PostScript

LETTERS

Studying the Hungarian anti-smoking movement

Carter describes how tobacco companies infiltrate into tobacco control movements in order to damage their efforts.[1] Industry documents on Hungary suggest similar intentions. The transnational tobacco corporations (TTCs) jumped into the new market and privatised the factories of the formerly state owned Hungarian tobacco monopoly in the very first years of the transition from communism (1991-92).[2] Using their sophisticated lobbying practices, the TTCs succeeded in transforming the regulatory framework of tobacco and easing marketing and trade restrictions on their products. As Philip Morris put it, they sought to protect "the legitimate interests of the company . . .against discriminatory or unfair legislation and practices".[3]

The Hungarian anti-smoking movement was relatively inexperienced in neutralising the political and economical power of a wealthy and influential industry. Nonetheless, documents show the TTCs intended to monitor closely and counteract its efforts.

In February 1993, Gabor Garamszegi, CA Manager of Philip Morris Hungary, received a research plan aimed at assessing "the social context of smoking in Hungary". The submission[4] came from the formerly state owned Tobacco Institute (Dohánykutató és Minõségfejlesztõ Intézet Kutató-Fejlesztõ Rt.), which had no previous experience in assessing the social and health issues in tobacco use. The plan states that "tobacco and smokers have become ostracized among the health-maniac snobs" and its authors considered smoking nothing more than "a scapegoat for the deteriorating health condition of the population".

The authors acknowledged that the tobacco control "snobs" had succeeded in putting tobacco control higher on the political agenda and gained power from the increasing involvement of its members into the international tobacco control efforts. This "challenge requires appropriate reactions from the tobacco industry", with the document proposing that a panel of smoking volunteers be formed who could be "regularly questioned to learn the public opinion on social issues". Members of these panels should be sent to collect information with the aim of learning more about the programmes of anti-tobacco organisations: "As a possible method it could be envisaged that members of the panels . . .also take part in these programs and pass on their experiences to the leaders of the panels."

Another document also mentions the "tight monitoring of activities and plans of government and anti-smoking groups" as an important strategy to "maintain the social acceptability of smoking", since the "growing anti-smoking sentiments . . .would damage ability of the company in all business area to represent and defend company interests".[5]

More recently, British American Tobacco has engaged in launching a "social dialogue" with tobacco control advocates and government based agencies. This is another effort of TTCs to portray themselves as if they are changed, contrite, and reformed.[6]

Hungary today faces an increasing epidemic of smoking related diseases, with 28 000 deaths (3.5 million people of 10 million population are smokers) attributable to smoking every year. The country ranks first in the world regarding mortality from lung and oral cancers.[7]

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Events of 11 September 2001 significantly reduced calls to the New Zealand Quitline

New Zealand has a national (free) telephone Quitline service that is promoted through regular mass media campaigns. Data are routinely collected on the over 100 callers per day. We used this data source to investigate the impact of the 11 September 2001 terrorist attacks in the USA on calls to this service.

On Wednesday 12 September (11 September in New York was 12 September in New Zealand) there was a sudden decline in the number of new callers to the Quitline (only 137 callers relative to 237 on the previous day—a 42% reduction). Similarly, relative to the preceding Wednesday, the number of new callers was down by 41%.

The effect was felt for at least several weeks. There was an overall 35% drop in the total number of new callers per week, when comparing the five weeks before 11 September with the five weeks afterwards. Using a generalised linear model we found an interaction between a "September 11" effect and time (week) ($p = 0.002$). Details of the model and the graphed results are available on a website.[1]

It appears that quitting "dropped off the personal agenda" for some New Zealand smokers in September 2001. It seems likely that at this time of increased media publicity of global security threats, the quitting plans of smokers were eclipsed by other concerns (for example, the psychological impact of these events appears to have been significant—at least for Americans[2]). This was despite the fact that New Zealand is an island nation that is very far removed from international trouble spots. It was also despite the fact that international terrorism has historically posed only a tiny risk of death to the general public relative to that from smoking (which kills half of long term smokers).

This reduction in calls is of concern considering that the Quitline (especially in the context of providing subsidised nicotine replacement therapy (NRT)) appears to be very successful in supporting quitting. Preliminary data from one survey suggests a point prevalence quit rate at three months of 44%.[3]

Other explanations for this sudden and sustained reduction in calls to the Quitline from 12 September seem unlikely. Nevertheless, this decline in new callers did occur in the context of a longer term decline in calls to the Quitline which had been occurring since a peak in November 2000. That peak was a result of callers becoming eligible to obtain vouchers for heavily subsidised NRT through the Quitline service.

One implication of this relation between global security issues and Quitline calls is that publicity for Quitline services may be less cost effective at times of perceived international crisis. However, the continuance of at least 120 calls per day to the Quitline, during September and October 2001, indicates the strength of the desire to quit in the population of smokers that the Quitline has tapped into.

