Efficacy of a clinical medication review on the number of potentially inappropriate prescriptions prescribed for community-dwelling elderly people

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Abstract

Background: The administration of many drugs concurrently to elderly patients is a well-known problem in geriatrics and involves numerous risks. One way to reduce polypharmacy is to provide information to physicians in order to modify their prescribing practices. The main objective of this study was to evaluate the impact of an intervention program that targeted physicians with the aim of reducing the number of potentially inappropriate prescriptions (PIPs) given to elderly patients.

Methods: A randomized controlled trial was carried out among community-dwelling elderly people in Sherbrooke, Que. The participants were 266 patients over 75 years of age (experimental group: n = 136, control group: n = 130). A team comprising 2 physicians, a pharmacist and a nurse reviewed the list of drugs and the diagnoses of a subgroup of the experimental group in a case conference. Suggestions were formulated and mailed to the patients' physicians together with relevant scientific documentation justifying the recommendations. The main outcome measure was the number of PIPs.

Results: The mean number of PIPs per patient declined by 0.24 in the experimental group (n = 127) and by 0.15 in the control group (n = 116). The decline in PIPs was even larger in the experimental group that had case conferences (n = 80), in which the mean number of PIPs per patient declined by 0.31. However, this difference between the experimental group and the control group was not statistically significant in the intent-to-treat analysis. The number of drugs prescribed was not modified by the intervention, nor were the results of the global assessment of the patients' drug profiles.

Interpretation: This study suggests that the intervention program had no effect on the prescribing of PIPs.

The administration of many drugs together to elderly patients is a well-known problem in geriatrics, not just in Canada but also in other countries.[1,2] It involves numerous risks including an increase in the number of potentially inappropriate prescriptions (PIPs), cognitive disorders, falls, hip fractures, depression and incontinence.[1,3] These factors can cause functional decline and thus reduce the autonomy of elderly people.[4]

According to a study done by the Quebec Health Insurance Board (Régie de l'assurance-maladie du Québec [RAMQ]),[1] approximately 10% of the elderly population have at least one PIP that meets the criteria for therapeutic overlapping, a high daily dose or a harmful drug interaction. The PIP risk increases exponentially with the number of drugs. For example, taking fewer than 4 drugs is associated with a 12% risk, whereas taking more than 5 drugs a day involves a 40% PIP risk. Using RAMQ data, Tamblyn and colleagues[5] analyzed the high-risk prescriptions given to elderly people. Three types of prescription were defined as high risk: drugs pre-

scribed for too long a period of time (e.g., benzodiazepines for more than 30 consecutive days), drugs that are relatively contraindicated for elderly patients (e.g., long-acting benzodiazepines) and dangerous combinations (e.g., 2 drugs in the same category). The authors showed that 52% of the subjects of the study possessed at least one high-risk prescription. Other studies have shown that drug-related problems were the cause of 6.5%–20% of admissions to hospital of elderly people and that the costs of polypharmacy are substantial.[6-9] One way to reduce polypharmacy is to provide information to physicians with the aim of modifying their prescribing practices. A literature review[10] indicates that sending written educational material seems, in isolation, to be consistently ineffective in modifying prescribing practices. However, specific educational and feedback strategies can improve the quality of care. Successful educational strategies involve face-to-face contact between an expert and the physician (academic detailing). Feedback that involves not only a description of current practice, but also includes specific recommendations for changes in the use of medications, can also improve practice.[10-15] However, "academic detailing" is an intervention that is difficult and expensive to implement.[16] The main objective of the present study was to evaluate the impact of an intervention program that targeted physicians with the aim of reducing the number of potentially inappropriate prescriptions given to elderly patients.

Methods

The study was a randomized clinical trial with a sample of 266 subjects who were randomly assigned to an experimental and a control group. The design was a 1-year longitudinal study, and the target population fulfilled the following criteria: they were over 75 years of age, living in the community, at risk of losing their autonomy and taking more than 3 drugs per day.

Subject selection was a 2-stage process. First, 1752 subjects who had been randomly selected from the RAMQ database were sent a postal questionnaire. The Sherbrooke Postal Questionnaire includes 6 items to be answered Yes or No. Subjects who had more than one risk factor or who did not return the questionnaire were labelled positive. In a predictive validity study, Hébert and colleagues[17] showed that this questionnaire detects people who will experience a functional decline over the next year with 75% sensitivity and 52% specificity. This study also showed that the consumption of 3 or more drugs per day increases the risk of functional decline in elderly people by 60%. Of the 778 subjects who were screened positive on the questionnaire, 503 agreed to participate in the study,[18] signed the consent form and were randomly assigned to the experimental ($n = 250$) or the control group ($n = 253$).

