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Contingency management versus psychotherapy for prenatal smoking cessation: A meta-analysis of randomized controlled trials

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Abstract

Objective: Prenatal smoking is the leading preventable cause of poor obstetric outcomes, yet treatment options are limited. Past reviews of prenatal smoking cessation have often grouped all counseling into a single category, which ignores the fact that psychotherapy is distinct from brief counseling. The objective of this study was to compare the effect sizes of two intensive interventions for prenatal smoking cessation: contingency management (i.e., financial incentives for abstinence) and psychotherapy.

Methods: A systematic search for randomized controlled trials testing the efficacy of contingency management or psychotherapy was completed using PubMed, PsycINFO, Web of Science, the Cochrane Library, and EMBASE. Independent raters extracted data and assessed trials for risk of bias. Treatment effects were analyzed for three times points: late pregnancy, early postpartum, and late postpartum.

Results: The search yielded 22 studies, and meta-analytic results indicated that interventions (compared to control) generally increased the odds of abstinence. Moderator analyses indicated that intervention type (contingency management versus psychotherapy) accounted for variability in effect sizes. When comparing treatment type, effects of contingency management interventions were significantly larger than those of psychotherapeutic interventions. While psychotherapy did

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DECLARATION OF INTERESTS

The authors have no conflicts of interest to declare.

(References included in meta-analysis are marked with a *)

not affect smoking abstinence, contingency management interventions had significant treatment effects at all three time points.

Conclusions: Contingency management appears to be a safe and efficacious prenatal smoking cessation treatment. Although psychotherapy alone did not show an effect on prenatal smoking abstinence, future research may seek to combine this approach with contingency management to promote prenatal smoking cessation.

INTRODUCTION

Smoking during pregnancy is the leading preventable cause of poor obstetric outcomes in industrialized nations and is associated with a number of fetal, infant, and childhood adverse health effects, including miscarriage, placental abruption, low birthweight, preterm birth, cleft palate, and sudden infant death syndrome (Einarson & Riordan, 2009). For women with continuing tobacco use, there are multitudinous health risks, including maternal complications from miscarriage or ectopic pregnancy, cancer (e.g., lung cancer, cervical cancer), early menopause, and pulmonary and cardiovascular disease (Mackay & Amos, 2003). Although risks of smoking during pregnancy are well known, rates of prenatal smoking in the United States (U.S.) have remained relatively static for the past decade at around 9–10% of pregnant women (Meernik & Goldstein, 2015; Tong et al., 2013). Rates of prenatal smoking are as high as 17% among certain high-risk groups of smokers – such as 20–24 year-olds, those with low education, and those with Medicaid insurance (Oncken, 2012; Tong et al., 2013) – and may exceed 20% in certain geographic areas (Curtin & Mathews, 2016). Moreover, only 10% of women who successfully quit smoking during pregnancy remain smoke-free one year postpartum (Meernik & Goldstein, 2015). To date, previous meta-analyses of behavioral interventions for smoking cessation have not contrasted the efficacy of two intensive forms of behavioral smoking cessation treatment: psychotherapy and contingency management.

For women who smoke during pregnancy, there are currently limited options for efficacious smoking cessation treatment. Among completed clinical trials, evidence suggests that it is neither conclusively safe nor efficacious to use bupropion or varenicline for prenatal smoking cessation (De Long, Barra, Hardy, & Holloway, 2014; Myung et al., 2012). Currently, neither the World Health Organization (WHO) nor the American College of Obstetricians and Gynecologists (ACOG) recommend any type of pharmacotherapy for pregnant smokers (ACOG, 2010; WHO, 2013). However, health organizations in the United Kingdom, Canada, Australia, and New Zealand differ from ACOG and WHO, since they recommend nicotine replacement therapy (NRT) for pregnant women who smoke and have difficulty quitting (Bar-Zeev et al., 2018). Meta-analytic data suggest that NRT may be efficacious and safe for prenatal smoking cessation through late pregnancy. However, when non-placebo RCTs (i.e., open label trials) were excluded from analysis, NRT was not more efficacious than placebo (Coleman et al., 2015). Many women's health providers are hesitant to recommend pharmacological interventions for prenatal smoking cessation, so the remaining treatment option is behavioral intervention. Given the substantial risks associated with prenatal smoking, research synthesis of behavioral interventions for pregnant smokers is crucial.

