The Effectiveness of Interventions To Promote Mammography among Women with Historically Lower Rates of Screening

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Abstract

This study examines mammography-enhancing intervention studies that focus on women in groups with historically lower rates of mammography use than the general population. These groups consist of women who are disproportionately older, poorer, of racial-ethnic minorities, have lower levels of formal education, and live in rural areas. We refer to them as diverse populations. The purpose of this report is to determine which types of mammography-enhancing interventions are most effective for these diverse populations. For this report, United States and international studies with concurrent controls that reported actual receipt of mammograms (usually based on self-report) as an outcome were eligible for inclusion. Intervention effects were measured by differences in intervention and control group screening rates postintervention and were weighted to reflect the certainty of each study's contribution. These effects differed significantly (Q = 218, 34 *df*), and the variation between studies was best explained by indicators of the use of access-enhancing approaches. Combined intervention effects were estimated for different categories of intervention types using random effects models for subgroups of studies. The strongest combination of approaches used access-enhancing and individual-directed strategies and resulted in an estimated 27% increase in mammography use (95% confidence interval, 9.9–43.9, nine studies). Additionally impressive was the access-enhancing and system-directed combination (20% increase and 95% confidence interval, 8.2–30.6, five studies). Access-enhancing strategies are an important complement to individual- and system-directed interventions for women with historically lower rates of screening.

Introduction

A substantial body of research has focused on identifying and overcoming women's, physicians', and system barriers to mammography (1–11). At the same time, there have been striking gains in United States mammography use by age-eligible women (12–14). In 1998, ~68% of United States women aged ≥40 reported a mammogram within the previous 2 years compared with <30% of women in 1987 (15).

Historically, African-American women and women of other racial/ethnic minority groups have obtained mammograms at rates lower than the general population (15). More recently, differences between Caucasian and African-American mammography rates have narrowed (16, 17), but rates for Hispanic women remain lower than those for either African-American or Caucasian women (18). Ethnic differences in screening are less pronounced than they were a decade ago (19), but they do persist in some regions and groups (20). Women in rural areas are less likely to be screened (3); there also are ethnic differences within rural areas, *e.g.*, using data from a statewide mammography registry in New Mexico, Gilliland *et al.* (21) showed that routine use of mammography was low relative to the national level among all women but especially low among Hispanic and Native-American women.

Disparities in mammography use, characterized by socioeconomic factors, such as education and income, remain (15), and they are related to other demographic characteristics, *e.g.*, whereas most women have had a recent mammogram, a substantial proportion of women, especially older women, are not having regular mammograms (16). In 1998, older African-American women were screened less often (61%) than older Caucasian women (64%). Women (53%) aged ≥50 whose household incomes were below the poverty threshold reported a mammogram compared with 72% of women living at or above the poverty level (15).

The purpose of this analysis was to determine which types of mammography-enhancing interventions are most effective for groups of women with historically lower use of mammography. The groups include the following categories under the label "diverse": high school education or less, low income (defined by study authors), ethnic or racial group, ≥60 years of age, and/or live in a rural or inner city area. Studies in any of these categories potentially were eligible for inclusion if they met other inclusion criteria as well.

Recent meta-analyses of mammography interventions have shown that relatively simple interventions, such as reminder letters, telephone calls, and counseling, can increase mammography use (10, 11, 22, 23). However, the meta-analyses conducted to date have not focused on strategies most effective for diverse groups of women. Several authors of the meta-analyses noted this and called for a greater focus on diverse populations (10, 22–24). However, until recently, there were not sufficient numbers of studies to permit analyses of these groups. Our goal was to bridge this gap by examining the

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literature on diverse populations to determine whether some strategies are more effective than others in increasing mammography use.

Conducting meta-analyses in this area is challenging not only because the populations are diverse in their composition but also because these diverse populations face many barriers that have been addressed by a variety of interventions. Several different sociodemographic characteristics (*e.g.,* low income, lower levels of formal education, race, ethnicity, culture, insurance status, and age) characterize populations with lower rates of breast cancer screening. Barriers include knowledge, beliefs, and attitudes (such as not knowing how often mammograms are needed and the ages for which mammograms are recommended), lack of provider recommendation, and health care system factors, *e.g.,* lack of regular source of primary care (1, 7, 13, 20, 25, 26). Interventions have differed along many dimensions, including the types of interventions used; the number, intensity, and duration of strategies; the settings in which they take place; and the research designs and outcome measures. Nonetheless, we developed an analytic framework to identify common elements that permitted synthesis of intervention study results and an assessment of sources of heterogeneity in intervention effectiveness.

Identification of the determinants of mammography use has helped the scientific community and public health practitioners design more effective strategies to reduce barriers to breast cancer screening (1, 8, 10, 22, 23). In the analyses presented here, we measured the effectiveness of interventions to increase mammography use when applied to diverse populations. In many cases, these were interventions, such as tailored and untailored reminder letters and telephone counseling (22, 23, 27, 28) that had been used successfully in other populations. In some cases, special interventions were developed (29–32).

Our analyses had three objectives: (*a*) to describe mammography intervention research efforts in diverse populations; (*b*) to determine which intervention characteristics explain variation in intervention effectiveness; and (*c*) to assess the effectiveness of particular types of interventions and this benefit for specific populations: older women, women with low incomes, and non-Caucasian women.

Materials and Methods

Study Selection. This research builds on an existing database of articles that describes screening mammography interventions (the "parent" database). Development of the parent database, described in detail elsewhere (2), included a search of Medline, Science Citation Index, and PsychINFO for mammography intervention studies published between 1984 and 1997. In addition, we checked references listed in published articles and searched for related articles by authors identified through the initial literature search. Studies were included in the parent database if they reported measuring interventions to increase mammography use in asymptomatic populations. The resulting database included 51 studies that focused on breast cancer screening either exclusively or in addition to cervical cancer screening. Studies not included in the parent database reported only needs assessments, promoted clinical breast examination or breast self-examination exclusively, focused on follow-up for abnormal findings or treatment, or exclusively addressed issues of efficacy or quality control.

A second database was constructed for the subset of articles in the parent database that initially met inclusion criteria. To update the database, searches were conducted of Medline, Social Science Citation Index, and EMBASE. Thus, the database contains all mammography intervention studies conducted in the United States and internationally (published in English from 1984 to August 2000). References listed in reviews and other published articles were also checked. To obtain results of the most current research, we mailed letters to all National Cancer Institute-funded investigators with breast cancer screening intervention grants and requested prepublication manuscripts or published articles that had not yet been entered into journal databases. This identified one additional study that was subsequently published 1 month after our inclusion cutoff date. Had we extended our search of the published literature to the date of that study, we would have identified one more article eligible for the analyses presented here. Of the ~750 articles identified through the literature search, 132 met our initial criteria for inclusion: (*a*) study aims to increase use of mammography among asymptomatic women in diverse populations; (*b*) reports of intervention outcomes based on actual receipt of mammograms, either by self-report or verified report in a clinical database or medical record; and (*c*) studies with experimental or quasi-experimental designs.

