The Efficacy and Safety of Transdermal Scopolamine for the Prevention of Postoperative Nausea and Vomiting: A Quantitative Systematic Review

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The role of scopolamine administered via transdermal therapeutic systems in the prevention of postoperative vomiting, nausea, and nausea and vomiting is unclear. We performed a systematic search for full reports of randomized comparisons of transdermal scopolamine with inactive control. Dichotomous data were extracted. In the meta-analysis, relative risks and numbers-needed-to-treat/harm were calculated with 95% confidence intervals (CI). In 23 trials, 979 patients received transdermal scopolamine, and 984 patients received placebo. Sensitivity analyses were performed using restricted data for truncated control event rates (40%–80%) and for large trials. With these data, the relative risks for postoperative vomiting (five reports), nausea (five reports), nausea and vomiting (eight reports), and rescue treatment (three reports) were 0.69 (95% CI, 0.58–0.82), 0.69 (95% CI, 0.54–0.87), 0.76 (95% CI, 0.66–0.88), and 0.68 (95% CI, 0.54–0.85), respectively. This means that of 100 patients who receive transdermal scopolamine, approximately 17 will not experience postoperative vomiting who would have done so had they all received a placebo. However, 18 of 100 patients will have visual disturbances, eight will report dry mouth, two will report dizziness, one will be classified as being agitated, and 1–13 patients who are prescribed transdermal scopolamine will not use it correctly. The timing of application does not alter efficacy.

Prevention of postoperative nausea and vomiting (PONV) is a well-investigated research area. However, the fact that numerous studies were published on this topic does not necessarily indicate that daily practice to cope with this big little problem reflects the current best evidence. This was demonstrated for metoclopramide that is still frequently used to prevent PONV, although a systematic review of the literature with quantitative estimation of its efficacy suggested a rather limited value for the prevention of PONV. However, the fact that an antiemetic is inexpensive or was introduced a long time ago into clinical practice does not imply that its efficacy is less pronounced than more costly newer drugs, which was demonstrated for the use of dexamethasone or droperidol in this setting. This may similarly apply to the risk associated with an intervention: many patients are required to allow a reliable estimation of adverse effects. In this context, systematic reviews can help to summarize these findings and to create league tables of efficacy.

A systematic review of data on the efficacy and safety of transdermal scopolamine is lacking. Although in use for a long time, the use of scopolamine (hyoscine) as part of pharmacological premedication together with opioids was initially limited because of its short duration of antiemetic action when parenterally administered and the large peak plasma concentrations that resulted in undesirable side effects. A transdermal delivery system (skin patch) that continuously releases scopolamine for up to 3 days seemed to overcome these shortcomings (1).

The aim of this quantitative systematic review was therefore to evaluate the efficacy and harm of transdermal scopolamine for the prevention of PONV.

Methods

The study was performed in accordance with the QUORUM statement for conducting systematic reviews (2). Features not described extensively were

Accepted for publication March 19, 2002.
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DOI: 10.1213/01.ANE.0000021318.30928.AA

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0003-2999/02

Anesth Analg 2002;95:133–43

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comparable to other analyses on related topics (3). Relevant studies were full reports of randomized comparisons of transdermal scopolamine (active) compared with placebo or no treatment (control) in the prevention of PONV after regional or general anesthesia.

We searched MEDLINE, EMBASE, and the Cochrane Controlled Trials Register up to September 2001. Free text words used were postoperative, postanesthetic, postanaesthetic, or surgical, nausea, emesis, vomiting, or retching, and scopolamine or hyoscine. The German

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R/B/D = randomization, blinding, dropouts; F/M = female/male; N = number of patients; n = number with feature; NA = not applicable/no data available; E = experimental (active) group, C = control group; TIVA = total intravenous anesthesia; NLA = Neuroleptanesthesia.

*Active, patch covered with a dressing. Control, dressing alone.

**It was stated that "no patients complained of any side effects".
manufacturer of transdermal scopolamine (Scopoderm TTS®, Novartis Pharma GmbH, Nürnberg, Germany) was contacted and asked for additional information, including unpublished data.