One issue in past meta-analyses of prenatal smoking is treating vastly different behavioral interventions as homogenous. In a recent meta-analysis of behavioral interventions, counseling interventions were associated with a small effect on abstinence rates (Chamberlain et al., 2017). However, a wide range of intervention types were coded as counseling. For example, both 5-minute physician's medical counseling as well as 12-session formal psychotherapy were coded as counseling (Chamberlain et al., 2017). This is problematic because brief medical counseling differs greatly from psychotherapy in terms of a number of considerations: participants who self-select into the treatment sample, typical sample size of the study, intervention intensity, and cost of implementation. While a 2011 meta-analytic review of randomized controlled trials (RCTs) found that for prenatal smoking cessation, counseling was not efficacious, this meta-analysis included studies testing minimal clinical intervention and did not specifically select RCTs that tested psychotherapy (Filion et al., 2011).

In contrast, a small but growing body of evidence suggests that contingency management (i.e., providing financial incentives contingent upon biochemically verified smoking abstinence) may be an efficacious, behaviorally based way of promoting prenatal smoking cessation. While the efficacy of contingency management has been shown in a number of contexts (Sigmon & Patrick, 2012), reviews for prenatal smoking have merely noted its promise, calling for more evidence of its efficacy. Higgins and colleagues (2012) emphasized the large effect size of contingency management for prenatal smoking, but also noted that further evidence was needed. Similarly, a recent review by Chamberlain and colleagues (2017) noted that although effect sizes of RCTs comparing incentives to less intensive treatments had the largest effect size estimates for prenatal smoking abstinence, there was a high level of variability in study results. Another meta-analytic review (Cahill, Hartmann-Boyce & Perera, 2015) indicated that although contingency management for smoking cessation has limited long-term efficacy in general, there is evidence of long-term efficacy of contingency management specifically for pregnant smokers.

As noted previously, psychotherapy should not be grouped meta-analytically with brief counseling for prenatal smoking cessation. Thus, it is important to specifically consider intensive interventions. This study examined the efficacy of psychotherapy and contingency management, two of the most difficult interventions to implement. Specifically, there are some concerns regarding fidelity to smoking cessation behavioral support (Lorenatto, West, Christopherson, & Michie, 2013), and contingency management costs have represented a barrier to its implementation (Boyd et al., 2016). Given the challenges of implementing these two types of prenatal smoking cessation interventions, it is relevant to first ascertain their efficacy for this specific population.

The objective of the meta-analysis was to address the question: Among RCTs for women smoking tobacco during pregnancy, what are the effect sizes of contingency management for prenatal smoking cessation compared to traditional psychotherapeutic interventions with no financial incentives?

MATERIALS AND METHODS

Consistent with Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA; Moher, Liberati, Tetzlaff, Altman, & The PRISMA Group, 2009), study team members created the review protocol, including search criteria, trial eligibility guidelines, data abstraction, and outcome variables prior to conducting the literature search.

Data Sources and Searches

We searched PubMed, PsycINFO, Web of Science, the Cochrane Library, and EMBASE for peer-reviewed, English-language RCTs published on or before July 6, 2017. Search terms broadly included keywords related to “pregnancy,” “smoking cessation,” “counseling” or “incentives,” and “randomized controlled trial.” See Appendix 1 for search criteria by database. We also reviewed bibliographies of included trials and applicable systematic reviews for publications not identified by our database search.

Trial Selection: Inclusion and Exclusion Criteria

Two reviewers (S.W. and P.C.) used pre-specified eligibility criteria to assess all titles and abstracts. Titles and abstracts were screened to ensure that each study tested a smoking cessation intervention in human pregnant women, did not mention using a non-RCT study design, and did not include non-incentive or non-psychotherapeutic intervention techniques (e.g., mobile/electronic health, pharmacotherapy).