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Big Mac index of cigarette affordability

As for any other commodity, demand for tobacco responds to price changes: when prices rise, demand for tobacco falls. Price increases encourage cessation,[1] reduce average cigarette consumption among continuing smokers,[2] and deter initiation.[3] Tax increases are thus widely accepted as a key component of tobacco control policy.[4–6]

Table 1 Cigarette prices in $US and tax levels compared to Big Mac index of cigarette affordability

Country	Price of 20 cigarettes ($US)	Total tax (%)	Cigarettes per Big Mac
Britain*	$6.33	79.5	9
Ireland*	$4.46	79.0	12
USA*‡	$4.30	27.7	12
Australia*	$4.02	68.9	9
Singapore**	$3.99	53.0	9
Hong Kong*	$3.97	52.0	7
New Zealand*	$3.88	74.5	10
Denmark*	$3.77	81.7	17
Sweden*	$3.64	70.5	15
Canada †	$3.80	71.1	11
Finland*	$3.53	79.0	15
France*	$2.76	75.5	20
Germany*	$2.76	68.9	18
Belgium*	$2.63	73.8	21
Netherlands*	$2.56	73.0	19
Austria*	$2.37	73.7	20
Japan**	$2.18	61.0	19
Luxemburg*	$1.94	67.7	30
Italy*	$1.93	74.7	24
Greece*	$1.79	72.8	22
Spain*	$1.66	71.2	28
Portugal*	$1.63	80.7	26
Malaysia**	$1.21	34.0	22
South Korea**	$1.02	68.0	50
Poland**	$0.92	69.0	32
Taiwan**	$0.91	44.0	45
Thailand**	$0.80	56.0	32
Brazil**	$0.57	75.0	50
Philippines**	$0.44	41.0	59
Indonesia**	$0.43	48.0	86

Based on the most popular price category.
Sources: *Smoking and Health Action Foundation; **Ash UK.
†Sales weighted average (reflects 17 June 2002 increase); ‡average of highest (New York) and lowest (Kentucky).

In calling for increases in tobacco tax, tobacco control advocates often find it useful to compare cigarette prices internationally with those in their own country. To do this, they must somehow convert prices in other countries using a standard measure, most commonly the price in $US. Exchange rates, however, may be influenced by many factors including inflation differentials, monetary policy, balance of payments, and market expectations.[7] Guindon *et al* proposes "purchasing power parity" (PPP) as a more appropriate measure for comparison. This theory argues that exchange rates are only at their "correct" levels when they are equal to the ratio of the two countries' price level of a fixed basket of goods and services.[8] Developing indices of PPP is a fairly time consuming exercise. *The Economist's* Big Mac index,[8] by contrast, provides a "quick and dirty" estimate of the extent to which various currencies may be under or over valued. McDonalds' Big Mac hamburgers are produced to more or less the same recipe in 120 countries and can be regarded as identical for currency translation. The "Big Mac PPP" is defined as the exchange rate that would result in hamburgers costing the same in the USA as elsewhere.[8]

While Big Mac prices may not perfectly represent a total basket of goods and services—meat prices for instance might vary in different markets—the Big Mac PPP does appear to compare favourably with other more rigorous estimates of purchasing power.[9]

To produce an update of Scollo's Big Mac index of cigarette affordability[10] we obtained Big Mac and cigarette prices in 30 countries. Big Mac prices were obtained from *The Economist* magazine[8] and through phone calls to a further 11 McDonalds restaurants worldwide (Dublin, Brugge, Amsterdam, Rome, Barcelona, Lisbon, Vienna, Stockholm, Helsinki, Athens, and Luxemburg, 28–31 May 2002). We used cigarette price and tax levels compiled by the Canadian NSRA[11] and ASH UK[12] and exchange rates as at 31 May 2002. We then divided the (local currency) price of a Big Mac in each country with the (local currency) price of a single cigarette (fig 1). Cigarette prices in $US and tax levels in 30 countries have been tabulated (table 1). The number of cigarettes per Big Mac provides a slightly different picture of relative affordability of cigarettes than that provided by a simple conversion to $US.

While by no means a perfect measure, the Big Mac index of cigarette affordability provides a reasonable estimation of relative affordability of cigarettes in the countries listed.