Scoring according to the validated three-item, five-point Oxford scale was similar to previous analyses. Sponsorship was assumed if it was explicitly mentioned, if members of the pharmaceutical company were co-authors (if this was the case, we assigned 2 of 2 points for sponsorship), or if it was stated that active or placebo patches were provided by a manufacturer (we assigned 1 of 2 points for sponsorship). In all other jurisdictions, R/B/D = randomization, blinding, dropouts; F/M = female/male; N = number of patients; n = number with feature; NA = not applicable/no data available; E = experimental (active) group, C = control group; TIVA = total intravenous anesthesia; NLA = Neuraxonal anesthesia.
cases, no sponsorship was assumed (0 of 2 points were assigned) (Table 1). Dichotomous data on the frequency of postoperative vomiting (PV), postoperative nausea (PN), PONV, rescue treatment, and the occurrence of any associated adverse effects, including problems associated with the use of the transdermal therapeutic system, were extracted for distinct time intervals.

We investigated the inter-study variability analyzing the robustness of the results for clinically homogeneous subgroups. In addition, we intended to retrospectively explore methodological reasons for heterogeneity for the overall observation period (0–24 h). To allow for the usually observed variability in the control event rate (CER), i.e., the incidence of symptoms in the control group, estimates of the efficacy of studies with a pre-defined CER were calculated.

Data entry and statistical calculations were performed using the computer program RevMan 4.1 provided by the Cochrane Collaboration (http://www.cochrane.org). A random-effects model was applied to calculate relative risks (RR) and numbers-needed-to-treat (NNT) with 95% confidence intervals (CI).

Results

We retrieved 43 potentially relevant reports (Fig. 1). Twenty reports were later excluded. One report was an abstract,1 and eight reports evaluated the effect of scopolamine administered by other routes of administration than transdermal (4–11). In one study, the operation was performed under local anesthesia (12), three used combinations of drugs or used an uncontrolled observational design (13–15), one trial had a different setting, e.g., prevention of PONV during chronic opioid medication (16), two had no placebo or inactive control (17,18), one trial was not randomized (19), and three reports were labeled re-analyses of previously published data (20–22).

We eventually analyzed data from 23 randomized, controlled trials published between 1984 and 1996 (23–45) (Table 1). Pharmaceutical companies did not provide additional relevant references or data. In those trials including the 1963 subjects, 979 patients received an active intervention, and 984 patients received an inactive control. The median size of active and control groups was 25 patients (range, 12–133) and 25 patients (range, 12–130), respectively. The median Oxford scale was 3 (range, 1–5): 1 report scored 1, 1 scored 2, 10 scored 3, 9 scored 4, and 2 scored 5.

Data on the early postoperative period (0–6 h) were available in approximately half of the trials eligible for a quantitative analysis depending on the outcome. The remainder did not report data on an early period. Transdermal scopolamine significantly reduced the incidence of early emetic symptoms. Although there was considerable statistical heterogeneity within the pooled studies, only a few trials demonstrated no benefit or no clinically relevant benefit in the prevention of PV (31,37,41), PN (27,31,41), and PONV (27,31,35,41). Rescue treatment was sparsely reported for the early period, and scopolamine application did not exert a beneficial effect. Because of the small CER for this time interval (e.g., 21% for PV), absolute risk reduction and the NNT was in a range that might not be considered as a clinically relevant reduction in emetic symptoms. We did not perform subgroup analyses considering the underlying CER and other influencing factors because of the limited data that were available for this interval and the fact that overall efficacy seemed a more appropriate and relevant end point for the patient. Pooled results are shown in Table 2 (early).

Emetic symptoms as well as rescue treatments were much better reported for the overall observation period (0–24 h). Of 15 trials, 790 and 793 patients were eligible for the main outcome PV in the control and active group, respectively. RR to suffer from PV, PN, and PONV with transdermal scopolamine prophylaxis was 0.63 (95% CI, 0.55–0.73), 0.64 (95% CI, 0.51–0.80), and 0.66 (95% CI, 0.57–0.76), respectively, when data of all eligible trials were pooled. Considering mean incidences for PV, PN, and PONV of 48%, 56%, and 70%, respectively, this resulted in a NNT of 5.6, 4.3, and 3.8 when all trials that reported the outcome

of interest were pooled. In the trials that reported the use of rescue treatment, the CER for this intervention was 44%. Despite the significant reduction in terms of a RR of 0.77 (95% CI, 0.66–0.90), overall effect was limited and clinical relevance seemed limited when the NNT of 11 (95% CI, 8–25) is considered. Pooled results are shown in Table 2 (overall) and Figure 3A–D.