Step 2 involved selecting from these 503 subjects the people who took more than 3 drugs per day. These 266 subjects formed our final sample: 130 control group subjects who continued to receive normal social and health care services and 136 experimental subjects who took part in our intervention program.

The independent variable in our study was the intervention program aimed at the experimental subjects' physicians. A nurse met with all the experimental subjects in their homes, took an inventory of their drugs and obtained their permission to contact their physicians for information about their health. A letter was then sent to the physicians to inform them of the study and request their cooperation.

Next, a case conference held by a team comprising 2 physicians, a pharmacist and the same nurse reviewed the list of drugs and the diagnoses of the experimental subjects. The list of diagnoses was recorded by the nurse during a telephone conversation with these patients' physicians, or was mailed back by the physician. The team analyzed the drug profile on the basis of the following criteria: indication, effectiveness, dosage, instructions and their applicability, drug interactions, drug–pathology interactions, therapeutic overlapping, duration of treatment and cost.[19] After identifying possible problems, if any, in the drug profile, suggestions were formulated and mailed to each patient's physician together with relevant scientific documentation justifying the recommendations. These recommendations concerned the following situations: therapeutic overlapping, absence of a relevant diagnosis and thus no indication for a drug, drugs that should be avoided or only used in small dosages for elderly patients, stopping benzodiazepines, using hypolipidemic agents, inappropriate dosages and drug interactions.

Thereafter, the team nurse contacted all the experimental group subjects on a monthly basis. She asked them if their medication had been changed and, if so, what the changes were, but was careful, for ethical reasons, to avoid mentioning whether the patient's physician had received any recommendations. If there had been no changes in the subjects' medications in line with the suggested recommendations, it was intended that the team would review the file again to determine whether one of the team physicians should intervene as an expert to discuss the subject's case with the patient's physician. This last more intensive part of the program, which is similar to academic detailing, was to be limited to some potentially reluctant physicians.

The primary outcome of interest was the number of PIPs. The secondary outcomes were the number of different drugs taken per day, the number of subjects with at least one PIP and the global assessment of any change in the medications between the preintervention and postintervention measurements for each group.

The number of PIPs was measured before the intervention program and after the intervention program, with a 1-year period between the 2 measures. The number of PIPs in each case was calculated from the list of drugs collected by a nurse who did not know to which group subjects had been assigned and who was not involved in the program. Therefore, this process was completely independent of the case conference assessment. There was no difference in the method of assessment between the intervention and control groups. PIPS were identified from a list of PIPs developed by the Quebec Committee on Drug Use in the Elderly,[1] which includes 3 types of PIPs: drug interactions, therapeutic overlapping and drugs of limited use. Although generated by a panel of experts, this list has never been validated with empirical data.

The global assessment of change in the medications between the preintervention and postintervention measures for each group was done blindly and a posteriori by J.A. and M.R. The drug profiles collected before and after the intervention program were identified only by numbers, and the assignment was unknown to the evaluators. They had to assess globally whether the profile had improved, deteriorated or remained stable over the 1-year period, and their judgement was mainly based on the presence or absence of drug interactions, therapeutic overlapping and drugs of limited use.

A sample of 260 subjects (130 per group) was needed in order to detect with 80% power a difference in the mean number of

PIPs per subject of 0.25 between groups, which represents a standardized difference of 0.35.

A process analysis related to the physicians' observance of the team's recommendations was carried out at 8–12 weeks after mailing the suggestions. This process analysis only applied to the experimental group subjects whose physicians had received the multidisciplinary team's comments.

The final outcome analyses were carried out, first, on all the experimental subjects (intent-to-treat analyses) and, second, only on those who had been discussed at a case conference. For the dependent variables, the following tests were used: an independent t-test comparing the experimental group with the control group, a paired t-test on the pre-postintervention difference and an analysis of covariance on the difference between groups after the intervention program, controlling for the preintervention value. For the number of subjects with at least one PIP, the χ^2 test was used to compare the experimental group with the control group both before and after the intervention program and the McNemar test was used on the difference between the pre- and postintervention measures. The odds ratio of not having a PIP after the intervention program was calculated by logistic regression analysis adjusted for the preintervention difference. This study was approved by the Ethical Review Board of the Sherbrooke Geriatric University Institute.