The full texts of potentially eligible trials were retrieved for further evaluation. The research team received training from S.W. on full-text screening. Two raters independently reviewed each full-text article for eligibility using the following criteria: 1) the sample had to include only women who were pregnant during the intervention (or results for pregnant women could be extracted from presented data if it was a mixed sample of prenatal/postpartum); 2) the RCT had to assess biochemically verified smoking status at a follow-up of at least 3 months post-randomization; 3) the study had to report point prevalence smoking abstinence; and 4) the active treatment had to be compared to standard of care, treatment as usual, best practice treatment, or another less-intensive treatment (e.g., non-contingent incentives). The eligibility criterion for incentive-based RCTs was that each study had to include at least one intervention arm with incentives to be earned contingent on abstinence, including money, vouchers, prizes, or drawings for potential incentives. We included RCTs that compared smoking cessation interventions with abstinence-contingent incentives to comparison conditions with either no incentives or non-contingent incentives. RCTs that tested psychotherapeutic smoking cessation interventions for pregnant women were included if the active treatment condition consisted of an intervention with at least 2 sessions, with at least 40 minutes of total therapy time (i.e., the equivalent of two 20-minute sessions or four 10-minute sessions). This criterion was selected to rule out non-psychotherapy provider encounters that may be labeled “counseling,” such as the gold standard 5A’s model for smoking cessation treatment (Ask about use, Advise to quit, Assess willingness to quit, Assist with quitting, and Arrange follow-up; Fiore et al., 2008). This criterion was also meant to ensure inclusion of brief psychotherapies often used for health behavior change (e.g., motivational interviewing; Miller & Rollnick, 2012), and is also in keeping with

clinical practice guidelines for tobacco use treatment, which offers 40 minutes total therapy time as an example of relatively intensive behavioral treatment for tobacco use (Fiore et al., 2008). RCTs were excluded if they used a non-experimental design or if both groups received financial incentives for abstinence. All ratings were reviewed by S.W., and disagreements in ratings were resolved by S.W. and P.C.

Data Extraction

Data were coded in duplicate by independent raters (S.W., L.N., and A.M.). After initial data extraction, discrepancies in extracted values between raters were resolved by the raters reviewing the relevant article and achieving consensus. Extracted variables included study type (i.e., psychotherapy or contingency management), population (i.e., low socioeconomic status/at-risk or not), sample size, comparison condition type, mean age, mean weeks gestation at baseline, mean cigarettes per day at baseline, and treatment effect size. Study populations were categorized as a low socioeconomic status or at-risk if one of the following criteria were met: majority of sample receiving WIC/Medicaid/public healthcare, majority of sample characterized as low-income, majority of sample with less than high school education, and/or majority of sample identified as a racial/ethnic minority. Comparison condition was categorized as one of the following: treatment as usual/best practice treatment, active alternative intervention (i.e., in-person/telephone sessions), self-help, or non-contingent incentives. If there were multiple comparison groups in the RCT, then the theoretically least active treatment was selected for coding.

Treatment effect sizes were measured by calculating odds ratios based on abstinence at each of three designated time points: late pregnancy, based on the latest time point available prior to delivery; early postpartum, based on the latest time point between 0 to 5 months after delivery; and late postpartum, based on the latest time point 6 or more months after delivery.

Data Analysis

Risk of bias—For each study, we also assessed and reported on the methodological risk of bias in accordance with the Cochrane Handbook for Systematic Reviews of Interventions (Higgins & Green, 2011). These guidelines recommend the explicit reporting of the following individual elements for RCTs: selection, performance, detection, attrition, and reporting. For each study, data were rated in duplicate (S.W., A.M., and T.H.) to assess risk of bias for each aforementioned element as high/low/unclear as set out in established criteria (Higgins & Green, 2011). Conflict of interest (yes/no/unclear) was also independently assessed for each study based upon funding sources and financial relationships reported. Discrepancies in ratings were resolved by reviewing the relevant article and achieving consensus. Overall risk of bias for each study was determined based upon risk ratings for study elements. Studies were deemed to be moderate overall risk of bias if they were assessed as high risk in at least one element, or if they had more than one element rated unclear. High overall risk of bias was indicated by three or more elements rated as high/unclear risk. Given the limited scope of this meta-analysis and a previous comprehensive review assessing risk of bias in the literature (Chamberlain et al., 2017), we did not assess risk of publication bias or selective reporting.