Of the 132 articles that met out initial criteria, 94 articles were excluded after more detailed scrutiny for the following reasons: 33 did not have a control group and/or were not designed to address intervention impact on mammography use; in 5, outcomes reported were not sufficiently described, or data were insufficient for use in meta-analysis; 16 were only system-directed or provider-directed interventions or outcomes; 31 did not report the intervention effect for a diverse population; and 8 were preliminary findings, pilot studies, descriptive, process evaluations, or needs assessments. A bibliography of excluded references is available from the authors and as supplementary data at http://cebp.aacrjournals.org.

The resulting meta-analysis database is composed of 38 controlled, experimental, and quasi-experimental interventions that specifically focused on or reported separate mammography outcomes for diverse populations. For reporting intervention effects, we required that the estimated proportion of women getting mammograms be reported separately for the control and intervention groups postintervention or be recoverable using, *e.g.,* ORs[2] and baseline rates. When available and relevant, multiple articles referring to a single study were used to provide information for the database. Interventions that exclusively focused on physicians or office systems as a means to increase mammography use were excluded. Meta-analyses and reviews of these types of strategies, which have found relatively consistent effect sizes, are reported elsewhere (10, 11, 22, 23).

Abstraction Methods. Studies were abstracted using an Access database developed to capture meta-analytic variables. Data were entered for every article by one co-author (C. C.) for consistency and double-entered by another (H. I. M., J. L., B. K. R., or N. B.) to ensure reliability. Every discrepancy was identified and reconciled by two of the co-authors (J. L. and H. I. M.) and then subject to approval by all of the team co-authors.

Study Level Variables. An analytic framework was devised to capture study data entered into the meta-analysis database. A key study level variable is the type of intervention. For every study, intervention group was categorized according to Rimer's intervention typology (8): (*a*) individual directed (*e.g.,* one-on-one counseling, tailored and untailored letters and reminders,

[2] The abbreviations used are: OR, odds ratio; CI, confidence interval; SE, Standard Error; SL, Social Learning; HBM, Health Belief Model.

and telephone counseling); (*b*) system directed (*e.g.,* provider prompts); (*c*) access enhancing (*e.g.,* transportation to appointments, facilitated scheduling, mobile vans, vouchers, and reduced cost mammograms); (*d*) social network (*e.g.,* peer leaders and lay health advisors); (*e*) community education; (*f*) mass media; and (*g*) multistrategy (combinations of interventions listed above).

We also recorded the initial and final years of the active intervention period on the assumption that secular trends may influence effect sizes over time. Baseline data were also entered when available.

Intervention Group Level. Abstracters identified each type of intervention strategy in terms of its intended recipients (*e.g.,* individual women or groups of women), subpopulations of interest (older, low income, ethnic/minority, and urban or rural women), delivery methods (in-person, mail, telephone, TV/radio/print), and settings (community, clinic, work site, church, and housing unit). We also recorded the number of different strategies reported to ascertain the variety and, to a lesser extent, intensity of intervention approaches. These intervention descriptors were summarized across all strategies used to reflect the salient features of an intervention group's experience. Indicator variables identified whether a particular type of strategy, delivery method, intended recipient, and setting were explicitly identified by the study author in the published report.

Control groups were characterized as those whose members received either no intervention, usual care, minimal intervention, or some other nonbreast cancer intervention identified by the authors. Usual care refers to the standard of care provided to clinic-based populations. In some studies, *e.g.,* those based in HMOs, usual care may have been quite comprehensive and often included reminder cards and other strategies. Minimal intervention control groups were exposed to interventions, such as mailed informational pamphlets.

Comparison Level. Each study generated one or more comparisons of an intervention group to a corresponding control/usual care group depending on the number of intervention groups, outcomes, time periods, and subgroups reported. Initially, if the study focused on a diverse population, comparisons were made for each intervention group in a study using the entire study sample, referred to here as "entire group comparisons." Selected comparisons for diverse subgroups of a study sample are referred to as "subgroup comparisons." Sufficient data were available for separate analyses of intervention effects for older women, low income women, and non-Caucasian women. Because "low income" is defined differently across the United States, abstracters deferred to study authors' definitions of low income. When the authors did not identify a low income group, the lowest income level was selected.

Three of the 38 studies contributed only subgroup comparisons. These studies (33–35) did not specifically target diverse populations but reported mammography outcomes on relevant subgroups. At least one entire group comparison was performed for all remaining 35 studies. In some cases, weighted combinations of outcome reports for two or three study-specific subgroups were created for comparability purposes (*e.g.,* the separate health department reports were combined for Burack *et al.*; Refs. 36 and 37). Six studies had two or more intervention groups in addition to the control groups (Table 1). For those analyses that used only a single comparison per study, the intervention group with the greatest number of strategies, the "highest dose" group, was the only comparison that study contributed. Sample sizes for the intervention and control group(s) were based on the outcomes that were reported and used in published reports.

Outcomes. The outcomes were study-specific adherence rates. The definition of adherence was the one provided by each study author. This allowed for the inclusion of a wider range of studies both with respect to follow-up time and study type. Because the field has been evolving, definitions have changed over time. Generally, study outcomes were typically described as obtaining a mammogram within a specified number of months, where the number of months varied from study to study. We adjusted for the number of months in nearly all of the analyses by using the number of months as a covariate in the models. Some studies recorded receipt of a mammogram, according to screening guidelines or receipt of an initial mammogram. Twelve months was used as an approximation for the number of months in these instances. In studies reporting more than one outcome, the primary outcome usually was used. When outcomes were assessed at several points in time, the first outcome was designated as the primary outcome, unless the authors identified some other outcome as primary. All outcomes included in the meta-analysis were based on women's self reports, chart reviews, and/or medical claims. The method of ascertainment was recorded. Only outcomes involving diverse populations were included among comparisons.

Statistical Analyses. First, we provide a descriptive summary of mammography intervention research in diverse populations. Next, intervention effects for individual studies are estimated, and approximate 95% CIs are graphed.

Intervention Effects. Estimates of the intervention effect for each study comparison were computed using the difference in the postintervention adherence rates between the intervention and control groups. Recent reports indicate the utility of this metric for public policy and clinical purposes (38–40). We performed a parallel analysis with log ORs. When a prepost design was used with baseline mammography rates reported by the study group, the intervention effect was computed as the difference in the changes from pre to postintervention for the intervention and control groups. When a preintervention survey was conducted to ascertain study eligibility, only the postintervention rates were used to compute intervention effects. When only the postintervention screening rates were used, the SE for the estimated intervention effects were approximated using the square roots of the following formula:

$$[p_i(1 - p_i)]/n_i + [p_c(1 - p_c)]/n_c$$

where i and c represent the intervention and control groups, respectively. When the difference in the preintervention screening rates was available, the approximate variance was computed as:

$$[p_i(1 - p_i)]/n_i + [p_c(1 - p_c)]/n_c + [p_{oi}(1 - p_{oi})]/n_{oi} + [p_{oc}(1 - p_{oc})]/n_{oc}$$

where p_{oi} and p_{oc} represent the preintervention screening rates for the intervention and control groups.