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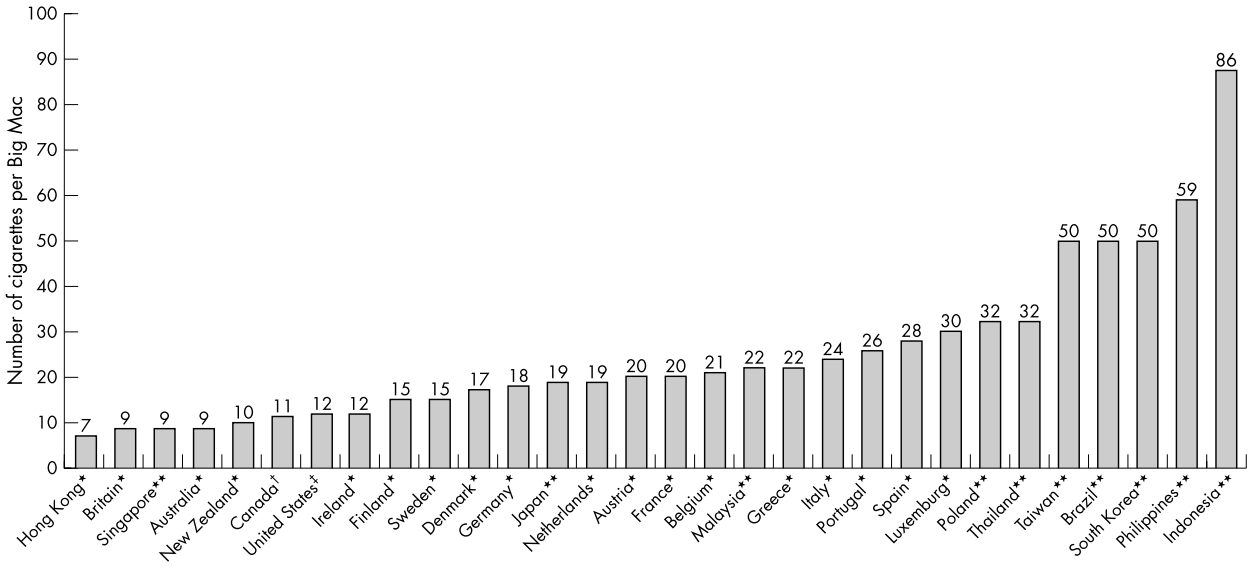

Figure 1 Big Mac ranking of world cigarette prices. Sources: *Smoking and Health Action Foundation; **Ash UK. †Sales weighted average (reflects 17 June 2002 increase); ‡average of highest (New York) and lowest (Kentucky).

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Is it time to abandon youth access programmes?

In their editorial "It is time to abandon youth access tobacco programmes", Ling *et al*[1] base their argument on an in press meta-analysis of youth access interventions by Fichtenberg and Glantz.[2] These authors conclude that there is no proof that youth access interventions work to reduce youth smoking rates. Sadly, this analysis includes 10 methodological flaws, each one of which individually renders the conclusions scientifically invalid.[2] One of the invalid figures from the Fichtenberg analysis has been reprinted in *Tobacco Control*.[1]

Three of the eight studies included in the meta-analysis did not involve any actual enforcement of the law, and the authors of a fourth study concluded that enforcement was inadequate because of a political backlash from merchants.[3–7] The inclusion of at least three of these studies is scientifically unjustifiable as it has been established for over a decade that merchant education programmes alone are ineffective at attaining the levels of merchant compliance that can be expected to reduce youth access to tobacco.[8 9] Three out of the five studies included in the analysis of the effects of youth access restrictions on past 30 day smoking did not involve enforcement. The authors inappropriately list the Baggot study as including enforcement and fines when in fact the inspection method was so flawed that no merchant was ever caught and none were prosecuted.[4]

In the Baggot study, merchant compliance is reported as 100%.[4] None of the stores sold to youths aged 13 years or under during enforcement checks, yet 100% of smokers among the community youths surveyed reported that they regularly bought tobacco from stores and only rare subjects reported ever having been turned down. The study's authors correctly concluded that the compliance inspections were an invalid measure of youth access. Yet Fichtenberg and Glantz included this invalid data in the analyses of a threshold effect and it is also included in the figure printed in *Tobacco Control*.[1 2]

It was improper to include a study from England where the legal age is 16 years as the majority of secondary school students would be of legal age to purchase and no impact on youths ages 14–15 would be expected.[4]

It was improper to include the study from Australia. In addition to the fact that the study involved no enforcement, 46% of the students in the intervention group actually lived outside the intervention area![5]

The meta-analysis improperly combined studies of different designs including cohort, cross sectional, controlled interventions and non-controlled interventions.

Combining these studies is also inappropriate because the ages of the youths, and the methods used to test compliance, differed dramatically from study to study. For example, a compliance rate of 82% for a 14 year old is equivalent to a compliance rate of 62% for a 17 year old.[10] A compliance rate of 42% for behind the counter sales is equivalent to a compliance rate of 58% for self service sales.[11] Differences in the techniques used to measure compliance render all of the computations and conclusions in this paper invalid.