Route of application was given by the inclusion criteria so that the analyzed patients would be very homogeneous and comparable in these circumstances. Usually it was stated which manufacturer was chosen (Ciba-Geigy, Toms River, NJ, in almost all cases) and which site of application was chosen (usually on the skin behind the ear overlying the mastoid process). One study reported that in some of the patients, an alternative place was chosen if both ears were lying in the surgical field (28).

Restricting analyses on patients receiving general versus any kind of regional anesthesia or excluding children (25,30) in which the patch was sometimes reduced in its size did not result in more homogenous subgroups neither with respect to the CER, nor with respect to the obtained efficacy data. It was argued in original studies that the time of the application could be crucial to obtain a sufficient antiemetic efficacy. However, there was no significant difference if the patch was applied the night before surgery (RR, 0.59; 95% CI, 0.50–0.83) or applied before the induction or during the operation (RR, 0.65; 95% CI, 0.48–0.74) or applied before the induction or during the operation (RR, 0.65; 95% CI, 0.48–0.74)

When graphically exploring the data by means of Forest plots (Fig. 2) and L’Abbé plots (Fig. 3A–D), it seemed reasonable to retrospectively perform a subgroup analysis allowing for the size of the investigated groups and the underlying CER. Therefore, we performed the following analyses for two subsets of the data: studies that investigated at least one group with more than 30 patients versus all other studies. Incidences observed in the inactive group were plotted against the incidences of emetic symptoms by means of L’Abbé plots.

These analyses suggested better results reported by the summarized smaller trials (Table 3). In part, these differences can be explained by the different observed CER between small and large studies. This implies that the differences between the efficacy data that were based on absolute risk reduction (NNT) become smaller if studies that observe a CER outside a defined range (CER banding) are excluded. The CER banding was the second restriction of the performed sensitivity analyses. Underlined figures in Table 3 represent the efficacy data (RR and NNT) obtained using these truncated data sets (CER, 40%–80% for PV, PN, and PONV; CER, 30%–60% for rescue treatment).

Characteristic adverse events (side effects) were reported in a dichotomous way in 17 of 23 trials (Table 1). One additional trial reported that no patients complained of any side effects. Not valid for further analysis were statements that side effects did not differ between the treatment groups.

For the sake of clarity, some descriptive terms of side effects were pooled and analyzed together, e.g., mydriasis, blurred vision, and amblyopia. The latter were reported in 14 trials. Dry mouth was reported in 12 trials, dizziness in eight trials, and agitation/confusion in seven trials. Some trials reported urinary retention (four trials), local skin irritation (four trials), sedation/somnolence/drowsiness (four trials), headache (two trials), anxiety (one trial), and problems with orthostatism (one trial).

Incidences of visual disturbances, dry mouth, and symptoms of agitation were almost consistently more frequent in the active versus control group; only one of 14 trials (visual disturbances), one of 12 trials (dry mouth), and no trial for agitation reported the opposite (i.e., an increased incidence of symptoms in the placebo group). Visual disturbances (RR, 2.15; 95% CI, 1.46–3.16) and dry mouth (RR, 1.49; 95% CI, 1.13–1.96) occurred significantly more often in the active group.

Table 2. Overall Pooled Results of the Meta-Analysis of Randomized, Controlled Trials on the Efficacy of Transdermal Scopolamine. Results are Given for the Early Observation Interval (0–6 h) and the Overall Observed Period (0–24 h)

<table>
<thead>
<tr>
<th>Outcome</th>
<th>Observation interval</th>
<th>Trials (n)</th>
<th>Active/placebo treated patients (n)</th>
<th>Relative risk</th>
<th>Number needed to treat/[harm]</th>
<th>References</th>
</tr>
</thead>
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<tr>
<td>PV</td>
<td>early (0–6 h)</td>
<td>9</td>
<td>518/520</td>
<td>0.65 (0.45–0.94)</td>
<td>12.5 (6.7–∞)</td>
<td>(24; 26–29; 31; 32; 37; 41)</td>
</tr>
<tr>
<td></td>
<td>overall (0–24 h)</td>
<td>15</td>
<td>790/793</td>
<td>0.63 (0.55–0.73)</td>
<td>5.6 (4.0–9.1)</td>
<td>(26; 28–33; 37–41; 43–45)</td>
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<td>PN</td>
<td>early (0–24 h)</td>
<td>8</td>
<td>392/383</td>
<td>0.63 (0.45–0.89)</td>
<td>6.3 (3.7–25.0)</td>
<td>(24; 27–29; 31; 32; 37; 41)</td>
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<td>overall (0–24 h)</td>
<td>13</td>
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<td>0.64 (0.51–0.80)</td>
<td>4.3 (2.9–8.3)</td>
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<td>0.68 (0.51–0.92)</td>
<td>6.3 (3.7–25.0)</td>
<td>(24; 26–29; 31; 32; 35; 37; 41)</td>
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<td>overall (0–24 h)</td>
<td>20</td>
<td>928/929</td>
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<td>[33.3 (10.0–∞)]</td>
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</table>