We conducted a test for homogeneity of the intervention effects and explained heterogeneity using study-level characteristics with mixed model regressions weighted to accommodate within- and between-study variation. In addition, we computed combined estimates of intervention effectiveness for categories of studies defined by intervention type. The combined effect estimates are presented for these groupings of studies along with approximate 95% CIs (Table 2; Fig. 1). Another set was estimated for the three diverse study popula-

Table 1 Selected features of studies included in the meta-analysis

First author, publication year (reference)	Group	Setting	Unit of assignment	Composition of the sample at baseline: % older	% with low income	% non-Caucasian (ethnic group)	Rural	Theory	Single (S) or multiple (M) strategies	Group sample size (Control)	$p_1 - p_0$	95% CI
Andersen, M. '00 (44)	1	Community	Neighborhood	N.S.	21	3	X	DM[b]	M	1630 (1688)	3	(0.1, 5.9)
	2	Community	Neighborhood	N.S.	21	3	X		M	1650 (1688)	1.3	(−1.6, 4.2)
	3	Community	Neighborhood	N.S.	21	3	X		M	1717 (1688)	3.2	(0.3, 6.1)
Bird, J. '98 (85)	1	Community	Neighborhood	N.S.	51	100 (A)		SL,[c] DF[d]	M	197 (125)	22	(8.4, 35.3)
Burack, R. '94 (36)	1	Health care	Individual	32	96	96 (B)		HBM[e]	M	1382 (1343)	13	(8.1, 18.0)
Burack, R. '96 (37)	1	Health care	Individual	26	96	96 (B)		HBM	M	388 (381)	5	(−1.4, 11.4)
	2	Health care	Individual	26	96	96 (B)			S	388 (381)	6	(−0.5, 12.5)
	3	Health care	Individual	24	96	96 (B)			S	370 (381)	−4	(−10.1, 2.1)
Crane, L. '98 (86)	1	Community	Individual	57.6	37	21.5 (B, H)		TT[f]	S	319 (293)	−0.7	(−7.2, 5.8)
	2	Community	Individual	55.8	39	22.5 (B, H)			M	305 (293)	0.5	(−6.2, 7.2)
Dolan, N. '99 (35)[g]	S	Health care	Individual	N.A.	N.A.	100 (B)[g]			M	163 (203)	15	(6.4, 23.6)
	S	Health care	Individual	100[g]	N.A.	N.A.				179 (137)	24	(13.3, 34.7)
Duan, N. '00 (32)	1	Community	Church	39	16	50 (B, H)		HBM	S	416 (397)	3.4	(−2.8, 9.6)
Erwin, D. '99 (69)	1	Community	County	N.S.	77	100 (B, H)	X	HBM, SL	M	152 (140)	9.3	(−6.8, 25.4)
Fletcher, S. '93 (29)	1	Both	County	35	35	19 (B)	X	PR[h]	M	486 (484)	10	(1.3, 18.7)
Flynn, B. '97 (67)	1	Both	Neighborhood	27.2	28	N.S.	X	PR, SL, DF	M	266 (274)	4	(−4.4, 12.4)
Fox, S. '98 (49)	1	Both	Neighborhood	8	X	100 (H)		HBM	M	101 (70)	14	(−5.1, 33.1)
Gardiner, J. '95 (87)	1	Both	County	45.7	N.S.	0	X		M	461 (420)	0.7	(−9.6, 11.0)
Hoare, T. '94 (45)	1	Community	Individual	0	N.S.	100 (A)			S	247 (251)	2	(−7.0, 11.0)
Janz, N. '97 (27)	1	Health care	Individual	100	N.S.	24		HBM	M	223 (237)	17	(11.2, 22.8)
Jenkins, C. '99 (70)	1	Community	County	N.S.	35.3	100 (A)			M	227 (211)	3.5	(−9.8, 16.8)
Kiefe, C. '94 (65)	1	Health care	Individual	100	100	84 (B, H)			M	61 (58)	34	(19.1, 49.0)
King, E. '95 (81)	1	Health care	Individual	100	N.S.	24		HBM, TT, CM[i]	M	185 (192)	14	(5.9, 22.1)
	2	Health care	Individual	100	N.S.	24			M	169 (192)	19	(10.3, 27.7)
King, E. '98 (66)	1	Community	Housing units	100	61	23		PR	M	95 (122)	8	(−2.3, 18.3)
	2	Community	Housing units	100	61	23			M	115 (122)	5	(−4.4, 14.4)
	3	Community	Housing units	100	61	23			M	104 (122)	2	(−7.3, 11.3)
Lantz, P. '95 (79)	1	Health care	Individual	26	100	0			M	277 (244)	20.3	(13.4, 27.2)
Lauver, D. '99 (77)	1	Health care	Individual	N.S.	41	45 (B)		CA[j]	S	46 (55)	5.7	(−12.9, 24.3)
Mandelblatt, J. '93 (80)	1	Health care	Hospital	100	100	100 (B, H)			S	160 (159)	21.6	(8.7, 34.5)
Margolis, K. '98 (47)	1	Health care	Individual	50	70	45 (N Am, B)			M	772 (711)	6.4	(1.5, 11.3)
Mishra, S. I. '98 (78)	1	Both	Individual	N.S.	54	100 (H)		EM[k]	S	51 (37)	−1	(−14.3, 12.3)
Navarro, A. '98 (88)	1	Community	Neighborhood	N.S.	70.6	100 (H)		SL	S	56 (56)	7	(−17.7, 31.7)
O'Connor, A. '98 (46)	1	Community	Individual	0	100	6 (Turkish)			M	236 (234)	6	(−3.2, 15.2)
Paskett, E. '99 (50)	1	Both	Neighborhood	57	X	78 (B)		HBM, PR, SL	M	168 (134)	18	(1.6, 34.5)
Rimer, B. K. '92 (64)	1	Community	Housing units	100	N.S.	0		HBM, SL	M	213 (199)	33	(24.8, 41.2)
Rimer, B. K. '93 (34)[g]	S	Health care	Individual	N.A.	100[g]	N.S.			M	221 (208)	15	(2.6, 27.4)
Rimer, B. K. '99 (56)	1	Health care	Individual	N.S.	77	80 (B)		TT	M	128 (128)	−4	(−13.2, 5.2)
	2	Health care	Individual	N.S.	77	80 (B)		TT	M	128 (128)	−1	(−9.8, 7.8)
Skaer, T. '96 (43)	1	Health care	Individual	N.S.	100	97 (H)	X		M	40 (40)	70	(54.1, 85.9)
Skinner, C. S. '00 (31)	1	Community	Neighborhood	46	N.S.	100 (B)		HBM, SL, TT	M	83 (69)	24	(1.4, 46.6)
Slater, J. '98 (30)	1	Community	Housing units	N.S.	85.1	15.6		SL	M	151 (163)	6.3	(−8.4, 21.0)
Stoner, T. '98 (71)	1	Both	Individual	N.S.	70	0	X		M	98 (90)	18	(−1.2, 37.2)
Suarez, L. '97 (51)	1	Community	Neighborhood	28.4	70.4	100 (H)		SL	M	450 (473)	−2.5	(−11.0, 6.0)
Sung, J. F. C. '97 (89)	1	Community	Individual	18	45	100 (B)			S	93 (102)	9.8	(−7.9, 27.5)
Taylor, V. '99 (48)	1	Health care	Health center	34	31	60 (B)		PR	M	262 (82)	27	(16.0, 38.0)
Trock, B. '93 (33)[g]	S	Health care	Individual	N.A.	N.A.	100 (B)[g]		HBM	M	86 (108)	3.2	(−10.9, 17.3)
	S	Health care	Individual	N.A.	100[g]	N.A.		HBM	M	371 (345)	32	(25.1, 38.9)
Weber, B. '97 (68)	1	Health care	Health center	N.S.	60	60 (B)			M	142 (157)	13	(1.6, 24.5)

[a] S, subgroup; A, Asian; B, black; H, Hispanic; NAm, Native-American; N.S., not stated in article; N.A., not applicable, subgroup only; X, article indicates the given characteristics and the study is included in effect size estimation, but percentages not stated.
[b] Decision Making.
[c] Social Learning Theory.
[d] Diffusion.
[e] Health Belief Model.
[f] Transtheoretical Model.
[g] Contribute subgroup comparisons only.
[h] PRECEDE.
[i] Conflict Model.
[j] Theory of Care.
[k] Empowerment Model.