The authors' basic premise is that the percentage change in merchant compliance should correlate with the percentage change in the prevalence of youth smoking. The use of this measure represents a straw man. In my review of 176 articles concerning youth access, I cannot recall anybody in this field ever suggesting that the change in percentage of merchant compliance is an appropriate measure of youth access. To the contrary, there is wide agreement among experts in this field that absolute levels of merchant compliance above 90%, as measured through realistic compliance checks using youths close to the legal limit, will be necessary to effect a change in the prevalence of youth smoking.[12]

In the figure presented in the *Tobacco Control* editorial,[1] intervention communities are being inappropriately compared to control communities from other continents and legal systems. If the authors wanted to compare smoking rates and youth access interventions across communities, a random sample should be used, uniform measures should be employed, and other confounding factors such as socioeconomic status and the cost of tobacco should be controlled for. When this type of analysis has been performed on a community and state level of analysis, reductions in youth smoking have been observed.[13 14]

It has been known for centuries that the prevalence of smoking increases during adolescence. This factor must be controlled for in cohort studies by the inclusion of a matched control group. During the period when most of these studies were conducted there was a secular trend of dramatically rising teen smoking rates observed in English speaking countries. Since merchant compliance would also be expected to increase over time in these intervention studies, it would be expected that a *positive* association between the intervention and smoking prevalence would be seen in both cohort and cross sectional studies if enforcement were completely ineffective. The meta-analysis does not appropriately incorporate control communities for each intervention community. Only three control communities are included for 15 intervention communities across seven studies.

In the same analysis, the few control communities are inappropriately included as additional "data points" in the mix. Baseline data rather than outcome data were used for one intervention community. These procedures indicate that the intention of this analysis was not to determine the impact of the interventions as the authors state.

The Fichtenberg and Glantz article[2] is strongly reminiscent of the "scientific" papers secretly commissioned by the now defunct Tobacco Institute. It is sad that the scientific literature continues to be poisoned for political ends. The *Tobacco Control* editorial[1] which was based on this travesty of science also excludes and misinterprets data which contradict the authors' long held biases.[15]

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Authors' replies

Since DiFranza's criticism of the editorial by Ling *et al*[1] concentrates mostly on criticism of the paper by Fichtenberg and Glantz, published in *Pediatrics*,[2] we are writing to respond to these criticisms separately. We recognise that this is unusual, since the standard procedure would have been for DiFranza to write to *Pediatrics* after the paper was published there. DiFranza, however, chose to write to *Tobacco Control* (based on a preprint we provided him as a courtesy), so we are responding here.

The premise of youth access programmes is that if merchant compliance reaches a high enough level, it will reduce youth access to cigarettes and, therefore, youth smoking. The goal of our analysis was to see if, based on the available literature, there was a relation between merchant compliance and youth smoking. Whether or not the laws were being enforced at the time and, if so, in what manner, is irrelevant to this analysis. If youth access programmes work because high merchant compliance leads to lower smoking, there should be an association between high merchant compliance rates and low youth smoking rates, regardless of what led to those rates of compliance. If an intervention designed to increase merchant compliance was successful, we should see high compliance rates and low smoking. If the intervention was not successful, because they did not include enforcement as DiFranza suggests, then we should see low compliance and low smoking. Both of these cases would contribute to our test of the hypothesis that increased merchant compliance was associated with reduced smoking. The data to not exhibit such an association (fig 1A of Fichtenberg and Glantz[2]).

All youth access programmes measure merchant compliance through undercover sales attempts by underage youth, as was done in the Bagot[3] study. If merchant compliance measured in this way is not an accurate reflection of youth access, then none of the studies of youth access that base their effectiveness on merchant compliance are valid. The goal of our analysis was not to determine if compliance is a good measure of youth access, but rather to relate the most commonly used metric for measuring the effectiveness of youth access programmes, namely merchant compliance, to youth smoking rates.

DiFranza says that we should not include studies from England because the legal age to purchase cigarettes is 16 years. We see no reason why youths aged 14–15 would not be affected by laws limiting purchase of cigarettes to those 16 and older.

DiFranza objects to including data from Australia, because 46% of the students lived outside the enforcement area.[4] As discussed above, whether or not active enforcement was involved is irrelevant to our analysis of the association between merchant compliance with youth access laws and youth smoking prevalence. All that is important is that compliance and smoking was assessed in the same community. In this case the authors point out that for the follow up survey, 46% of students in the intervention community—which was defined based on school location—did not live in the intervention area. They conclude that this would be a problem if these children bought cigarettes closer to home rather than to school. Since there was no residence information from the baseline survey it was not possible to limit the analysis to students living in the intervention area. Nevertheless, we chose to include the study in our analysis despite this limitation. It is important to note that the results of this study were consistent with the others.