Results

There were no significant differences between the sociodemographic characteristics of the 136 subjects in the experimental group and the 130 control group subjects. There were 89 women in the experimental group and 91 in the control group. The mean age was about 80 years in each group. Table 1 presents the sociodemographic characteristics of the study subjects.

Twenty subjects died during the year of the study (6 in the experimental group, 14 in the control group, $\chi^2 = 3.86$,

$p = 0.049$), and 3 subjects in the experimental group refused to be assessed after the intervention program. Of the remaining 127 subjects in the experimental group, only 80 actually participated in the intervention, that is, their files were discussed by the multidisciplinary team at a case conference. The reasons for not participating were the following: subject refused to participate in the program (10), physician refused to provide the diagnoses (21), or the eligibility criteria were not satisfied when the program nurse did the evaluation (the number of drugs had decreased to fewer than 3 between the initial screening and the nurse's evaluation) (16). Fig. 1 presents a flow chart of the subjects in the study. Before the intervention program, the subjects in the experimental and control groups did not differ significantly regarding the primary and secondary variables (Table 2).

With regard to the physicians, 71 (59 general practitioners and 12 specialists) had patients in the experimental group, and 77 physicians (63 general practitioners and 14 specialists) had patients in the control group. Of the 71 physicians who had patients in the experimental group, 52 (45 general practitioners and 7 specialists) received the multidisciplinary team's comments, and 30 of these 52 also had patients in the control group.

Process analysis

The process analysis only applied to the experimental group subjects whose physicians had received the multidisciplinary team's comments following an analysis of the diagnoses and drug profiles. This analysis was done 8–12 weeks after sending the suggestions to the physician. Of the 80 subjects whose files had been studied at a case conference, 70 had been seen again by their physician. In 32.5% of these cases, the team had no specific suggestions to make to the physicians and had informed them of this by letter. The multidisciplinary team formulated a total of 147 recommendations, 37 of which were accepted by the physicians (25.2%). The recommendations were with regard to the following situations: therapeutic overlapping (10), the absence of a relevant diagnosis and thus no indication for a drug (41), drugs that should be avoided or only used in small dosages for elderly patients (32), stopping benzodiazepines (39), using hypolipidemic agents (13), inappropriate dosages (6), drug interactions (2) and miscellaneous reasons (4).

Our intervention program originally included plans for an "academic detailing" type of meeting with the attending physicians who did not modify their prescriptions in line with the suggested recommendations. In fact, only one physician would probably have warranted a visit from an expert, and this was not done because it would not have changed the results significantly.

Outcome analyses

The mean number of PIPs per patient declined by 0.24 in the experimental group and by 0.15 in the control group

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Table 1: Sociodemographic characteristics of study subjects

Characteristic	Study group, no. (and %)*	
	Experimental group n = 136	Control group n = 130
Sex, female	89 (65.4)	91 (70.0)
Mean age (and SD), yr	80.4 (4.3)	80.7 (4.6)
Language, french	120 (88.2)	112 (86.2)
Married	60 (44.1)	53 (40.8)
Mean no. of years (and SD) of education†	7.3 (3.9)	7.2 (3.6)
Annual income, $		
< 20 000	78 (65.0)‡	82 (70.7)§
20 000 – 29 000	29 (24.2)	26 (22.4)
30 000 – 39 000	9 (7.5)	3 (2.6)
40 000 – 59 000	2 (1.7)	4 (3.5)
≥ 60 000	2 (1.7)	1 (0.9)

Note: SD = standard deviation.
*Unless stated otherwise.
†$N = 261$.
‡$N = 120$.
§$N = 116$.

(p < 0.001) (Table 3). The decline in PIPs was even larger in the experimental group that had case conferences, in which the mean number of PIPs per patient declined by 0.31, which represents a decrease of 36% compared with the control group, which showed a decrease of 19%. However, the difference between the experimental group and the control group was not statistically significant for this primary outcome. The number of subjects who had at least one PIP decreased significantly (p = 0.049) in the experimental group. Taking into account the situation before the intervention program, the logistic regression analysis showed that the risk of not having a PIP after the intervention for the subjects in the experimental group could be expressed as follows: odds ratio (OR) = 1.83 (95% confidence interval [CI] 0.94–3.57) in the intent-to-treat analysis and OR = 2.16 (95% CI 1.01–4.56) including only the subjects discussed at a case conference. The number of drugs prescribed was similar for the control group and the experimental group before the intervention program and was not significantly modified by the intervention.