Table 2 Estimated intervention effects

Estimated combined effect sizes with confidence limits for each intervention type. Random effects models. Studies may be classified as implementing more than one type of intervention. Entire group comparisons for primary outcomes.

Intervention type	No. of studies[a]	Combined estimate difference		Combined estimate OR (95% CI)
		Unadjusted (95% CI)[b]	Adjusted for months[c]	
Access enhancing[d]	14	18.9 (10.4, 27.4)	16.5	2.3 (1.7, 3.1)
Individual directed associated with a health care setting	15	17.6 (11.6, 24.0)	14.1	2.5 (1.9, 3.4)
Individual directed associated with a community setting	13	6.8 (1.8, 11.8)	7.0	1.3 (1.0, 1.6)
Community education[b]	14	9.7 (3.9, 15.6)	10.4	1.5 (1.2, 1.9)
Media campaigns[b]	6	5.9 (0.3, 11.5)	11.4	1.3 (1.0, 1.8)
Social network[b]	7	5.8 (−0.2, 11.9)	13.0	1.4 (1.0, 2.0)

[a] Some studies have components in both settings.
[b] CIs only approximate because cluster randomization is not taken into account.
[c] Model includes outcome months.
[d] Control groups do not include strategies of the same intervention type.

tions: (*a*) groups of older women; (*b*) women with low income; and (*c*) non-Caucasian women. (Fig. 1).

Descriptive Analyses. For each study, we report study authors, year in which the intervention was initiated, year in which the data were collected, publication year, setting (health care, community, or both), unit of assignment, diverse groups included, theories used, whether single or multiple strategies were used, sample sizes for each group, and estimated intervention effect along with approximate upper and lower 95% CIs for that effect. Key study level characteristics were aggregated and summarized to present a picture of the literature.

Regression Modeling. Intervention effects and 95% CIs were computed and are reported for each intervention group comparison. For the next portion of the analysis, we used only one comparison per study by restricting comparisons to entire study group comparisons of primary outcomes for the intervention group from each study with the greatest number of strategies. The Dersimnonian and Laird (41) Q-statistic was calculated to test for the homogeneity of the intervention effects. When significant, we used a random effects model that accommodates this heterogeneity. To obtain an estimate of the combined intervention effect in the presence of heterogeneity, we used the standard approach of weighting each study's estimated effect by the inverse of the sum of its own estimated variance and the estimated variability between studies.

When significant heterogeneity was found, study-level characteristics were included in a model, along with random study effects to explain the heterogeneity in the intervention effects. The "metareg" command in Stata (42) was used to fit these models. As above, each study contributed only one estimated intervention effect. Initially, again weighting to take within- and between-study variation into account, models were fit using each of the study-level characteristics appearing in Table 3 one at a time. Study covariates were judged significant if the corresponding Wald tests were significant at the 0.05 level. Next, the significant study covariates were included in a model. All two-way interactions of the significant covariates were considered for inclusion, but none were significant in the presence of their main effects. Because of the particular interest in the types of interventions that work well together, indicators of combinations of different types of intervention strategies also were considered for inclusion. When one or more covariates were in the model, the potential for multicollinearity was assessed by examining correlations between the covariates and changes in the coefficient estimates. Residuals were examined for potential outliers and to assess the validity of the model assumptions.

Potentially influential studies and outliers, such as Skaer *et al.* (43) and Andersen *et al.* (44), were removed from the analyses to gauge their impact on the study results. We also performed analyses with and without the international studies (45, 46). Because the conclusions were not altered, we decided not to exclude these studies from subsequent analyses.

To complement the regression results, we provide a descriptive summary of the results of our analyses for particular subgroups of studies. These summaries illustrate how individual studies compare to one another, and they permit the inclusion of some studies' subgroup comparisons that would not have been included in regression modeling because of our restriction to one comparison per study. We grouped studies by the type of intervention used as defined by Rimer's intervention typology (8), outlined earlier in the "Materials and Methods" section. Combined intervention effects and 95% CIs were estimated based on a random effects model for each intervention type. The combined intervention effect estimates for each group of studies were adjusted for the number of months used for the outcome by including the number of months centered at 12 months in the model. Thus, the combined effects are estimates of the excess percentage of screening in the intervention group for mammograms obtained in the past 12 months.

Combined effect estimates also were estimated for groups of studies or subgroups of studies consisting of three different subgroups: (*a*) older women; (*b*) low income women; and (*c*) non-Caucasian women. Combined intervention effects were estimated for low income women based on entire group comparisons from studies with baseline study samples, in which $>$40% of the women had low incomes and for subgroups of larger study samples described as low income. In some instances, intervention effects were included when the exact percentage of the study sample with low incomes was not reported, but it was clear from the exposition that the study sample included a substantial proportion of women with low incomes. The same approach was used for non-Caucasian women. Non-Caucasians included African-Americans, Hispanics, Asians, and Native-Americans, in descending order of frequency. Some subgroup analyses were done for African-American women, but there were too few studies reporting on the other ethnic/minority groups for meaningful analyses.

Results

Descriptive Analyses. Key features of each of the 38 studies included in this meta-analysis appear in Table 1. Over half of all intervention studies were initiated after 1992, and all were completed by 1997. Over half of the articles in our study were