There is no problem with combining studies of different design in a quantitative meta-analysis, as long as all studies are measuring the same end point.[5 6] As was reported in the methods section of our paper, the quantitative meta-analysis only included controlled studies.

DiFranza objects to combining studies because the ages of the youths, and the methods used to test compliance, differed. While we agree that factors such as age and sex of the youths may impact measured merchant compliance, we did not expect this variability to mask the effect of youth access programmes, if they actually affected youth smoking rates. The small number (five) of controlled studies of youth access programmes which reported youth smoking made it impossible to stratify according to the age of the youths used in the compliance checks.

DiFranza objected to our evaluation of the change in youth smoking prevalence as a function of change in merchant compliance on the grounds that it is necessary to obtain compliance rates above 90% to have an effect on youth smoking prevalence.[7] In addition to the fact that the data show no empirical evidence to support the hypothesis of such a threshold (fig 1A in Fichtenberg and Glantz,[2] reproduced as fig 1 in Ling *et al*[1]), our basic premise is that if youth access programmes actually reduced youth smoking, higher compliance rates would be associated with lower youth smoking rates. We examined this hypothesis in two ways. First, we compared compliance and smoking rates in all communities for which both variables were measured at the same time. Since this is an ecological analysis which does not take into account trends over time, we then examined the relation between changes in compliance and changes in smoking in case what mattered was whether there was a reduction in sales to youth rather than the absolute level of compliance at one time (fig 1B in Fichtenberg and Glantz[2]). The data presented in fig 1A show that there is no threshold of effectiveness at 90% compliance. Smoking rates for communities with compliance above 90% vary between 19.4–32.5%, with a mean of 25.9%. In communities with compliance rates below 90%, smoking rates vary between 15.6–37.7% with a mean of 25.7%. There is no evidence of a threshold of effectiveness.

DiFranza suggested that we control for a wide variety of socioeconomic and demographic factors, because "When this type of analysis has been performed on a community and state level of analysis, reductions in youth smoking have been observed".[8 9] Given the small number of studies available, it was not possible to explore the effects of potential confounders such as other tobacco control policies, price of cigarettes, and socioeconomic status. Nonetheless, in our discussion we report the results of population based studies, including but not limited to, those referred to by DiFranza. Chaloupka and Pacula,[9] in the study cited by DiFranza, do indeed find that statewide enactment and enforcement of youth access laws was associated with reduced youth smoking. However, in another analysis[10] the same authors found that this effect was restricted to black teens. The study by Siegel *et al*[8] does indeed find that the presence of youth access laws was associated with decreased smoking initiation rates; however, they conclude that this decrease was not mediated by decreased access because youths reported no decrease in perceived access.

In the first part of our analysis (fig 1A), we compared compliance and smoking in all communities for which there was information. Since we were not trying to assess the effects of interventions but rather to see if there is a relationship between compliance and smoking, we did not make a distinction between control and intervention communities, or between baseline and follow up data. As DiFranza points out, this type of analysis does not take into account temporal trends or other potential confounders. In order to take these into account we performed a quantitative meta-analysis using only controlled studies (n = 5). This analysis yielded a pooled effect of a 1.5% decrease in youth prevalence (95% confidence interval 6% decrease to 3% increase).

Tutt cited a paper by his group[11] that was not included in our meta-analysis because it was not listed in Medline or cited in any of the other papers we located. Adding his results to those we report, however, does not affect the conclusions of our analysis. The correlation between merchant compliance and 30 day teen smoking prevalence including these data is 0.042 (p = 0.799) compared with 0.116 (p = 0.486) reported in fig 1A of our paper.[2] Likewise the correlation between change in merchant compliance and change in youth smoking is −0.163 (p = 0.504) compared with 0.294 (p = 0.237) without it. Thus, including the data from Tutt *et al*[11] actually strengthens the conclusions in our paper.

It is time for enthusiasts of youth access interventions to recognise that while these interventions may have seemed like a good idea, they do not achieve their primary goal of reducing youth smoking. All that happens is that youth obtain their cigarettes from other sources.[21]

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Both Tutt and DiFranza are missing the larger point of our editorial. Unlike public health forces, the tobacco industry has unlimited resources to push their agenda. We made the point that in a real world of limited public health resources, those resources are better concentrated where they have been shown to be most effective. Youth access is clearly not that area. Tobacco industry documents show that the industry has run rings around public health forces when it comes to youth access, successfully co-opting it to the point that it now serves the industry's purposes.

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Health messages on smoking and breastfeeding in maternity hospitals of Eastern Europe

Smoking, particularly antenatal smoking by the mother, has been consistently shown in many studies to be associated with increased risk for sudden infant death syndrome (SIDS).[1] After the prone sleep position, smoking is the next most important modifiable risk factor for SIDS. Smoking not only undermines the health, development, and survival of the child, but of the mother and other family members, too. A survey of maternity hospitals in Eastern European countries was undertaken in 1999 to collect information on practices associated with increased risk of SIDS. We report here a comparison of smoking and breastfeeding practices of these hospitals.

