Title
The Effect of a Physician Partner Program on Physician Efficiency and Patient Satisfaction

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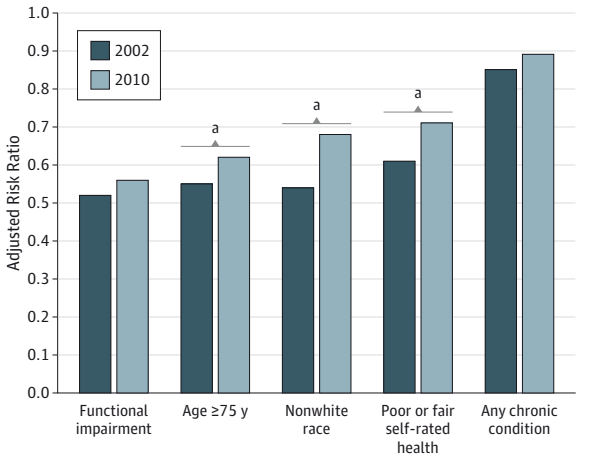
Figure. Adjusted Risk Ratios for Internet Use in 2002 and 2010 in Low-Use Groups



Relative risk of 1.0 indicates no difference in change from 2002 to 2010 compared with reference group (from left to right, vs no functional impairment [P = .08], age younger than 75 years [P = .04], white race [P = .01], good or better self-rated health [P = .02], no chronic condition [P = .86]). Risk ratios are adjusted for demographic characteristics (sex, race, marital status) and socioeconomic status (education and net worth). All analyses are weighted for differential probability of selection and the complex sampling design of the Health and Retirement Study.
[a]Statistically significant comparison.

Meaningful use of EMRs will soon require patient portal use by Medicare patients, and more seniors are going online now than ever[6]; however, our findings highlight the need for health care providers to address functional barriers to Internet use and for future research to target digital health interventions to the specific needs of the frailest patients in this aging population.

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Study concept and design: Greysen, Chin Garcia, Sudore, Covinsky.
Acquisition, analysis, or interpretation of data: All authors.
Drafting of the manuscript: Greysen, Chin Garcia, Sudore.
Critical revision of the manuscript for important intellectual content: All authors.
Statistical analysis: Greysen, Cenzer, Covinsky.
Obtained funding: Chin Garcia, Covinsky.
Administrative, technical, or material support: Covinsky.
Study supervision: Chin Garcia, Covinsky.

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The Effect of a Physician Partner Program on Physician Efficiency and Patient Satisfaction

Despite the advantages of electronic health records, concerns have been raised about the amount of computer time spent documenting care[1] and its adverse effects on the physician-patient relationship. Using scribes to reduce physician documentation time has resulted in improved satisfaction among urologists[2] and increased productivity among emergency department physicians[3] and cardiologists.[4] Although scribes have been used in primary care,[5] their effects have received little formal evaluation.

