Figure 3. Only one study provided data for a DVD intervention at 1-month follow-up and one study provided data for integrated video-telephony intervention at 12-month follow-up. Neither intervention type increased the odds of cessation at their respective follow-up time periods.

## Outcomes by cessation measure

The odds of cessation differed by outcome measure at 1, 3, and 6-months follow-up. At 1-month follow-up, the odds of cessation were significant for self-reported abstinence measures (OR 2.02, 95% CI 1.16, 3.53) but not for biochemically validated abstinence measures,

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however heterogeneity was high ( $I^2=74\%$ ,  $Q=15.4$ ,  $p<.01$ ). At 3 and 6-months follow-up, the odds of cessation were significant for biochemically validated measures (OR 1.98, 95% CI 1.24, 3.17) with low heterogeneity ( $I^2=0\%$ ,  $Q=4.6$ ,  $p>.05$ ) and (OR 1.33, 95% CI 1.03, 1.72) with low heterogeneity ( $I^2=0\%$ ,  $Q=4.7$ ,  $p>.05$ ) respectively but not for self-reported abstinence measures. All studies that conducted 12 and 18-months follow-up used biochemical validation.

### **Non-disadvantaged and disadvantaged groups**

Two studies[60, 61] provided data on direct comparisons between groups for non-disadvantaged and disadvantaged. The absolute difference in quit rates at 6-months final follow-up between intervention and control arms were similar: Maori (38/176 (22%) vs. 33/179 (18%)) compared to non-Maori (178/676 (26%) vs. 169/674 (25%)) and low-SES (90/1088 (8.3%) vs. 64/1054 (6%)) compared to high-SES (147/1233(12%) vs. 156/1238 (12.6%). Although the absolute rates of cessation are higher in non-disadvantaged groups the relative effectiveness of the technology-based interventions appeared to be comparable between groups.

### **Pharmacotherapy use**

Five studies provided percentages for participants who received NRT but no quantitative measure for adherence was reported and therefore intervention effects based on pharmacotherapy use could not be assessed.[49, 50, 55, 57, 58] Only two studies reported on pharmacotherapy use and abstinence rates and neither study found a significant difference between those who used or did not use pharmacotherapy on cessation rates.[55, 57]

### **Discussion**

#### **Summary of main findings**

This is the first review to examine the effectiveness of technology-based interventions on smoking cessation for disadvantaged population groups. Meta-analysis results showed mobile phone text-messaging, internet websites, and computer-delivered cessation support interventions were effective at increasing smoking cessation rates for up to 18 months post-intervention. The disadvantaged groups targeted were limited to low-SES smokers, smokers with a mental illness, African-American smokers, HIV positive smokers, substance dependent smokers, and Indigenous (New Zealand Maori) smokers. Despite higher rates of smoking occurring in prisoner populations, homeless persons, single mothers, and at-risk-youth and among other socially disadvantaged groups, we did not identify studies that targeted these disadvantaged population groups using a technology-based intervention.

Findings of the review suggest that methodological quality of included studies was moderate overall. While 45 studies were identified in the review process, most were excluded due to poor methodological quality since they did not meet minimum EPOC criteria. Only one included study was rated high for methodological quality. All 13 studies were RCTs with 10 studies using an ITT analysis. Eight of the 13 studies adequately reported at least five out of seven categories for risk of bias and were considered to be moderately methodologically

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rigorous. This finding highlights that there was a general scarcity of methodologically rigorous trials targeting disadvantaged smokers using technology-based interventions which corroborates the findings of a 2012 review examining socioeconomic status and smoking [13]. High risk categories included incomplete outcome data,[53, 54, 60] selective reporting,[53, 57, 58] and other biases which indicated the randomization process was not effective.[53, 57]Most studies provided unclear information about the adequacy of allocation concealment, blinding of participants and assessors, and blinding of outcome assessment. The lack of methodologically rigorous interventions suggests the overall effectiveness of these findings be interpreted cautiously.