The collaborative network of the World Health Organization in Eastern Europe (CCEE/NIS) identified country coordinators in 22 Eastern European countries and data were received from 489 hospitals in 20 countries. The study instrument, in either English or Russian, sought information on whether hospitals gave written information to parents and/or had a written policy on various practices including smoking and breastfeeding. Data entry and statistical analysis was undertaken with Epi-info software (Version 6.04c, Centers for Disease Control and Prevention, Atlanta. Georgia, USA). There were more hospitals providing written information to parents about breastfeeding (72%) than about smoking effects (20%). Likewise, there were more hospitals with a written policy on breastfeeding (61%) than on smoking (12%). This difference was consistent across countries.

In contrast to the success of SIDS prevention campaigns advising that babies should not sleep prone, it has been much more difficult to motivate parents not to smoke. UNICEF and WHO have launched the "The Baby Friendly Hospital Initiative" where hospitals are encouraged to adopt 10 evidenced based steps to promote breastfeeding. One of these steps is to have a written hospital breastfeeding policy. Our data may reflect the success of this initiative, in that 72% of maternity units had written information on breastfeeding available for parents and 61% had a written policy. In contrast, our data suggest that only 20% of units had written information available on smoking and only 12% of hospitals had a written policy (table 1). Given that maternal smoking undermines breastfeeding through increased risk of early weaning, reduced milk supply, reduced prolactin concentrations, and low fat concentrations in milk from smoking mothers,[2] a tobacco strategy is likely to enhance breastfeeding outcomes as well as many other health benefits to babies. The "Tobacco Free Initiative" is one of WHO's current priority programmes. Pregnancy and the birth of a child are important intervention points to encourage parents to stop or reduce smoking. The well established and strong association between smoking and SIDS and the evidence of a dose effect of reduced risk with reduced smoking provide encouraging messages to help motivate parents to address their smoking before and after the birth of their infant.

Within maternity hospitals in Eastern Europe breastfeeding promotion messages appear to be more widely available than anti-smoking messages. Smoking prevention strategies should ensure that parents receive written information on the health risks of smoking and hospitals should have written policies. Consideration should be given to including evidenced based strategies to prevent and reduce smoking into an expanded Baby Friendly Hospital Initiative.

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Table 1 Written information given to parents and written hospital policy on smoking and breastfeeding for 489 hospitals in 20 countries in Eastern Europe

		Smoking		Breastfeeding	
Country	n	Info	Policy	Info	Policy
Albania	11	0	0	1	2
Armenia	14	1	0	10	11
Belarus	17	3	3	12	15
BH Sarajevo	7	2	0	7	1
BH Republic Srpska	3	1	1	3	1
Estonia	3	0	0	3	3
Georgia	12	1	0	11	6
Hungary	66	14	12	40	30
Kazakhstan	62	19	14	62	60
Latvia	3	0	0	1	0
Lithuania	11	1	0	9	5
Macedonia, FYR	21	21	3	21	20
Moldova	11	1	1	5	8
Romania	69	22	5	55	23
Russian Fed (Barens)	7	2	1	7	7
Slovak Republic	41	2	1	8	8
Slovenia	14	2	0	13	11
Turkmenistan	57	1	2	50	48
Ukraine	45	7	14	32	35
Uzbekistan	15	0	0	1	6
Total	489	20%	12%	72%	61%

BH, Bosnia Herzegovina; FYR, Former Yugoslav Republic.

Tobacco industry documents: comparing the Minnesota Depository and internet access

I applaud the efforts of Balbach and colleagues[1] to determine systematically what differences, if any, there are likely to be between searches conducted on tobacco industry documents websites and searches conducted at the Minnesota Depository of tobacco documents. However, I think one additional consideration is quite important for documents researchers: the fact that at the Minnesota Depository, it is possible to peruse visually through scroll-down menus the actual list of words or terms by which documents are indexed, using an interface that, to the best of my knowledge, is to date available only at the depository. This enables identification of interesting search terms that might not otherwise occur to a researcher. Both the 4A index terms and the 4B index terms are included. While the indexes themselves may be available for searching elsewhere, the interfaces available do not permit this type of direct visual examination—searching is dependent on already having a search term in mind. Given the industry's well-known use of code names, acronyms, etc. for various projects, I believe that this remains an additional reason why visits to the depository can still be helpful for researchers.