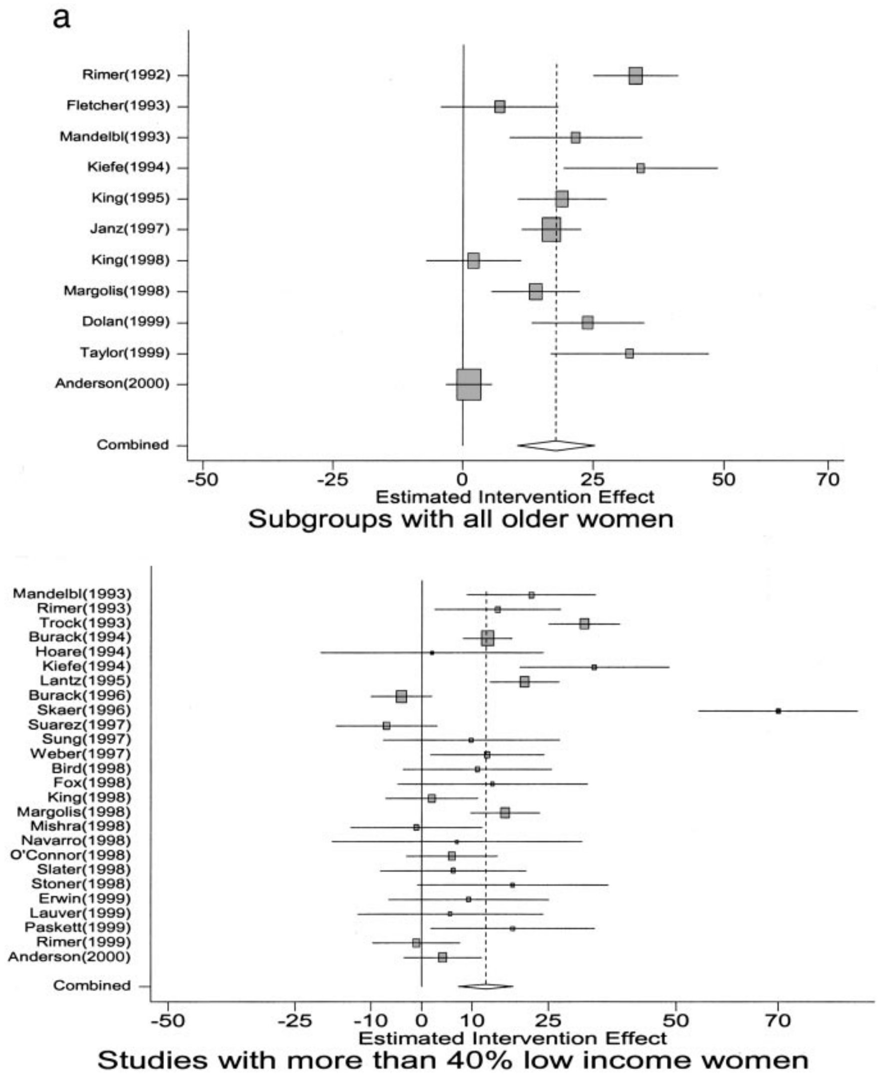

Fig. 1. *a,* individual and combined intervention effects for specific subgroups. Estimated intervention effects for individual studies and combined intervention effect estimates for studies with all older women and >40% with low incomes. *b,* individual and combined intervention effects for specific subgroups. Estimated intervention effects for individual studies and combined intervention effect estimates for studies with >40% non-Caucasian women or >40% African-Americans.

published after 1997. The 23 studies conducted in health care settings were published at a relatively uniform rate, but there has been a recent bolus of reports on community-based studies, with 8 of 22 studies published in 1998 alone. Seven studies had components in both community and health care settings. The only international studies that met the eligibility criteria and were included in this analysis were Hoare *et al.* and O'Connor *et al.* (45, 46).

Most of the studies (>83%) had a single intervention group (29 of 35 studies), 3 studies had two intervention groups, and 3 studies had three intervention groups. Of the 35 studies contributing entire group comparisons, 25 studies (71%) made random treatment assignments. In many cases, the individual was the unit of assignment. However, 19 studies assigned interventions to larger groups, such as neighborhoods or health centers (Table 1).

In addition to study setting, Table 1 provides the sociodemographic composition of studies at baseline. Nine intervention groups (from 6 studies) consisted entirely of older women, and 5 additional studies (44, 47, 48) also reported intervention effects for subgroups of older women. Twenty-six studies contributed comparisons to estimate intervention effects for women with low income levels. Two studies (49, 50) described their study populations as low income, and text corroborated this. Consequently, comparisons from these studies were included in the low income intervention effect estimates. For Margolis *et al.* (47), we used a weighted combination of the effects reported for the Medicaid recipients and the self-insured as a proxy for the low income comparison. Although the Suarez *et al.* (51) sample contained >40% low income women, we opted to use the estimate reported for a subset of this study consisting entirely of women with low incomes. Four other studies (33, 34, 44, 45) contributed estimates for low income subgroups of their study populations. Women of color made up >40% of the comparisons for 24 or the studies, two-thirds of whom were African-American women. Seven intervention studies took place in rural areas. A majority of studies used multiple strategies in their attempts to increase mammography prevalence. The HBM was the predominant theory driving intervention strategies (13 studies), followed by SL Theory (9 studies). About 14 of the studies made no mention of theory. Of those that indicated a theory, 7 mentioned more than one theory.

Study sample sizes and estimated intervention effects for

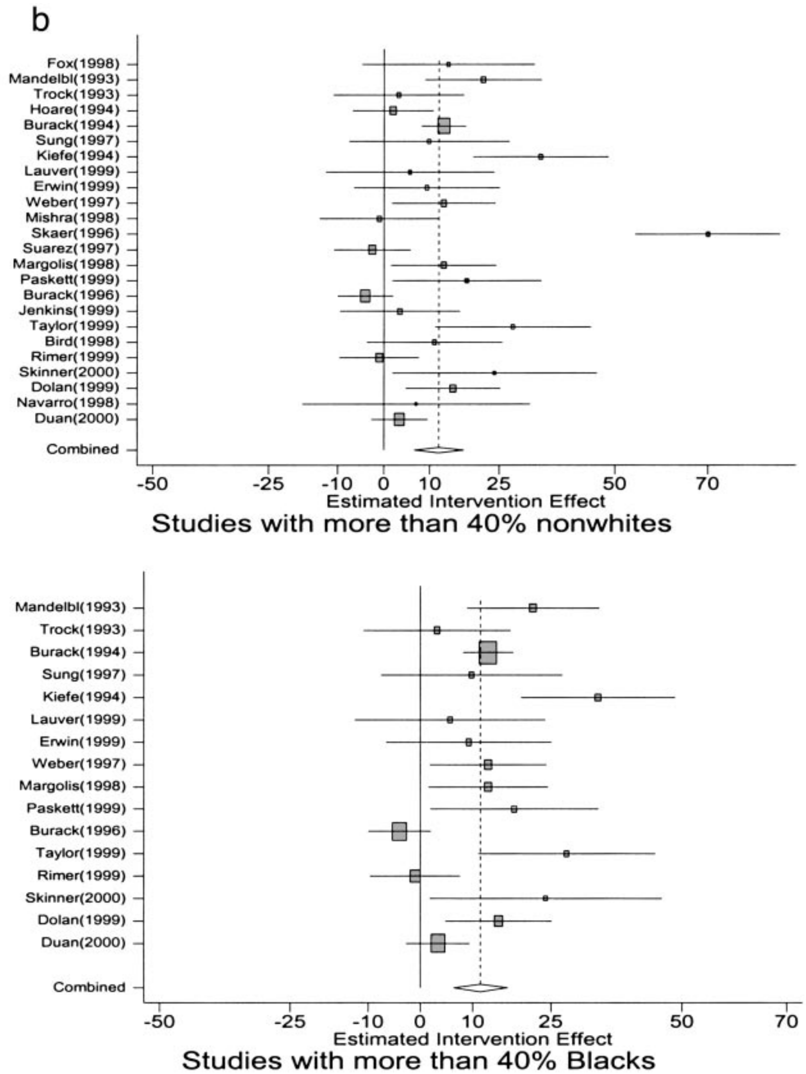

Fig. 1. *Continued.*

all intervention groups are shown in Table 1. Sample sizes varied considerably; smaller studies assigned ~40–50 women/group, whereas the largest study reported on ~1700 women/group. Sixteen studies reported more than one mammography outcome. The most common outcome was mammogram within 12 months (11 of 35 studies). Thirteen of the 35 studies used mammography outcomes for a time period of ≤7 months.