Meta-Analysis.—All data were entered into SPSS Statistics Version 23 for analysis, and meta-analysis macros were used to analyze the data (Wilson, 2010). Prior to conducting analyses, odds ratios were log-transformed and weighted by their inverse variance weights (Lipsey & Wilson, 2001). Mean effect sizes were computed and homogeneity analyses were conducted using the MeanES macro (Wilson, 2010). Homogeneity analyses determine whether there is more variability in effect sizes between studies than would be expected based on sampling error alone (Cooper, 2010). Random effects models were used to compute the mean effect sizes because given the differences in the types of studies included, it was expected that the effect sizes between studies would vary by more than would be expected if differences were due to sampling error alone (Lipsey & Wilson, 2001). Forest plots were created using the Excel spreadsheet created by Neyeloff, Fuchs, & Moreira (2012).

The MetaF macro (Wilson, 2010), which conducts an analysis analogous to an ANOVA, was used to test the moderating effect of treatment type (i.e., contingency management vs. psychotherapy). This macro computes both the between-class variability (Q_B) and the within-class variability (Q_W) based on the grouping variable. Homogeneity between groups is rejected when the between-class effect is significant. When the within-class effect is not significant, the grouping variable adequately explains the variability between studies in effect sizes; if the within-class effect remains significant, the grouping variable only partially explains the variability (Lipsey & Wilson, 2001). Mixed effects models were run because it was expected that there would still be significant heterogeneity between studies after accounting for the effect of treatment type. Since this meta-analysis concerns a specific hypothesis, analyses examining potential moderating effects of other extracted variables (e.g., comparison condition type, mean cigarettes per day at baseline) were not conducted.

RESULTS

Systematic Search

The initial search strategy yielded 1,126 records through database searches and 7 records identified through other sources (e.g., citation list of other reviews) for a total of 1,133 citations (see Figure 1 for PRISMA flow diagram). Abstracts of all 1,133 records were screened, and 999 were excluded for failing to meet initial criteria. The remaining 134 records were retained for full-text assessment. Of those, 112 were excluded for not meeting inclusion criteria (Figure 1). 22 studies were retained for qualitative synthesis and quantitative meta-analysis. See Table 1 for a list of studies as well as descriptive information for each study.

Quality Ratings

Most of the included studies were rated with a low level of overall risk of bias (63.6%; see Appendix 2). No studies included in the review were classified as high risk of bias; eight were rated moderate overall risk of bias. Proportions of included studies with moderate risk ratings did not differ by intervention type. Explanations of moderate risk ratings are as follows. The study by Bradizza et al. (2017) was rated with a high risk of attrition bias because all women postnatal at follow-up were excluded from efficacy analyses (nearly half

the sample at 6-month follow-up), and these outcome data were not reported. The RCT by Donatelle et al. (2000) had high performance bias because of the lack of standardization of intervention (i.e., designated female social support person's relationship was not consistent across participants). This study also had a high detection bias, given that the treatment incentive samples (salivary cotinine) were also used for the outcome assessment (i.e., only the treatment group was incentivized to provide an outcome assessment). The study by Tuten et al. (2012) was rated as high reporting bias since the methodology stated that CO and urine cotinine were collected at 1-month, 3-month, and 6-weeks postpartum, but these data were not reported (difference tests were reported, however). The studies by Rigotti et al. (2006) and Ruger et al. (2008) both were rated high risk in performance bias due to lack of standardization of the number of counseling sessions offered to each participant. High attrition bias ratings were assigned to studies by Secker-Walker et al. (1994), Secker-Walker et al. (1998), and Stotts et al. (2002) due to unequal attrition across groups.

Meta-Analytic Comparison of Intervention Strategies

See mean effect size estimates in Table 2; and forest plots of the effect sizes in Figures 2–4. The study by Tuten and colleagues (2012) had a much larger effect size than any of the other studies at the late pregnancy time point. The analyses for late pregnancy were run both including and excluding this study, and the results did not differ. It is likely that the results did not differ when this study was excluded due to its small sample size, which resulted in this study being given relatively little weight in the weighted analyses. Accordingly, the Tuten study was retained in all analyses.

Overall, meta-analytic results indicated that prenatal smoking interventions significantly increased the odds of pregnant women achieving abstinence from tobacco products during pregnancy relative to comparison conditions. There was not a significant overall effect in abstinence rates in early postpartum or late postpartum, though the effect at late postpartum was a non-significant trend ($.05 < p < .10$). The homogeneity analyses for all three time points were significant, indicating there was more variability in effect size estimates between studies than expected by sampling error alone.