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Filter vent blocking

In their recent article Kozlowski and O'Connor[1] criticise a 1997 review[2] on cigarette filter ventilation blocking and claim it is in error because it (1) relies on saliva based estimates, (2) does not consider degree of ventilation, (3) does not address brand-to-brand variation, and (4) omits certain tobacco industry studies. We disagree and stand by our conclusions.[2 3]

In their criticisms Kozlowski and O'Connor refer only to the 1997 review[2] presented at a conference and not a peer reviewed article published in early 2001.[3] In the latter review, Dr Baker and I considered measurement techniques, effects of vent blocking on machine smoke yields, effects of vent blocking on human smoke yields, and simultaneous determination of vent blocking and smoke yields. We concluded that vent blocking among smokers has only a relatively minor effect on *human* smoke yields compared to other smoking behaviour factors.[3] The large effects observed with smoking *machines* are misleading because people do not smoke like machines.

Concerning the allegation that we erred because of our reliance on saliva based estimates, the facts are that we discussed the pros, cons, and limitations of all techniques used to estimate the extent of vent blocking.[3] We reported that four studies by Kozlowski and colleagues, using the "tar"' stain technique, indicate that 50–59% of the 14 to 158 filters examined in each study showed some degree of vent blocking. Two other studies,[4 5] using the same technique but each based on over 1000 filters, indicate that 21–30% of the filter vents examined were blocked, and most were only partially blocked.[4] These latter studies are in reasonable agreement with large studies conducted by industry scientists using the saliva stain technique,[3] which indicate that up to 24% of filters examined were blocked by lips, and again, most only partially. Direct video observation indicates finger blocking is negligible since most smokers release their fingers from the cigarette as they take a puff,[2 3] but it would be virtually impossible to determine from the video whether smokers' lips had covered the vents.

We devoted a large part of our 2001 review[3] to considering the degree of filter ventilation across a number of cigarette brands (cf. allegations 2 and 3). Reassuringly, some of the latest results from Kozlowski *et al* and industry scientists are in reasonable agreement, despite the very different experimental techniques used.

Kozlowski and O'Connor state that "one notable omission" from the 1997 review[2] is a 1982 study of a 1 mg "tar" cigarette smoked under various puffing conditions[6] (allegation 4). In fact, data from that study are plotted in fig 8 of the 1997 review.[2] We attribute the results to RP Ferris, the project leader, rather than T Hirji, the author of the memo, but it is the same study. They quote the smoke yields from the study[1] but fail to notice that the data are the same as those in our review.[2]

Likewise, Kozlowski and O'Connor say that we ignored pertinent Swiss[7] and Canadian[8] studies, but data summaries are included in our 1997 review.[2] Our 2001 review[3] quotes both studies as indicating a dependence of insertion depth on "tar" yield (that is, degree of ventilation). Kozlowski and O'Connor[1] concentrate on the less detailed unpublished Swiss data but virtually ignore similar trends pointed out in the more comprehensive data published by Baker *et al*.[9] (Kozlowski and O'Connor even re-plot some of the Swiss data to emphasise their point, ignoring the fact that these data were obtained using the saliva stain technique that they criticise elsewhere[1]).

Kozlowski and O'Connor correctly state that we did not mention a 1977 study by Creighton.[10] They quote from this report that "[o]ne subject was seen to cover the ventilation holes with clear adhesive tape". They fail to mention, however, that the "subjects" in this study were R&D scientists evaluating two competitors' filter ventilated cigarettes. Such ad libitum experimentation with the innovative (for 1977) filter design is exactly what one would expect of industry scientists. This experimentation is irrelevant to the behaviour of consumers, and there is nothing more in the report about vent blocking. We considered this report of no relevance to our reviews.

Kozlowski and O'Connor state that we have "ignored the extensive machine smoking studies by Rickert and colleagues on Canadian cigarettes".[11] We cite this study[3] and discuss smoking machine data at length. Rickert *et al* used only one ventilation blocking condition (50%) and the studies we chose to consider used multiple vent blocking conditions.

Finally, Kozlowski and O'Connor also refer to Philip Morris reports not covered in our reviews. In fact, we did not know of their existence until recently. The topics of those memos are comprehensively covered by similar studies in our reviews, and add nothing new.

Kozlowski and O'Connor lament the fact that they cannot find on the internet some industry studies used in our reviews. Not all tobacco companies are obligated to post their internal documents on the internet. Also, as they mention, the internet databases are constantly updated and some documents may not be posted at the time of a given search.

Kozlowski and O'Connor criticise our 1997 review[2] because we did not refer to certain unpublished industry studies. Yet when we sent our updated review for publication in *Psychopharmacology,* the manuscript was rejected on the advice of a reviewer who said it was too dependent on unpublished industry studies (and whose comments read, coincidentally, very much like the Kozlowski and O'Connor article[1]). It is therefore interesting that over 60% of Kozlowski's and O'Connor's references are unpublished industry documents. Many of these are short memos written for internal use, not complete research reports, and assessment by those not involved can lead to misleading conclusions, such as the discrepancy in attribution noted with Ferris and Hirji. It is very difficult to place these documents in proper context, and, in some cases, to try to do so nearly 50 years after they were written.