Reflecting secular trends in mammography screening, the control screening rates increased significantly over time (P = 0.02), particularly among those trials with community components. A concomitant decline in intervention effects was observed [P = 0.04; Skaer *et al.* (43) was excluded]. The average duration of the community studies (11.1 months) was greater than that of the 13 studies without a community component (average 5.7 months, P = 0.02). For studies with larger proportions of older women, the control rates were lower (P = 0.02), and the estimated intervention effects tended to be greater (P = 0.01). This same pattern held for low income women, although the results were not statistically significant. Similar effects were not found for non-Caucasians.

Regression Analyses. The estimated intervention effect for each study and 95% CI appear in Table 1. Significant heterogeneity was found among intervention effects associated with comparisons based on the single most intense intervention from each entire study (Q = 218, $P < 0.001$, compared with a χ^2 with 34 *df*). Models with random effects and study-level covariates found that the following variables were positively associated with intervention effect at a 0.05 level when examined one at a time: (a) access-enhancing intervention type; (*b*) an active control group (usual care or minimal intervention); (*c*) the number of months for the outcome; (*d*) whether the baseline sample consisted of >40% of women with low incomes; and (*e*) whether the sample consisted entirely of older women.

Community settings were associated with smaller intervention effects compared with studies with no community components. Two important study-level covariates, year of data collection and the number of months within which a mammogram was reported, could not both be included in the models. Over time, outcome definitions have evolved. More recent studies have used 3-month windows and mammograms within 15 or 27 months as acceptable (*e.g.*, Ref. 52). When both the number of months and the year of data collection were included in a model, year was not significant. Because the months variable was highly significant in nearly all models, and the

Table 3 Covariates for regressions

A. Study level characteristics
Design
Random intervention group assignment (Yes or No)
Unit of assignment (individual women, Yes or No)
Theory based (Yes or No)
Prepost (Yes or No)
Active control group (Yes or No)
Time frame
Start year
Data collection year
Year of publication
Number of months for the outcome
B. Population groups
% non-Caucasian
% older women
% low income
>40% non-Caucasian
>40% women with low income
C. Intervention characteristics
Intervention type
Access enhancing
Individual directed
System directed
Community education
Media campaign
Social network
Setting
Health care
Community
Method of delivery
Face to face
Phone
Face to face or phone
Other
Multiple intervention types
Multiple strategies reported by author

year was rarely significant, we adjusted for months and omitted year. The results did not differ when both variables were in the model. When all of the variables were included in a model and eliminated one at a time until only those significantly associated with the intervention effect remained, the final model retained only access-enhancing intervention type (9%, $P = 0.019$), adjusted for the number of outcome months. Community setting was nearly significant (−7.1%, $P = 0.058$) in a model with access-enhancement and number of months.

When pairs of intervention type indicators were considered for inclusion in a model, only the pair consisting of access-enhancing and individual-directed intervention types was significant once adjustment for the number of outcome months was made, with 95% CI estimates of additional effect (2.1 and 16.7%) for access-enhancement and (0.8 and 14.6%) individual-directed interventions. There was no statistically significant interaction between these two intervention types.

It may be argued that some of the covariates should be retained in the regressions on substantive grounds. Specifically, *a priori* one may believe that random intervention group assignment, adjustment for a preintervention baseline comparison, active control group, and community setting would necessarily influence an intervention effect. In fact, a model forcing these variables resulted in a single significant variable, access-enhancement intervention type, while adjusting for the outcome months.

Sensitivity Analyses. We conducted sensitivity analyses to examine the impact on results of studies with large sample sizes and extreme effects. The final model remains the same with the removal of the Andersen study (44). If Skaer *et al.* (43) is removed, the backward elimination leads to a model with older women and access-enhancement intervention type remaining, with the largest estimated effect ascribed to studies with 100% older women (an estimated increase of 11% in the mammography screening rate, on average) and access-enhancement intervention type adding ~6%.

Fig. 2 provides a summary of the history and variation of the effectiveness of access-enhancing intervention studies by displaying the estimated intervention effects from earliest to most recent. Access-enhancing interventions had the greatest impact on mammography use (Fig. 2; Table 2), with an estimated intervention effect of 18.9% (95% CI, 10.4–27.4; 14 studies). These studies all used multiple types of interventions; the majority involved some form of person-to-person contact (Table 4).

Fig. 2 also shows that the magnitude of the impact of individual-directed interventions in health care settings was nearly identical to that of access-enhancing strategies, with an estimated effect of 17.6% (95% CI, 11.6–24; 15 studies). Individual-directed efforts in community settings yielded effects of 6.8% (95% CI, 1.8–11.8; 13 studies), whereas effect sizes for community education, social network, and media interventions were: 9.7, 5.8, and 5.9%, based on 13, 7, and 6 studies, respectively. The use of multiple intervention types was effective, with intervention effects averaging 13.3% overall (95% CI, 8.6–18; 26 studies). With the exception of the social network interventions, the estimated intervention effects were significantly >0 for all of the groupings. However, each grouping exhibited significant heterogeneity. The most effective combination of intervention types appears to be access-enhancing interventions combined with individual-directed interventions. These studies had an estimated combined intervention effect of 26.9 (95% CI, 9.9 and 43.9; 9 studies). The next largest effect was for the 5 studies combining access-enhancing and system-directed interventions; their effects were 19.4 (95% CI, 8.2 and 30.6). Caution should be used in interpreting these results because the number of studies available to examine pairs of combinations was quite small.

Additional analyses were conducted to examine the combined intervention effects for specific diverse populations. The estimated intervention effect was greatest for older women, 7.9% (95% CI, 10.5–25.4; 11 studies), followed by an estimated effect of 12.7% (95% CI, 7.3–18.1; 26 studies) for comparisons consisting of >40% low income women (Fig. 1*a*). When comparisons were made for intervention groups with >40% non-Caucasian women, the estimated effects were 12 (95% CI, 6.7–17.4; 24 studies) and 11.6% (95% CI, 6.4–16.7; 16 studies), if comparisons are limited to ≥40% African-American women. Again, significant heterogeneity between the studies was evident for each of these groupings.

Discussion

The strongest categories of mammography-enhancing interventions were access-enhancing interventions (18.9% increase in mammography use) followed by individual-directed interventions in health care settings (17.6%). These effect sizes were similar, and the additional costs, if any, of the former are not known. In addition, individual-directed interventions rarely operate in a vacuum but often are built on enhanced usual care systems found in HMOs and community health centers, *e.g.*, (28, 33, 52–56).