The findings for technology-based smoking cessation interventions suggest they are effective at increasing quit rates at 1-, 3, 6- and 18-months follow-up for disadvantaged smokers. Moreover, certain technology platforms appear to be more effective than others at increasing cessation for disadvantaged groups. Specifically, the single study using mobile phone text-message quit support was effective at increasing short-term quit rates for Maori participants.[60] Computer-delivered interventions increased both short and long-term quit rates for disadvantaged groups while internet websites increased 6-month cessation for disadvantaged groups. Quit rates did not differ between usual care and integrated video-telephony, and a culturally specific DVD. Despite the proliferation of m-Health technology only one study used mobile phone text-messaging[60] and none used smartphone applications, incorporated online chatroom support, or used social media such as Facebook. Due to the low numbers of studies examining different intervention types and the heterogeneity identified in some estimates, these findings are should be interpreted cautiously.

Cessation outcome measures were not uniform across studies. Just over 50% of studies used biochemically validated measures, however, only one study used the Russell Standard 6-



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month sustained abstinence,[61] considered by many scholars as the most robust measure. It is evident from this review that studies targeting disadvantaged groups are adopting biochemically validated cessation outcome measures over self-report measures but the variation in measures adopted does not allow for accurate interpretation of the data.[47, 62] In addition to this, long-term cessation is classified as 6-month abstinence or longer but only approximately 60% of studies collected abstinence data at 6-months follow-up, with less than 40% of studies collecting data at 12-months follow-up, and 15% of studies collected data at 18-months follow-up. If long-term cessation rates are to be improved in populations where high smoking rates persist, long-term cessation data needs to be collected and reported in a standardised way.

Eight studies included the use of pharmacotherapy, the majority provided pharmacotherapy treatment free of charge to participants, however, this information was not clearly reported and study authors needed to be contacted. Although confirmation was received by most authors, two studies were unclear if medication was provided for free[51, 53] and one study did not provide medication for free.[59] Among studies providing pharmacotherapy treatment, treatment adherence was not adequately reported. Research evidence suggests combining behavioural support with pharmacotherapies can increase 6-12-month sustained abstinence[14] and significantly increases the odds of successful quitting compared to smokers who do not use either method.[15] Despite this, pharmacotherapy was not provided in some studies and although the cost of pharmacotherapy is often cited as a barrier to treatment among disadvantaged groups[63] some studies did not clearly report if the provision of pharmacotherapy was free. Moreover, despite evidence that non-adherence is a significant problem[64] and is a barrier to cessation for disadvantaged groups[17] adherence was not reported in a way that was objectively measurable in any of the studies. Only two studies reported on abstinence rates between those who used and did not use

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pharmacotherapy but without adequately reporting on what constituted use. Such findings should be interpreted with caution particularly since population surveys have identified NRT is often used for two weeks or less[64] when no support is provided.

Despite over 80% of the world's current smokers living in low or middle-income countries,[3] only two studies conducted in low to middle income countries were identified in the search strategy. However, both studies were excluded because they did not meet inclusion criteria, to test a smoking cessation intervention among a low-SES or disadvantaged smoking population[65] or that was primarily delivered on a technology-based platform.[66]

According to a World Health Organization report on m-Health for tobacco control, there are 5 billion mobile phone subscriptions worldwide and 70% reside in low and middle income countries.[67] If global health inequalities caused by smoking and smoking related deaths and disease are to be addressed, low and middle income countries need to be the focus of future smoking cessation interventions. Since accessibility and penetration of mobile phone, smartphone, and internet usage is high in low, middle, and high-income countries, targeting population groups with the highest rates of smoking on a platform to which the majority have access is one approach to providing cost-effective healthcare to overcome health inequalities amongst the most vulnerable and marginalized groups.

### **Limitations of the evidence**

This review is limited by the small number of eligible studies and all 13 were RCTs. Such study designs have excellent internal validity but are more likely to encounter limitations when implemented at a population level.[68] It was not possible to compare interventions on the basis of intensity or duration due to high variability among interventions and reporting of these variables. While not a key aim of this review, two studies additionally provided data for non-disadvantaged samples, allowing only a qualitative examination of quit rates between non-disadvantaged and disadvantaged groups. This small number of studies precluded the use