Confidence Intervals (95%) are given in parentheses. PV = postoperative vomiting; PN = postoperative nausea; PONV = post-operative nausea and vomiting.
compared with the control group. Data on dizziness were rather heterogeneous, and an equal number of trials reported protective and negative effects with the active group (RR, 1.13; 95% CI, 0.71–1.79). Agitation and sedation occurred more often in the active group. However, based on small absolute incidences, this difference failed to reach statistical significance (RR for agitation, 2.15; 95% CI, 0.78–5.92; RR for sedation, 2.01; 95% CI, 0.59–6.84). Numbers-needed-to-harm (NNH) for the 4 most consistently reported adverse effects were 5.6 (95% CI, 3.1–33.3) for dry mouth, 12.5 (95% CI, 7.7–33.3) for visual disturbances, 50.0 (95% CI, 9.1–infinity) for dizziness, and 100.1 (95% CI, 33.3–infinity) for agitation.

In seven trials, dichotomous data on problems with the use of the scopolamine patches could be extracted (Table 1). These included, for instance, failures to apply the patch or unintentional patch removal either before surgery or within the study period. The mean prevalence of such difficulties was 4.7% (range, 1.2%–12.6%). In terms of an absolute risk reduction, this means that of 100 patients receiving transdermal scopolamine, in approximately 1 to 13 patients it will not work properly because of problems associated with the correct use of the patch, assuming a best and worst case scenario.

**Discussion**

The main findings of this systematic review are that transdermal scopolamine significantly reduces the risk of suffering from emetic symptoms in the postoperative period. The most reliable point estimate of efficacy for the prevention of PV seemed to be a RR of 0.69 (95% CI, 0.58–0.82). Using a more clinically relevant measure of efficacy, this means that of 100 patients having anesthesia for a surgical procedure who receive standardized transdermal scopolamine, 17 (NNT = 5.9) will not vomit in the postoperative period who would have done so had they all received a placebo. Results apply to a patient receiving the patch at any time before the occurrence of symptoms (prevention) not being restricted to a specific type of surgery. These results take into account that smaller trials report efficacy data that are far better, and a large variation in the CERs also influences the results of the analyses, i.e., these data are based on the larger ones of the included studies restricted to those with a defined range of CERs.

The statistical technique of combining results from randomized, controlled trials in systematic reviews is well established, and there are an increasing number of articles using this method for systematically reviewing the literature. Specific questions arising from meta-analyses on PONV (e.g., using an appropriate end point [dichotomous data]) or methodological issues (e.g., using the NNT as a measure of effectiveness) are already extensively discussed. Thus, we focus on two major issues that arise from this meta-analysis.

**Dealing with Heterogeneity**

Meta-analyses usually provide a powerful tool to further a realistic estimation of the efficacy of an intervention. The primary aims in the process of pooling results from valid resources are to minimize the risk of any kind of bias and to get valid results that appropriately reflect the truth.
Table 3. Results of the Meta-Analysis of Randomized Controlled Trials on the Efficacy of Transdermal Scopolamine. Results are Presented for Large (At Least One Group with More Than 30 Patients) Versus Small Studies (All Other Trials).