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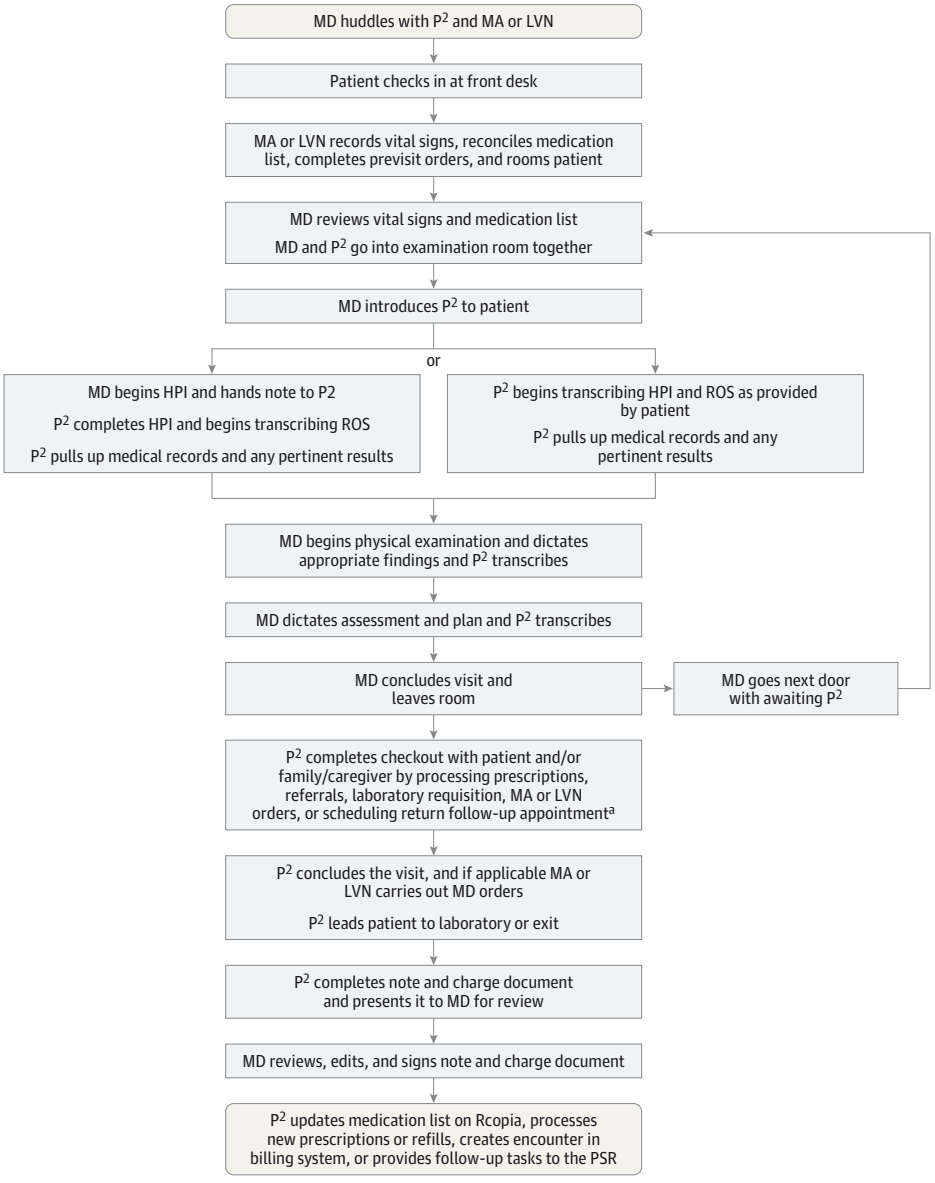
We created a new position, a Physician Partner (P^2), to facilitate patient care during the office visit and tested this in 2 practices at an academic medical center to determine its effect on physician efficiency and patient satisfaction.

Methods | Two P^{2}s, one with a bachelor's degree and the other a licensed vocational nurse, performed scribing and other administrative functions for 3 geriatricians (D.B.R., B.K.K., and 1 other) and 2 general internists (E.G. and 1 other) in a 2:1 ratio (**Figure**). During the study, the practices used an electronic health record (cView; Orion Health) that relied primarily on scanned paper outpatient notes.

Figure. Choreography and Roles of Physician Partners



HPI indicates history of present illness; LVN, licensed vocational nurse; MA, medical assistant; MD, physician; P2, Physician Partner; PSR, patient services representative; ROS, review of systems. Rcopia is an electronic prescribing system (DrFirst).
[a]For general internal medicine visits, the P2 did not perform checkout functions in the room and referred patients to the front desk to perform these.

Each physician had 4-hour clinic sessions with and without P2s, thereby allowing comparisons. Efficiency was measured in a subsample of sessions by (1) direct measurement of physician time in the examining room and (2) retrospective physician diaries of time spent before and after each session. Patient satisfaction was evaluated using questions from the Consumer Assessment of Healthcare Providers and Systems Clinician & Group Survey.[6]

On the basis of time-study data, we calculated the total physician time in the examining room per session. Wilcoxon rank sum tests were used to compare median physician times before, during, and after each session with and without P2s. We used χ^2 tests to compare patient responses with survey questions. The project was a quality improvement project and was not considered research by the institutional review board.