An analysis was carried out to verify whether there had been any contamination, that is, if there had been a transfer in prescribing practice among the physicians who had subjects in both groups. Thirty physicians had subjects in both the control group and the experimental group with a case conference, and 65 subjects (50%) in the control group had a physician who also had a patient in the experimental group with a case conference. A logistic regression analysis showed that having a physician with patients in both groups was not a significant factor in the risk of not having a PIP after the intervention program.

The global assessment of the change in medication between the preintervention measure and the postintervention measure for each group shows that there was an improvement in the drug profile of 20% of subjects, a deterioration in 5% and that it remained stable in 70%. However, the differences between the experimental and control groups were not statistically significant.

Interpretation

Very few randomized studies have examined the effect of an educational intervention on physicians' prescribing practices concerning elderly patients. A recent literature review by Anderson and Lexchin[14] noted that few studies had evaluated a global approach to improving drug prescribing. They considered 9 clinical trials, most of which examined limited aspects of prescribing, namely, costs, polypharmacy, the use of specific drugs such as antibiotics, nonsteroidal anti-inflammatories, antiulcer agents and the treatment of specific conditions such as hypertension and urinary tract infections. Only one study[13] looked at a broader range of quality-of-care issues very similar to the outcomes that we used in our study. However, our study was different in that it was patient oriented but was also directed at the patients' physicians, the majority of whom were family physicians. Moreover, the intervention related to the subjects' overall drug profile, was inexpensive ($70 per patient) and, therefore, practical.

There was a significant decrease in the number of PIPs in the experimental group but, because there was also a decrease in the control group, this differ-
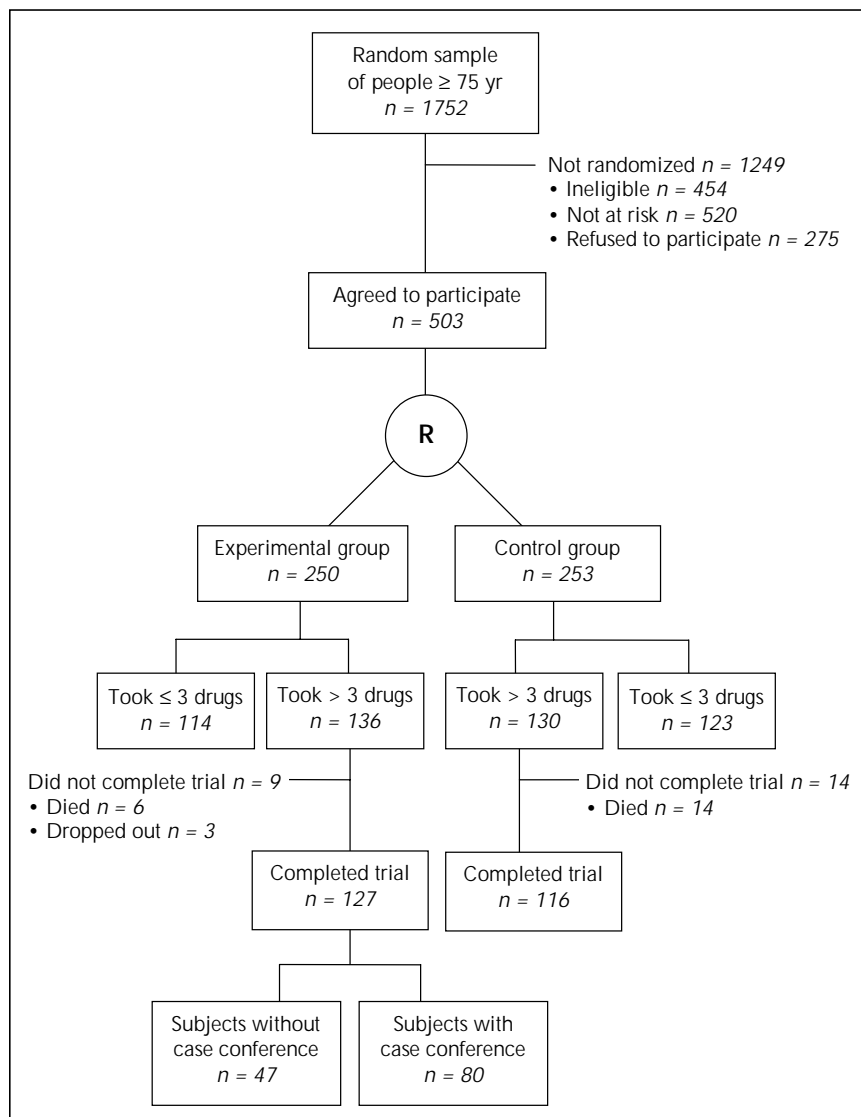