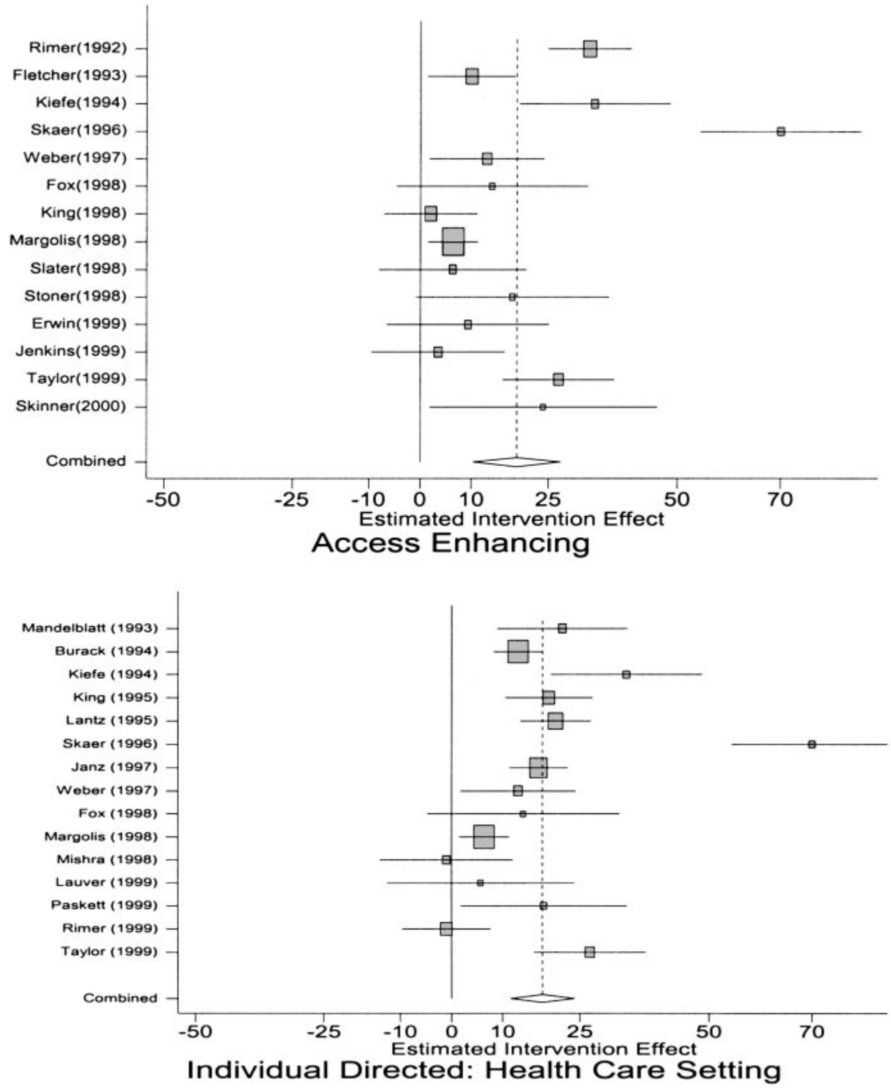

Fig. 2. Individual and combined intervention effects for specific types. Estimated intervention effects for individual studies and combined intervention effect estimates for studies using access-enhancing strategies or individual-directed strategies: differences in postintervention screening proportions. Years correspond to times of intervention, not publication dates.

The most impressive effects were not for single category approaches but for combinations of interventions. Of special note is the effect size of 26.9 (95% CI, 9.9–43.9; 9 studies) for the combination of access-enhancing and individual-directed interventions and 19.6 (95% CI, 8.2–30.6; 5 studies) for the combination of access-enhancing and system-directed interventions. Because the number of studies available to examine pairs of combinations was quite small, it will be important to conduct additional analyses when results from more studies become available. Nevertheless, these effect sizes reflect the interrelated behaviors of women, their health care providers, health care systems, and the larger environment (57–61). Our findings support previous conclusions that individual behavior is shaped also by the social, structural, and economic context that can facilitate or pose barriers to the behavior, *e.g.,* community disadvantage and geographic location (7, 20, 25, 26). This is consistent with an ecological perspective, which suggests that efforts to promote health behavior can be enhanced by interventions that reflect the interplay of individual, system, and environmental factors (59, 62, 63). Contextual, financial, organizational, and geographic factors should be considered, in addition to individual patient and provider factors.

The strongest interventions addressed structural, economic, and geographic barriers to mammography use (what we called access-enhancing interventions), as well as intrapersonal and interpersonal factors (31, 43, 48, 64, 65). Access-enhancing interventions may serve as a bridge between health care settings and the environment in which women reside. In some cases, access enhancement means bringing the service to women (*e.g.,* mobile mammography vans; Refs. 30, 31, 48, 64, and 66–68). It also can include facilitating the use of mammography by overcoming financial and structural barriers through vouchers and same-day appointments (29, 65, 69–71). Likewise, access enhancements may provide cues to action, *e.g.,* a sign-up desk for a mobile van and the subsequent appearance of the van serve as visible reminders about mammography, build social support and opportunities for modeling, and provide opportunities to get screened, consistent with SL Theory (60). Similarly, these reminders are also in keeping with the components of the HBM by providing cues to action and strategies to overcome barriers (60, 72). Thus, access-enhancing interventions may not only alter individual cognitions but actively facilitate the behavior.

The study with the largest effect in the meta-analysis was conducted in a migrant farm community in California (43). This

Table 4 Access-enhancing strategies

First author	Years of study	Access-enhancing strategy	Other intervention types	Method
Burack[a]	1989–1991	Facilitated appointments; reduced costs	Individual System	Mailed reminders; phone
Erwin	1993–1994	Reduced costs; vouchers	Community Education	In person
Fletcher	1987–1989	Reduced costs; coupons	Social Network System Media Community Education	In person
Flynn[a]	1991–1994	Reduced costs; mobile van	Community Education	In person, phone, local media
Fox	1987–1990	Mobile van; low cost	Community Education	In person
Jenkins	1992–1996	Reduced costs; vouchers; translation services	Media Community Education	Radio, print, TV
Kiefe	1992–1992	Reduced costs; vouchers	Individual	In person
King	1993–1994	Transportation; facilitated appointments	System Individual	Mail, in person, phone
Margolis	1992–1995	Facilitated scheduling	Individual	In person
Rimer	1989–1991	Reduced costs; mobile van	System Individual Community Education	In person, print
Skaer	1995–1995	Reduced costs; vouchers	Individual	In person
Skinner	1995–1996	Mobile van	Community Education Social Network	In person
Slater	1991–1993	Free mammograms; facilitated scheduling; transportation	System Individual Community Education Social Network	In person, mail
Stoner	1992–1994	Reduced costs; vouchers	System Social Network	Mail
Taylor	1995–1996	Transportation; facilitated appointment; rescheduling	Individual System	Phone, mail
Weber	1993–1994	Facilitated scheduling; reduced costs; transportation; dependent care; navigation through the medical system	Individual	In person

[a] Access-enhancing control group.

intervention facilitated appointments, provided free mammograms, and informed women about the benefits of mammography, all in Spanish. The researchers also ensured that clinics and transportation were readily available to the intervention group.

In addition to the strategies used in the above study, other access-enhancing interventions included mobile vans, help with appointment/scheduling, dependent care, and navigation through the health care system. A recent study examining the effect of the Medicare benefit for mammography use among older women (73) also provides evidence that cultural and logistical factors associated with access to a usual source of health care are important. To eliminate disparities in breast cancer screening may require a focus on overcoming social, structural, and economic barriers associated with access to health care. Other strategies, including community education and system-directed interventions, and especially individual-directed interventions in health care settings, had impressive effects. As has been shown for other health behaviors (74), we found that multiple strategies were generally more effective than single strategies for increasing mammography use.