Results | From October 29, 2012, through June 28, 2013, the 5 physicians in the 2 practices had 326 sessions that included P2s. Of these, 37 sessions (22 with and 15 without P2s) that included 289 visits had visit times monitored, and 42 sessions (21 with and 21 without P2s) had physician diaries recording the amount of time they spent before and after the session. Surveys were administered to 156 patients (84 visits with and 72 visits without P2s).

In geriatrics, visits with P2s were a median 2.7 minutes shorter than visits without P2s, 18.0 (interquartile range, 14-21) vs 20.7 (15-26) minutes (P = .01). Among internists, the difference was less, 10.0 (8-14) vs 12.0 (8-15) minutes (P = .15). Per 4-hour scheduled session (**Table**), an estimated 122 minutes (geriatrics) and 75 minutes (internal medicine) were saved during P2 sessions.

Table. Physician Time Spent per 240-Minute (4-Hour) Scheduled Session

Characteristic	Median (IQR), min		*P* Value
	Control	Physician Partner	
Geriatrics			
Preparation before session[a]	30 (30-30)	15 (10-15)	.002
Time spent in examining room[b]	248 (180-312)	216 (168-252)	.01
Wrap-up after session[a]	90 (15-120)	15 (10-18)	.01
Total estimated physician time per session	**368 (225-462)**	**246 (188-285)**	
Internal medicine			
Preparation before session[a]	20 (20-30)	5 (5-15)	.004
Time spent in examining room[b]	192 (128-240)	160 (120-224)	.15
Wrap-up after session[a]	28 (0-43)	0	.005
Total estimated physician time per session	**240 (148-313)**	**165 (125-239)**	

Abbreviation: IQR, interquartile range.

[a] Self-reported using physician diaries for geriatrics sessions (13 with Physician Partners [P²s] and 8 without P²s) and internist sessions (8 with P²s and 13 without P²s).

[b] Physician time per visit (based on 87 visits over 10 sessions with P²s and 63 visits over 7 sessions without P²s in geriatrics and 78 visits over 12 sessions with P²s and 61 visits over 8 sessions without P²s in internal medicine). The median was multiplied by the number of patients that the practice schedules per session. *P* value represents Wilcoxon rank sum tests based on individual visits (see the Results section).

Patients were more likely to strongly agree that the physician spent enough time with them during P² visits (88.1% vs 75.0%, $P = .03$). Although 17.7% were uncomfortable with P²s in the room, 79.3% of patients agreed that they helped the visit run smoothly.

Discussion | In this study, adding personnel to perform more administrative components of office practice was associated with less presession and postsession physician time, shorter geriatric visits, and higher patient satisfaction. Despite these positive findings, several issues remain. First, what background and training do P²s need? We have increasingly employed bachelor's degree–level personnel. Training includes medical vocabulary modules, use of the electronic health record, referral and order entry, and optimizing clinic work flow. A related issue concerns scope of practice regulations. It is possible that documentation requirements of different health care systems and reimbursement regulations may impede diffusion. Finally, what are the financial implications of implementing a P² program? Some practices have estimated that by adding 2 more visits per session, scribe programs can pay for themselves. However, because of diverse cost and reimbursement structures, the business case may vary.

Limitations of the study include the single site, small sample size, inability to measure actual time spent communicating with patients, and self-reported or measured times for only a subsample of sessions.

In summary, the P² program provides a potential model to improve physician efficiency and satisfaction in the office setting without compromising patient satisfaction. The program should be tested in larger samples and additional settings.

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Study concept and design: All authors.
Acquisition, analysis, or interpretation of data: All authors.
Drafting of the manuscript: Reuben, Senelick, Glazier.
Critical revision of the manuscript for important intellectual content: Knudsen, Senelick, Koretz.
Statistical analysis: Reuben, Knudsen, Senelick.
Obtained funding: Koretz.
Administrative, technical, or material support: Senelick, Glazier, Koretz.
Study supervision: Reuben, Senelick, Glazier, Koretz.