<table>
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<th>Outcome</th>
<th>Observation interval</th>
<th>Trials (n)</th>
<th>Active/placebo treated patients (n)</th>
<th>Relative risk (95%)</th>
<th>Number needed to treat</th>
<th>References</th>
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<td>3.6 (2.7–5.0)</td>
<td>(28–30; 33; 40; 43; 44)</td>
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<td>Large studies</td>
<td>8</td>
<td>135/138</td>
<td>0.44 (0.31–0.63)</td>
<td>3.3 (2.4–5.0)</td>
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<td>PN</td>
<td>Small studies</td>
<td>7</td>
<td>344/343</td>
<td>0.69 (0.58–0.82)</td>
<td>5.9 (4.2–11.1)</td>
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<td>Large studies</td>
<td>2</td>
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<td>0.65 (0.31–1.38)</td>
<td>5.3 (2.0–∞)</td>
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<td>PONV</td>
<td>Small studies</td>
<td>11</td>
<td>233/235</td>
<td>0.46 (0.34–0.63)</td>
<td>2.5 (1.9–3.6)</td>
<td>(25; 28–30; 33; 34; 36; 40; 42–44)</td>
</tr>
<tr>
<td></td>
<td>Large studies</td>
<td>8</td>
<td>182/181</td>
<td>0.49 (0.37–0.63)</td>
<td>2.9 (2.2–4.2)</td>
<td></td>
</tr>
<tr>
<td>Rescue</td>
<td>Small studies</td>
<td>4</td>
<td>695/694</td>
<td>0.78 (0.69–0.89)</td>
<td>7.1 (5.0–12.5)</td>
<td>(23; 26; 31; 32; 37–40; 41; 45)</td>
</tr>
<tr>
<td></td>
<td>Large studies</td>
<td>8</td>
<td>658/664</td>
<td>0.76 (0.66–0.88)</td>
<td>6.7 (4.5–12.5)</td>
<td></td>
</tr>
</tbody>
</table>

Underlined figures represent the efficacy data for a defined control event rate (CER) banding to allow for comparisons with other analyses. Confidence intervals (95%) are given in parentheses.

PV = postoperative vomiting; PN = postoperative nausea; PONV = postoperative nausea and vomiting.

An essential prerequisite is to create clinically homogeneous groups to prevent “apples” from being compared to “pears.” But as to what extent groups should be homogeneous from a clinical point of view is far from clear. This question is addressed in the controversy about lumping, i.e., pooling all retrieved data, versus splitting, i.e., calculating results for small, well-defined populations. Both strategies have been applied in previous meta-analyses on PONV (46, 47). Fortunately, the type of intervention in this review is very focused compared with other reviews on antiemetics that dealt with different doses and routes of the administration. Because the type of anesthesia (balanced versus inhaled versus neuraxial blocks) and the timing of the application of the scopolamine patch seem not to influence results, we pooled the results. This may also be convenient for daily practice where a global estimate of an interventions efficacy is helpful. The timing was discussed as a crucial factor in some studies with a reasoning based on pharmacological arguments, but no direct comparisons are available so far, and our findings that differences are negligible must be handled with caution. Furthermore, we cannot exclude that transdermal scopolamine is better suited for distinct types of procedures. The fact that subgroup analyses based on the type of operation (i.e., considering solely otorhinolaryngology procedures) resulted in better efficacy data is only sufficient to generate a hypothesis and not to suggest that this might be the truth for two reasons. First, such assumptions would be based on relatively few patients, and the precision of any point estimate of the calculated efficacy data is small. Second, interaction with the study size might explain the observed difference, because with respect to the mentioned otorhinolaryngology procedures, all three eligible reports belong to the group of the small trials.

Graphically exploring the data summarized in meta-analyses may represent a valuable tool to find underlying influences that may account for certain results. We have done this using characteristic Forest plots and L’Abbé plots. Because we had no data on the true underlying risk (48), we had to assume that this variability is caused by the different underlying risks, and we took this control event rate as a surrogate marker for the risk, as previously suggested (49). This was the rationale for the sensitivity analysis with truncated data within a restricted range of CERs (49).

The hints for the dependency of efficacy data upon trial size suggested that a subgroup analysis with an explorative character should be performed. The cutoff point of trials with more than 30 patients in at least one group versus the other trials was arbitrarily chosen, because it divided the trials eligible for analysis into two subsets with almost identical numbers of trials. Statistically, it might be more appropriate to set the cutoff point in the range of 75 patients per group, thus circumventing a possible confounding factor in terms of the so-called publication bias, i.e., that studies with positive results are getting published more readily than trials that show no or minor effects. This number derived from a power-analysis reflects a minimal group size that is required to confirm negative effects with adequate power ($\alpha = 0.05$, $\beta = 0.2$), taking
into account a baseline incidence of 60% and assuming a relative reduction of 40% (absolute reduction with the active intervention to 36%). We did not apply this approach because restricting analyses on studies with group sizes of more than 75 patients and considering the restriction for the CER would leave only two reports (26,32) eligible for the pooled analysis with respect to the outcome PV restricted to large studies. Therefore, we would like to argue that the considered threshold of 30 patients represents a suitable compromise between trustworthiness and precision of the efficacy data. However, it may be