Moderator tests to evaluate differences in the effect size between studies that used incentive-based interventions compared to psychotherapy-based interventions are presented in Table 3. Results indicated that intervention type accounted for the variability in effect sizes between studies at all time points (late pregnancy, early postpartum, late postpartum). In addition, the treatment effect sizes for incentive-based interventions were significantly greater than the effect sizes for psychotherapy-based interventions at all three time points (Table 3). Psychotherapy-based interventions had non-significant ($p > .05$) mean effect sizes at all three time points (range 1.12 – 1.18). Contrastingly, contingency management interventions had statistically significant mean effect sizes at all three time points (range: 2.86 – 3.96).

DISCUSSION

The findings from the present meta-analysis indicate that contingency management is an efficacious treatment for promoting smoking abstinence at each of the three time points examined. In contrast, psychotherapy-based interventions had non-significant effects at each

time point. Notably, these findings are highly consistent with the conclusions of two previous reviews of the efficacy of contingency management (Higgins et al., 2012) and counseling (Filion et al., 2011) for prenatal smoking. For example, Higgins and colleagues (2012) analyzed pooled data from multiple contingency management studies and found that it promoted smoking abstinence among pregnant women both during and after pregnancy. Filion and colleagues' meta-analysis (2011) of randomized controlled trials of counseling interventions for prenatal smoking cessation revealed an overall non-significant effect that remained non-significant when analyses were limited to either face-to-face or telephone-based counseling. Thus, we believe that the results of the present meta-analysis are consistent with much of the extant literature and suggest that, whenever feasible, incentive-based interventions should be used to promote prenatal smoking cessation.

This meta-analysis expands upon previous systematic reviews in one key way. Unlike several previous meta-analyses, this study had more stringent inclusion criteria for smoking cessation psychotherapy trials, since psychotherapy is distinct from provider advice, education, or one-time 5 A's interventions. While other reviews have noted the efficacy of counseling interventions in general (e.g., Chamberlain et al., 2017), the current study indicates that – unlike counseling in general – psychotherapy may have limited efficacy in promoting prenatal smoking cessation. These results call into question whether there may be diminishing returns above and beyond brief smoking cessation counseling. When considering general evidence of smoking cessation using varying modalities, it is somewhat unsurprising that psychotherapy may have less evidence of its efficacy than clinician advice. Many pregnant smokers quit on their own without any intervention (Cooper et al., 2017); and among smokers, those who seek out treatment are less likely to quit than those who self-initiate a quit attempt (Chapman & MacKenzie, 2010; Shiffman, Brockwell, Pillitteri, & Gitchell, 2008). Large RCT's testing the effect of clinician advice on abstinence may be targeting a general population of pregnant women smokers, many of whom would not self-select into a more intensive smoking cessation intervention (like psychotherapy). Findings also underscore variability in effect sizes among psychotherapy trials, which often depends upon treatment methodology, patient characteristics, provider skill and rapport with patients, and many other factors. Variability in efficacy among psychotherapeutic approaches to prenatal smoking cessation appears to be a fruitful topic for further exploration.

While the present findings are encouraging, they should be interpreted within the context of several limitations. First, literature on contingency management for prenatal smoking cessation is still limited ($n = 6$). Therefore, as the results from more trials become available, additional meta-analyses should be undertaken in order to provide more reliable estimates of the treatment effect of contingency management for prenatal smoking cessation. Second, there is a lack of standardization for reporting smoking cessation interventions for pregnant women, with many trials failing to provide details regarding how providers were trained or what type of advice was provided to participants (Bryant, Passey, Hall, & Sanson-Fisher, 2014). While none of the studies included in the present analyses were classified as having a high risk of bias, eight of the studies were rated as having a moderate risk of bias. As noted by Bryant and colleagues (2014), research in this area would benefit from publication of smoking cessation intervention protocols as well as the use of smoking cessation-specific checklists for the reporting clinical trial results. A final limitation of the present analyses is

that we were unable to additionally examine the effects of several novel, non-counseling-based behavioral interventions for prenatal smoking cessation (e.g., text messaging and mobile health applications). While many trials examining these types of novel approaches are currently underway, there were not enough completed trials available at the time of the present study to allow for meta-analytic approaches.