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Additional Contributions: Niki Alejo, BS, and Krisan May Soriano, BS, LVN, in their roles as Physician Partners, assisted in defining the choreography and tasks and reviewed the manuscript; Matthew Abrishamian, BA, provided data collection and manuscript review; and Lee Jennings, MD, MSPH, contributed statistical assistance. They received no additional compensation for their roles in this study, and all are affiliated with the Department of Medicine, David Geffen School of Medicine at UCLA, except Mr Abrishamian, who was a volunteer.

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COMMENT & RESPONSE

Meditation Intervention Reviews

To the Editor We appreciated the meta-analysis by Goyal and colleagues[1] concerning the benefits of meditation for psychological health. This review reflects a growing scientific interest in health applications for meditation therapies. As authors active in National Institutes of Health– and Department of Defense–sponsored meditation research, we offer several observations that may assist readers in placing these results into a broader context.

We commend the authors' inclusion of only randomized clinical trials (RCTs) with active controls. However, the review by Goyal et al[1] demonstrates the profound effect of relatively subtle decisions about study inclusion criteria for participants with "a clinical condition" or "stressed populations."[1] In contrast, a 2013 meta-analysis of transcendental meditation (TM)[2] identified 10 RCTs on anxiety with active controls (vs 3 in the review by Goyal et al[1]), with results showing significant effects of TM on anxiety that were not found by Goyal et al. This suggests that different meta-analyses of high-quality studies can arrive at different conclusions based on authors' selection of studies (see also the meditation review by Sedlmeier et al,[3] 2012).

Goyal et al[1] restricted their review to psychological outcomes and did not include other important clinical outcomes. Given their inclusion criteria, we find this a major limitation in design. The American Heart Association (AHA) published in 2013 its systematic review of RCTs of meditation for high blood pressure and reported that "TM may be considered in clinical practice to lower BP," and because of negative or insufficient studies, "other meditation techniques are not recommended in clinical practice to lower BP at this time."[4(p1365)] The AHA statement also reports RCT data showing reduced rates of mortality and cardiovascular disease events associated with TM practice.[5]

We would like to reinforce the authors' limitations concerning the mixed design quality of the meditation trials. Future meditation research would benefit from a systematic categorization of meditation techniques, taking into account the distinctiveness of widely used techniques, quality assurance (certification) of treatment providers, and the regularity of practice by participants.

The authors refer to the important question of noninferiority of meditation to standard treatments. We believe that this is a critical direction for future meditation research. Studies investigating the comparative effectiveness of meditation therapies to more established treatments are crucial next steps for establishing meditation as bona fide treatments.

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Conflict of Interest Disclosures: None reported.

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To the Editor A recent publication on the effects of meditation programs against stress[1] reviews rigorously randomized clinical trials (RCTs) with active control groups. We would like to point out a couple of unsolved issues that may arise when discussing the impact of these findings.

The review has only collated evidence from RCTs with active control groups. Randomized clinical trials can only be done, by definition, with patients and individuals who are willing to be randomized. Thereby such trials are excluding the potentially most beneficial therapeutic agent: conscious choice and active engagement. Thus, by default, RCTs can only test and describe what is the minimum effect on people who use a certain intervention, as if it were delivered to them as a passive recipient, like a medication. But meditation is no medication. It requires active involvement and the decision to dedicate regularly a specific amount of time, over a larger period in order to change one's habits and attitudes. This can only be assessed in long-term comparative cohort studies that in other conditions and occasions have shown reliable results comparable to RCTs.[2]

Against active treatments this meta-analysis showed no effect. We find that this part of the analysis did not represent studies adequately. For instance, 2 studies regarding mindfulness-based cognitive therapy for depression relapse prevention[3,4] were not included, likely because they used treatment as usual as a control, which was an exclusion criterion. Why should, in a condition like recurrent depression, where any treatment is very difficult and has in fact not worked, be treatment as usual be an inadequate control and hence studies be excluded?