Figure 3. (A–D) L’Abbé plots of trials investigating overall efficacy of transdermal scopolamine in the prevention of (A) postoperative vomiting (PV), (B) postoperative nausea (PN), (C) postoperative nausea and vomiting (PONV), and (D) the need for rescue treatment. Symbols are comparisons between scopolamine arms and control arms. Line with 45° slope indicates equality.
argued that the statistical model already takes into account, at least partially, the trial size when a specific weight of an individual trial on the overall pooled results is attributed by the model used in the computer program. Nevertheless, we considered it appropriate to further explore this potential association because it was previously shown to exert a considerable effect on the efficacy estimation (50). In addition, relying on small trials has led to questionable conclusions in meta-analyses (51) that could not be confirmed by a subsequent larger trial (52). These restrictions led to a strong increase in homogeneity as calculated by means of the meta-analysis software. However, from simulated models, we do know that heterogeneity statistics must be handled with caution (53). Therefore, our approach does not claim to have found the absolute truth. It does seem to provide a hint that trial size and baseline incidences are important in the efficacy evaluation, thus confirming previous results on this issue related to meta-analyses.

When both restrictions were applied, we obtained the underlined figures of efficacy as presented in Table 3. With a NNT of five to seven for the prevention of emetic symptoms in a moderate to high-risk setting, these results do not seem reassuring. However, it should not be left unmentioned that efficacy data for ondansetron are in the same range (3).

**Drug-Related Adverse Effects**

One potential advantage of systematic reviews may lie in the detection of significant associations of rare outcomes with certain interventions. The latter usually cannot be obtained with certainty from single studies that are designed to accept or reject a null hypothesis of a more frequent effect (usually a treatment effect). Unfortunately, in some systematic reviews this potential methodological strength was not so obvious because reporting of adverse effects was rather sparse. This was not the case in this analysis on the safety of transdermal scopolamine. After all, 17 of 23 trials reported side effects in a dichotomous way so that they were eligible for further analyses. The two most consistently reported side effects were visual disturbances and dry mouth in 14 and 12 trials, respectively. The results suggest that of 100 patients who receive standardized transdermal scopolamine, 18 (NNH = 5.6) will have visual disturbances, eight patients will report a dry mouth (NNH = 12.5), two patients will report dizziness (NNH = 50.0), and one patient would be classified as being agitated (NNH = 100.1) who would not have reported these side effects had they all received a placebo.

It may be questioned why the readiness to report adverse effects differs largely across various studies on one topic, e.g., scopolamine trials, and between studies that investigate various interventions, e.g., trials that investigate ondansetron versus trials on dolasetron. The question arises as to how to judge the validity of data in trials that state that no adverse effects were observed in either the control or the treatment groups. However, it may be that the initial clinical or experimental reports that focus on an intervention act as a paragon for consecutively performed trials and call them to explicitly investigate these side effects rather than waiting for what patients report spontaneously. If this is assumed, it may itself introduce a bias when comparing side effects of different interventions. We should be aware not to consider interventions as inferior to others simply because studies were performed in a more thorough manner than with competitive interventions. However, because head-to-head comparisons including an additional inactive control group are rare because of the large numbers of patients that are required, we have to rely to some extent on indirect comparisons. Sponsorship may also constitute a relevant factor. In summary, it is our impression that, in trials on antiemetics, the fact that a sponsor is involved does more good than harm with respect to the reporting of side effects. This becomes very obvious when data on dolasetron are considered, where most of the trials were performed with an obvious research agenda and most large trials were supported by the manufacturer (54).

Using transdermal therapeutic systems, it may also be wise to report dropouts because of problems with the administration of the system itself. This would more accurately allow the calculation of comprehensive efficacy data and judgment of the clinical performance of a treatment option. In this analysis, less than one-third of all trials reported problems associated with the use of the system itself.

In conclusion, in the postoperative setting after anesthesia, transdermal scopolamine is antiemetic compared with placebo. Time of application (night before versus before surgery) seems to be a negligible factor. Scopolamine’s antiemetic effect is associated with side effects, such as visual disturbances or dry mouth, which should be considered when scopolamine patches are prescribed.

We thank Novartis Pharma GmbH, Nürnberg (Germany), for responding to our enquiry.

**References**


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