IMPLICATIONS FOR PRACTICE AND POLICY

The present findings provide support for the use of contingency management. It is important to also reiterate that NRT and other forms of smoking cessation pharmacotherapy have not to date been shown to be completely safe for pregnant smokers (De Long et al., 2014; Myung et al., 2012). Contingency management for prenatal smoking abstinence likely represents a safe and efficacious strategy for helping pregnant smokers achieve abstinence during pregnancy, which in turn can improve maternal, birth and early childhood outcomes.

However, despite growing evidence for the efficacy of contingency management for prenatal smoking cessation, there are barriers to implementing this as a public health strategy for pregnant women. Most notably, public opinion does not favor financial incentives as a strategy to promote prenatal smoking cessation.

There are three common concerns regarding contingency management: 1) public funding should not be levied towards incentivizing health behaviors of only a select subgroup; 2) incentives may decrease autonomy; and 3) incentives do not address underlying social determinants of smoking (Lynagh, Sanson-Fisher, & Bonevski, 2013; Stephens, 2014). Additionally, there are concerns regarding the overall cost of implementing contingency management for prenatal smoking, despite preliminary evidence of its cost-effectiveness (Boyd et al., 2016). Indeed, only 41% of the general public agree that it is acceptable to provide vouchers for pregnant women who prove that they are smoke-free (Hoddinott et al., 2014). Furthermore, only half of healthcare professionals agree that providing financial incentives for prenatal smoking cessation is an acceptable practice (Hoddinott et al., 2015).

However, when individuals are provided with information about the benefits and efficacy of prenatal contingency management, it can, in fact, shift their attitudes to be more favorable of this intervention approach (Wen, Higgins, Xie, & Epstein, 2016). This suggests that providers who seek to implement a contingency management program for smoking cessation may want to provide key stakeholders with relevant information regarding the potential benefits of contingency management in terms of pregnancy and childbirth outcomes. Additionally, there may be benefit in utilizing implementation science to increase resources, readiness, and uptake for contingency management (Petty et al., 2017).

Given the medium to large effect sizes of some psychotherapy trials reviewed in this study (e.g., cognitive behavioral therapy [Dornelas et al., 2006] and emotion regulation [Bradizza et al., 2017]), it also may be advantageous to combine contingency management with psychotherapy for prenatal smoking cessation. This approach is being tested in other populations of smokers, such as rural smokers (Wilson et al., 2016). If efficacious, this approach may be scalable as well as more favorable in terms of public opinion.

CONCLUSIONS

Findings from this meta-analysis indicate that contingency management is an efficacious treatment for promoting abstinence from smoking during late pregnancy and into the postpartum period. In contrast, psychotherapy-based interventions may have limitations to efficacy in promoting smoking cessation either during pregnancy or postpartum, although there were a few psychotherapy RCT's with moderate to strong intervention effect sizes. Given that NRT and other forms of smoking cessation pharmacotherapy have also not been definitively demonstrated to be completely safe and efficacious options for promoting prenatal smoking cessation, contingency management appears to be a safe and efficacious prenatal smoking cessation treatment. Future research may seek to investigate the combined effects of psychotherapy and contingency management. Additionally, increased public health efforts should be made to implement and disseminate contingency management to women smoking during pregnancy.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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Figure 1.
PRISMA Flow Diagram

Figure 2.
Forest plot of intervention effect sizes (i.e., natural log of the odds ratio) in late pregnancy.

Figure 3.
Forest plot of intervention effect sizes (i.e., natural log of the odds ratio) at early post-partum.

Forest plot of intervention effect sizes (i.e., natural log of the odds ratio) at late post-partum.

Table 1.

Studies included in meta-analysis.