Individual-directed interventions in health care settings also demonstrated impressive effects on mammography use and support the logic of capitalizing on opportunities for screening, whereas women are using other health care or social services. Within these settings is one of the strongest motivators for mammography use: physician and other health professional recommendations (5, 26, 75, 76). "Inreach" strategies recruited women in community health centers and clinics serving predominantly indigent populations (36, 43, 47, 48, 50, 56, 65, 68, 77–80) and in some cases provided a complement to broader community outreach efforts (49, 50, 78) Bilingual program materials (48–50), individualized in-person or telephone counseling (27, 50, 79, 81), individualized letters and reminders (27, 36, 48, 50, 68, 79, 81), vouchers, coupons, bus passes, appointment scheduling, or other facilitators (27, 43, 48, 65) and case management or intervention by health educators or lay health advisors (47, 68) are examples of the many individual-directed strategies that have been used in the health care setting. Although women with historically lower rates of screening may have access to health care, these studies suggest that additional cues and enhancements are needed to facilitate access to and receipt of mammography.

Our analyses indicate that some intervention combinations are stronger than others (Table 1). However, only two studies used factorial designs that permitted within-study comparisons of specific intervention components (66, 82). They assessed the relative effect of access-enhancing strategies compared with other strategies, such as community education. More research like this is needed to examine which combinations of strategies have the greatest impact and are most cost effective (10, 11, 22), *e.g.*, Hurley *et al.* (83) determined the effectiveness, costs, and cost effectiveness of three public recruitment and five personal strategies for Australian programs offering free screening. Evidence from these types of analyses is critically needed for making informed decisions on how to allocate resources for public health programs.

People who do not follow recommended health advice are often referred to as the hard to reach. As our results show, in the case of mammography, they may not be so much hard to reach as not reached with the appropriate interventions. We examined intervention effects for diverse populations, including older women, non-Caucasian women, and those with lower incomes. Although the results should be interpreted with caution because of small numbers, they provide reason for optimism. Effect sizes ranged from 17.9 for older women to 12.7 for studies with >40% low income women. It is likely that even greater impact can be achieved by identifying appropriate combinations of intervention components.

Meta-analyses of mammography-enhancing interventions have been reported previously. Although they all use different categories and inclusion/exclusion criteria, making direct comparisons impossible, the conclusions for general populations are consistent with but not identical to our findings for diverse populations.

Wagner's (23) review focused on mammography reminders for general populations and concluded that women who received reminders were more likely to be screened then those who did not (OR = 1.48, $P < 0.001$ for $n = 11$ studies). Tailored reminders were more effective than generic ones, and effects of reminders were strongest in non-United States studies. Other reviews (84) have found that telephone calls to women increased mammography use.

Yabroff and Mandelblatt's (10) review used a different categorization scheme; however, there are some important similarities. Our access-enhancing category (effect size 18.9%; 95% CI, 10.4–27.4; 14 studies) was similar in content to their patient-targeted sociological interventions (effect size 12.6%; 95% CI, 7.4–17.9; 8 studies). Their categories of behavioral (effect size 13.2%; 95% CI, 4.7–21.2; 5 studies) and cognitive interventions (effect size 23.6%; 95% CI, 16.4–30.1; 2 studies) paralleled our individual-directed interventions (effect size 17.6%; 95% CI, 11.6–24; 15 studies). We use different intervention categories and limited our focus to studies involving diverse populations, whereas Yabroff and Mandellblatt's earlier analysis addressed general populations with fewer studies. Nevertheless, our results for diverse populations are similar to their findings for the general literature.

In another meta-analysis, Bonfill *et al.* (24) concluded that the most effective interventions were, in order of effect, mailed educational materials (2.81), letters of invitation plus phone calls (2.53), training and direct reminders to women (2.46), letter of invitations (1.92), and home visits (1.6). Their review included few, if any, of the studies we categorized as access-enhancing. The strongest interventions fall into a category comparable with our individual-directed grouping. Similar to our review, they also concluded that combinations of effective interventions can have an important effect. The authors specifically recommended studies to explore the effect of interventions on diverse subgroups. Thus, previous reviews generally are consistent with our conclusions about the efficacy of individual-directed interventions, although effect sizes vary. Focusing on studies of diverse populations revealed the importance of access-enhancing approaches. Strong secular increases may diminish intervention effects for several possible reasons. An overall increase in mammography prevalence coincided with reports that control group mammography rates were greater than expected (31, 32, 54). Nonetheless, some populations in the studies presented here were not greatly affected by the widespread secular increases in mammography use, as reflected by their consistently low prevalence over time. Another reason why intervention effects may have decreased over time is a shift in focus from initial to repeat screening. Interventions promoting initial mammograms may yield a significantly larger improvement than those aimed at maintenance, but not all studies consistently distinguished initial from repeat screening outcomes. To the extent possible, we tried to account for time trends by considering the year of outcome data collection in each model.

Some studies assigned interventions to groups, such as neighborhoods, hospitals, or counties. However, we based our error estimates on individual women, because most of the studies did not report cluster-specific results [exceptions were Andersen *et al.* (44) and Rimer *et al.* (64)]. Thus, the CIs we reported are likely to be narrower than intervals using the cluster-specific data. Because we recorded the number of clusters randomized for each study, we were able to conduct a crude sensitivity analysis treating the number of clusters as the sample size. Using this very conservative approach, the access-enhancing studies remained highly significant, followed by individual-directed interventions in the health care setting. The remaining types were not significantly different from 0. Understandably, journal space constraints prohibit reporting the cluster-specific information that would be needed to compute more accurate CIs. An alternative may be to use the Internet as a repository for more detailed information, such as cluster-specific results. As research synthesis becomes an increasingly important part of building the knowledge base in particular scientific areas, innovative approaches that make essential information (such as the postintervention screening proportions for each group) readily available to conduct meta-analyses will be feasible and invaluable.

The results of this meta-analysis suggest that the past decade's investment in communication and behavioral interventions has led to a number of effective interventions for diverse populations. These data do not compare intervention effects for diverse populations with those for mainstream populations. Rather, we provide evidence of substantial effects for categories of interventions directed at subsets of populations that have been historically underserved. These findings merit further consideration even in the context of the higher mammography prevalence rates of today. Whereas racial/ethnic differences in mammography use are no longer evident, significant differentials by education, income, health insurance coverage, and having a usual source of health care still persist (90). Therefore, we suggest that access-enhancing strategies are an important complement to individual- and system-directed interventions for women who may lack the resources to readily learn about or obtain screening services.

Future research should measure the effects of the different strategies used, and their intensity, to evaluate the effectiveness of each element that comprises an intervention package. Practitioners and policy makers should be encouraged to select and promote efficacious interventions. The CDC's "Guide to Community Preventive Services" will facilitate this process by making information about the efficacy of interventions widely available.[3] Researchers should build on the cumulative knowledge base to design the next generation of behavioral interventions.

Acknowledgments

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[3] Internet address: http://www.thecommunityguide.org.

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