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of sub-group analyses to compare quit rates between non-disadvantaged and disadvantaged groups quantitatively. Previous reviews on technology-based interventions[19, 25, 69] have been conducted in non-disadvantaged samples but none have compared quit rates by population group. Future studies aiming to make a direct comparison in quit rates between disadvantaged and non-disadvantaged groups may opt to use a broader search strategy that explicitly includes non-disadvantaged samples. Abstinence measures were not uniform across studies with a mix of biochemically validated and self-report, 7-day point prevalence and continuous abstinence rates reported. Approximately half of the studies provided cessation medications which limited the power to assess the combined effect of technology-based support with medication. With the limited number of studies, the disadvantaged groups were heterogeneous and we were unable to assess the effectiveness of technology-based interventions on specific disadvantaged groups. Our findings are limited to the disadvantaged groups assessed and cannot be generalised to other disadvantaged groups (e.g., homeless persons). Although the studies were clinically heterogeneous, we found very low (0%) statistical heterogeneity in the effect estimates for 6- and 12-month cessation by intervention type. This could be due to the effect estimates and the confidence intervals from each individual study almost entirely overlapping and were very consistent between studies. Secondly, there may have been insufficient power to detect true heterogeneity. Chi-square tests for heterogeneity in meta-analyses are typically underpowered and it is likely that heterogeneity exists even when it is not detected in a statistical test.[44, 70] Furthermore, the 13 included studies were conducted in high-income countries, so the transferability of results to disadvantaged groups in low and middle income countries is unknown. Due to small sample sizes, heterogeneity of some pooled estimates was high, and should be interpreted cautiously.

## **Conclusions**

This review identified a scarcity of high-quality technology-based intervention research aimed at increasing quit rates for disadvantaged smokers. The scarcity of research is further magnified when investigating specific sub-groups of disadvantage e.g. prisoners, homeless persons etc. Consequently there is a need for further intervention research output for these neglected populations groups.[36] However mobile phone text-messaging and computer-delivered quit support showed promise at increasing quit rates in certain disadvantaged populations (such as those with low-SES, and Indigenous, psychiatric and inpatient substance use disorder patients). Research is needed on technology-based interventions in overcoming health inequalities, particularly in low-income countries.

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## **Competing interests**

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**Table 1.** Odds of quitting for combined interventions and for each intervention type, at each study follow-up (odds ratios [OR], 95% confidence intervals).

		Overall	
Intervention type	Data points	<i>n</i>	OR (95%CI)
<b>All interventions</b>			
1 month	<b>8</b>	<b>3342</b>	<b>1.70 (1.10 to 2.63)<sup>*,a</sup></b>
3 month	<b>10</b>	<b>3540</b>	<b>1.30 (1.07 to 1.59)*</b>
6 months	<b>8</b>	<b>3497</b>	<b>1.29 (1.03 to 1.62)*</b>
12 months	6	1421	1.30 (0.94 to 1.81)
18 months	<b>2</b>	<b>546</b>	<b>1.83 (1.11 to 3.01)*</b>
<b>Computer Program</b>			
1 month	1	151	8.16 (0.43 to 153.71)
3 month	<b>6</b>	<b>870</b>	<b>2.04 (1.25 to 3.34)*</b>
6 months	3	646	1.28 (0.80 to 2.06)
12 months	<b>3</b>	<b>646</b>	<b>1.68 (1.06 to 2.68)*</b>
18 months	<b>2</b>	<b>546</b>	<b>1.83 (1.11 to 3.01)*</b>
<b>DVD</b>			
1 month	1	140	2.97 (0.72 to 12.16)
3 month	-	-	-
6 months	-	-	-
12 months	-	-	-
<b>Mobile text-messaging</b>			
1 month	<b>1</b>	<b>355</b>	<b>2.81 (1.58 to 4.99)*</b>
3 month	1	355	1.50 (0.91 to 2.47)
6 months	1	355	1.22 (0.72 to 2.05)
12 months	-	-	-
<b>Video-telephony</b>			
1 month	-	-	-
3 month	-	-	-
6 months	-	-	-
12 months	1	566	1.13 (0.28 to 2.07)
<b>Website</b>			
1 month	4	2586	1.48 (0.90 to 2.45)
3 month	2	2210	1.22 (0.71 to 2.11)
6 months	<b>3</b>	<b>2386</b>	<b>1.37 (1.01 to 1.85)*</b>
12 months	1	99	0.95 (0.35 to 2.62)

Note: Bold text indicates significant effects.

\* = Significant effect at  $p < .05$

‘-’ = No data available.

<sup>a</sup> = Significant, high levels of heterogeneity,  $I^2 = 51-75$ ,  $p < .05$