First Author	Year	Intervention Type	Protocol Therapy Time / Incentive Maximum	Comparison Condition	Sample Size	Study Site	Mean CPD	Mean Age	Weeks Gestation	Low-SES or At-Risk	Late Preg. OR	Early Post-Part. OR	Late Post-Part. OR
Bradizza	2016	P	480 min.	Active	70	Public prenatal clinic	7.5	25	15	Yes	3.00	-	-
Bullock	2009	P	260 min.	TAU/BP	341	WIC clinic	-	24	14	Yes	0.99	0.93	-
Cinciripini	2010	P	600 min.	Active	266	Community	9.8	25	20	Yes	1.27	1.06	0.74
Dornelas	2006	P	180 min.	TAU/BP	105	Prenatal clinic	10.9	26	19	Yes	3.71	-	2.60
Ershoff	1999	P	90 min.	TAU/BP	257	HMO	6.5	29	-	No	0.90	-	-
Lee	2015	P	120 min.	TAU/BP	277	Prenatal clinic	7.6	27	-	Yes	1.34	1.64	-
McBride	2004	P	48 min.	TAU/BP	178	Army medical center	6.0	24	11	No	0.71	-	-
Panjari	1999	P	56 min.	TAU/BP	941	Women's hospital	11.0	26	12	No	1.26	-	-
Rigotti	2006	P	70 min.	TAU/BP	442	Prenatal clinics	10.2	29	13	No	1.37	0.93	-
Ruger	2008	P	180 min.	TAU/BP	210	Prenatal clinic	-	26	-	No	-	-	0.78
Secker-Walker	1994	P	65 min.	TAU/BP	600	Prenatal clinic	13.2	24	14	No	1.14	-	0.84
Secker-Walker	1998	P	75 min.	TAU/BP	399	MIC clinic	12.6	23	15	Yes	1.45	-	1.91
Stotts	2002	P	50 min.	TAU/BP	269	Prenatal clinics	-	28	-	No	1.01	-	-
Stotts	2009	P	72 min.	TAU/BP	240	WIC clinic	11.4	25	22	Yes	1.85	-	-
Tappin	2000	P	152 min.	TAU/BP	100	Maternity hospital	14.0	26	14	No	0.49	-	-
Tappin	2005	P	180 min.	TAU/BP	762	Maternity hospital	11.5	27	13	No	1.05	-	-
Donatelle	2000	I	\$350	TAU/BP	220	WIC clinic	-	27	17	Yes	4.95	4.35	-
Heil	2008	I	\$1180	NC Incent.	82	WIC clinic	8.7	24	9	Yes	6.14	12.54	3.44
Higgins	2014	I	\$1180	NC Incent.	86	WIC clinic	8.7	24	10	Yes	3.74	0.97	2.55
Undersma	2012	I	\$250	TAU/BP	54	Prenatal clinic	8.0	29	-	Yes	0.89	-	-
Tappin	2015	I	\$564	TAU/BP	612	Maternity hospital	-	28	12	No	3.10	-	4.40
Tuten	2012	I	\$1064	NC Incent.	70	Addiction program	17.9	31	17	Yes	26.09	-	-

* Notes. When intended total face-to-face/telephone therapy time was not available, this was estimated based on methodology description; CPD=cigarettes per day; HMO=health maintenance organization; I=Incentives; MIC=Maternal Infant Care; NC Incent.=Non-Contingent Incentives; No.=Number; Preg.= Pregnancy; Post-Part.=Postpartum; P=Psychotherapy; SES=socioeconomic status; TAU/BP=Treatment as Usual/Best Practice; WIC=Women, Infants and Children Program

Table 2.

Mean effect size estimates for the odds ratio of abstinence from smoking between the experimental and control conditions at each time point.

Time Point	Mean Effect Size	95% CI	<i>z</i>	Homogeneity Analysis <i>Q</i> (<i>df</i>)
Late pregnancy	1.55	1.19 to 2.02	3.27 ^{**}	48.43 (20) ^{***}
Early postpartum	1.47	0.90 to 2.39	1.55	13.19 (6) [*]
Late postpartum	1.68	0.95 to 2.98	1.78 [†]	15.59 (7) [*]

Note. CI = Confidence interval.

[†]
 $p < .10$,

^{*}
 $p < .05$,

^{**}
 $p < .01$,

^{***}
 $p < .001$

Table 3.

Test of the moderating effect of intervention type.

Time Point	Intervention Type	Between-Group Effect Q_B	k	Mean Effect Size by Group	Within-Group Effect Q_W
Late pregnancy	Incentive Psychotherapy	31.30 ***	6	3.67 ***	5.35
			15	1.18 †	11.78
Early postpartum	Incentive Psychotherapy	3.99 *	3	2.86 **	4.89 †
			4	1.12	0.99
Late postpartum	Incentive Psychotherapy	10.69 **	3	3.96 ***	0.48
			5	1.17	4.42

† $p < .10$,* $p < .05$,** $p < .01$,*** $p < .001$