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Authors' reply

Lewis takes us to task for criticising an article published in 1997[1] by noting that we ignore new points they made in a paper published,

unknown to us, in an industry sponsored journal.[2] We learned of this publication a year after our paper was accepted for publication.

Lewis implies that we had reviewed an earlier submission of their paper to *Psychopharmacology*. We did review this draft, but were not privy to its fate. Journal rules and professional ethics require that the information in their submitted paper be treated as confidential, and we did not mention or make use of any of this confidential draft in our articles. That Lewis and Baker publish a revised paper that was informed by our thinking and suggestions on the topic should hardly be an occasion for criticising our discussion of a work[1] that had not been informed by our advice.

Our paper appeared in a *special* journal issue dealing with available industry documents. Ideally, review articles should derive from published, peer reviewed research. Failing that, public availability (as on the internet) of the primary reports should be expected. But when industry scientists (here from RJ Reynolds and British American Tobacco) characterise internal reports—that may not be or ever become available on the web—the opportunity for independent evaluation of findings may be lacking. Presumably, industry scientists have the ability to bring primary source internal research to peer reviewed publication. For non-industry scientists, in contrast, industry documents on the web are likely all that is available. In other words, *we* are limited to discuss those findings that are open to public view, while *they* are in a position to characterise studies to which independent scientists have no access. It would be best if all studies used to support or refute findings were available to all interested parties, preferably through peer reviewed publication.

Figure 8 in their 1997 paper,[1] which they attribute to Ferris, is related to data that we attribute to Hirji.[3] Compared to the Hirji version, their fig 8 contains both more data (another blocking condition) and at the same time significantly less data (for example, no mention of results from a 75 ml puff in 1 second every 25 seconds, that produces from a nominal 1 mg total particulate matter (TPM) cigarette a TPM yield of 15 mg with no blocking and 23 mg TPM with a 50% block.[1] The Hirji report[3] mentions by name the individuals who did the work, and Ferris is not mentioned.

Lewis writes that Creighton[4] used industry scientists (as was noted in the version we have) who could be expected to conduct "ad lib experimentation" with the then innovative filter design. One of these scientists/ad hoc experimenters dropped out of the study after a day because of "an unpleasant taste in the mouth, persistent irritation and lack of satisfaction" (page 5).[4] Why Creighton did not report that he received *testimony* from his colleagues that abuses were happening, rather than having to "observe" or write that "one subject was seen to cover up the ventilation holes" with tape, is interesting.

Lewis engages us particularly on the issue of vent blocking—a theme we think is less important overall than taste and puff volume, and probably only important for less common heavily ventilated cigarettes. (We never say the saliva based measures of blocking are worthless, just much less sensitive.) In their recent paper,[2] they go into some puff volume data, but for them, interestingly, the blocked vent results (smaller puffs, fewer puffs) are caused by *under-puffing* on blocked cigarettes rather than *over-puffing* on unblocked cigarettes. Their rhetoric encourages us to see a self protecting smoker, rather a compensating smoker. Nice try!

The data in their more recent paper[2] also support the position that filter ventilation is a defective and dangerous design that contributes to the misleading nature of standardised testing of cigarettes.

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SOLUTION to the crossword on page 279

1 C	O	2 N	T	3 R	O	4 L	■	5 L	■	6 E	■	■	■	7 C
I	■	T	■	A	■	A	■	8 A	S	M	A	9 Y	B	E
10 G	R	A	N	D	P	R	I	X	■	P	■	E	■	S
A	■	■	■	I	■	Y	■	11 I	N	H	A	L	E	S
12 R	E	13 A	L	O	R	N	O	T	■	Y	■	L	■	P
S	■	C	■	■	■	X	■	14 Y	E	S	N	O	■	O
■	15 S	T	I	16 F	F	E	N	■	■	E	■	17 W	H	O
18 D	■	A	■	I	■	S	■	19 M	■	M	■	L	■	L
20 E	T	S	■	E	■	■	21 C	A	D	A	V	E	R	■
D	■	22 A	L	L	O	23 W	■	R	■	■	■	E	■	24 F
I	■	L	■	D	■	25 I	L	L	I	26 B	E	R	A	L
27 C	H	A	P	M	A	N	■	B	■	A	■	■	■	O
A	■	M	■	I	■	28 D	O	O	M	S	A	29 Y	E	R
30 T	O	B	A	C	C	O	■	R	■	T	■	E	■	A
E	■	■	■	E	■	W	■	31 O	V	E	R	A	L	L