Fig. 1: Flow of subjects through the study. R = randomization. The case conference was a review of a subject's prescriptions and diagnoses by the research team.

ence was not statistically significant. Our sample was not sufficient to detect a 0.10 difference between the means (0.20 standardized difference). The fact that 38% of the subjects in the experimental group did not receive the full intervention also reduced the effective power of the study accordingly. However, it proved useful in reducing the number of subjects with at least one PIP, and there was also a 36% decrease in the mean number of PIPs in the experimental group that had the case conference; the difference between this group and the control group is close to the significance threshold ($p = 0.08$). With regard to the secondary outcome measures, our intervention showed no decrease in the number of drugs prescribed, and there was no effect on the global assessment of the drug profile.

Proof of the effectiveness of the intervention was limited by the improvement in the drug profile of the control group subjects. There was a 19% decline in the mean number of PIPs and an overall improvement in the drug profile of 18% of the subjects. Our analyses did not support the contamination hypothesis because there was a similar improvement in the control group subjects whose physicians did not have any patients in the study group and were, therefore, not exposed to the intervention.

Our hypothesis is that there was a general improvement in care for the elderly in the study area regardless of our program. The presence of a Geriatric University Institute in Sherbrooke and continuing medical education programs,

among other factors, could explain this potential confounder. Therefore, the conclusion must be that there is a general positive trend, at least in the area of the study, which in itself is good news.

Our study used a methodological approach that centred on the patient, not the physician. Hence, we have few data on the intrinsic quality of the prescriptions or the characteristics of the physicians themselves. Nor do we know how many physicians could have simultaneously prescribed drugs for the subjects in the study or how many pharmacies were involved for each subject. With regard to these questions, a study[20] has shown that the number of prescribing physicians was the most important risk factor for potentially inappropriate drug combinations and that the use of a single dispensing pharmacy lowered the risk.

The program did not have a significant effect. This may be attributable to the fact that it involved a single intervention over a 1-year period. During this period, many events could have occurred over which we had no control but which could have had an impact on the drug profile. In addition, the main author knew most of the patients' physicians, and this could have had a positive impact on the results. Finally, the fact that an improvement in the drug profile was noticed in only 25% of the subjects in the experimental group with case conferences is a result undoubtedly influenced by the lack of consensus on certain questions related to drug use (for example, whether to treat hyperlip-

Table 2: Comparison of the primary and secondary variables before the intervention program

Variable	Experimental group (n = 127)	p value	Control group (n = 116)	p value	Experimental group with case conference (n = 80)
Mean no. (and SD) of PIPs	0.77 (0.87)	0.54*	0.79 (0.77)	0.98*	0.85 (0.96)
No. (and %) of subjects with ≥ 1 PIP	72 (56.7)	0.48†	71 (61.2)	0.73†	47 (58.8)
Mean no. (and SD) of drugs prescribed	6.05 (1.76)	0.12*	6.50 (2.55)	0.48*	6.27 (2.55)

Note: PIP = potentially inappropriate prescription.
*Calculated using Student's t-test.
†Calculated using the χ^2 test.

Table 3: Difference between the preintervention and postintervention measures for the primary and secondary outcomes

Outcome	Experimental group (n = 127)	p value	Control group (n = 116)	p value	Experimental group with case conference (n = 80)
Mean no. (and SD) of PIPs	0.24 (0.69)	0.13*	0.15 (0.52)	0.08*	0.31 (0.77)
No. (and %) of subjects with ≥ 1 PIP	18 (14.2)	0.07†	8 (6.9)	0.04†	14 (17.5)
Mean no. (and SD) of drugs prescribed	0.24 (2.15)	0.46*	0.13 (1.67)	0.44*	0.31 (2.29)
No. (and %) of subjects with improvement in medications prescribed	27 (21.3)	0.82‡	21 (18.1)	0.50‡	20 (25.0)

*Analysis of covariance on the difference after the intervention program, controlling for the preintervention value.
†Logistic regression analysis controlling for the situation before the intervention program.
‡Calculated using the χ^2 test.

idemias in patients over the age of 75 years). Therefore, it is probable that some of our recommendations were not followed because physicians disagreed with them.

In conclusion, the results of this study suggest that the intervention program had no effect on the prescribing of PIPs. However, the program needs to be tested on larger samples of patients, on other populations of patients and, also, on randomized samples of physicians.

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