From: Boyd, Ian (Defra)
To: PMelchett@soilassociation.org;
cc: [redacted] Davies, Arwyn (Defra); [redacted]
Subject: RE: Letter for Professor Ian Boyd neonicotinoids, field and lab studies, peer review and publication
Date: 26 April 2013 14:42:43
Attachments: document2013-04-26-120952.pdf

Dear Peter

Thank you for your further e-mail of 15 April. I am sorry I has taken me a little time to respond but I have been out of the country. I also apologise that this response is not as expansive as the subject demands.

Before responding specifically to your points, I probably need to make some general points.

First, I appreciate your position and that you have a particular point of view. You are quite understandably campaigning to make that point of view clear within the government and regulatory system. My formal role on the other hand is to provide advice based upon a weight of evidence, the uncertainties that lie within the evidence and an assessment of the consequences of various policy options based upon the evidence. I listen carefully to any arguments put to me, and that are in the published literature, but I am always aware of the potential biases that exist as a result of the philosophical direction from which arguments are made. Bias can also exist within the published literature depending upon how parameters have been set for establishing studies, because of the way studies have been designed and, of course, because the science publication process tends only to publish positive results (which curiously is slightly at odds with the principles of the hypothetico-reductive process). This applies across the whole spectrum of problems I am dealing with but is probably particularly difficult in the area of pesticides. There is a general argument, emanating mainly from biomedicine within the US, that the whole of the scientific literature is systematically biased for these reasons. Systematic bias is corrosive and much of the campaigning and moral argument that has infiltrated the field of pesticide management since the DDT issues of the 1960s means that it is enormously difficult – perhaps impossible - to obtain unbiased evidence. This is the kind of lens through which I see almost everything that comes in front of me. *Nullius in verba*.

Second, there are wider, strategic issues to consider. As Mark Walport says in his letter published today on-line by the FT (attached), we have to find a balanced approach to food production. This means we have to take a systems approach that considers how best to balance legitimate concerns across a much wider range of issue than are addressed by focussing upon specific pesticides alone. For example, I am just a concerned about the evidence around the future of food supply and demand and how we manage that. Many in the UK have the luxury of being able to access plentiful, wholesome food which they largely take for granted and yet current evidence suggests that we cannot guarantee this in the long-term.

I am sure you appreciate both these factors. However, you raise a number of specific points.

Dose levels. There is uncertainty about levels of exposure in the field but also in the laboratory studies. Few, if any, of the laboratory studies actually have a reasonable way of measuring dose. On the basis of the available evidence it appears that difference in dose levels will be one of the reasons for differences observed between lab and field studies.

You characterise one of the key honey bee field studies as a secret study funded by a pesticides company. Aside from the description as secret not being the case, the fact that it is a regulatory study provided by industry does not in itself make it invalid. These studies must be conducted to internationally recognised guidelines and have verified Good Laboratory Practice and quality assurance certification. Both negative and positive effects are reported. Academic research is not

subject to these requirements which is one of the reasons it is subject to peer review before publication. The principle that those being regulated should carry the burden of generating the appropriate information needed for regulatory decision-making is widely accepted and employed in regulatory systems throughout the world. Such studies are also examined in detail by regulatory authorities in Member States and by EFSA and I would argue in a lot more detail than a peer reviewer would review a study for an academic journal. Certainly, my experience of sitting on science committees reporting to international treaty organisations is that scientific studies submitted through that process get a grilling that is much more stringent than any applied even in the globally-leading science journals (which I also have experience of, since I am on the Board of Reviewing Editors of *Science*). Peer review for scientific journals is a very blunt instrument and is often not transparent, and it certainly does not 'guarantee quality and integrity of science' as you argue. My view is that the regulatory process constructed to review science outputs from industry has a much greater chance of producing an unbiased result than the free market approach involving academic research and journal peer review.

I also have some observations on the conclusions you draw.

The pesticides risk assessment assesses the risk in realistic conditions and does not assume perfect practice as you suggest. It does however assume and reinforce good practice through conditions on the authorisation, user training and certification and codes of practice.

You also refer to personal attacks on scientists. Actually my reading of situation with EFSA is that this dispute is not about what appeared in the scientific assessment but how that assessment was described in EFSA's press release. This is not a scientific issue and EFSA in their conclusions, quite rightly, make no judgement on acceptability of neonicotinoids, but report the outcome of their risk assessment, using risk assessment guidelines which have in fact not yet been agreed. Nevertheless, I agree completely that this should not be personalised.

Finally, I am not able to agree with most of the statements in your final paragraph. The "severe methodological shortcomings" apply at least as much to the laboratory studies which, as I have said to you in the past, fail to address the important question. Even if the laboratory studies were "good science" from a methodological perspective – and I have explained why they are not – such science is no good if it does not answer the key question. In addition "threats" do not necessarily equal "effects" and I need to see evidence that such effects exist in realistic situations. The balance of evidence suggests they do not. However, as I have indicated before, I would be very keen to have that evidence just so long as it is unbiased.

Kind regards
Ian

From: Peter Melchett [mailto:PMelchett@soilassociation.org]
Sent: 15 April 2013 15:23
To: Boyd, Ian (Defra)
Subject: Letter for Professor Ian Boyd neonicotinoids, field and lab studies, peer review and publication

Dear Ian,

Thank you very much for your email of 3 April, and the careful and thoughtful way in which you responded to my criticisms of Defra's scientific position on neonicotinoids and pollinators. I have taken some time to reply, because I have been giving careful

thought to the arguments you put to me, and obtaining some expert scientific advice. I have also drawn on expertise from PAN UK, Buglife and the RSPB in drafting this letter.

I know this argument has been going on in more public fora with a range of ngos and scientists, but I hope positions are not completely entrenched, and I do take very seriously your comment that you are keen to see evidence that might change Defra's position.

Our correspondence covered two general points. First, the contrasting results between most lab studies and the field studies which you drew to my attention, and which are covered in detail in Defra's report 'An assessment of key evidence about Neonicotinoids and bees' (March 2013). Here, the issue is, as you say, Defra's criticism of the dose levels used in most of the laboratory studies, and the apparently contrasting results from field studies. These (unrealistic does levels in lab studies and contrasting results from field studies) seem to me to be the key issues, and I have addressed them first.

Third, the point I stressed in my original message to you, that Defra are breaking government guidelines on the use of science, by relying on non-peer reviewed work. I'm afraid I still think that is an entirely valid criticism, which makes your evidence base appear more political than scientific.

Finally, I discuss what further research might satisfy Defra's demands for further evidence, and whether this is possible, and touch on how the scientific debate on this issue is being conducted.

Dose levels

On dose levels, I think if we are being straight with each other, we should both acknowledge that information about possible levels of ingestion of neonicotinoids in the farmed environment are uncertain. This will vary by species, and will depend on a wide range of factors, including alternative sources of food for pollinators, weather, competition for food, the length of time over which insects are feeding on a particular crop, and other factors. In an extreme case, where a species of pollinator is particularly attracted to oil seed rape, and where there are several OSR fields near the colony or nest, and where those fields flower at slightly different times because of different drilling dates, and where there are no attractive alternative feed sources, a pollinator could continue to feed on OSR more or less exclusively for a considerable period of time. (One of the many weaknesses in the Defra field trial study is that none of the sites looked at appear to be typical of what you call 'intensive OSR production' – by modern standards they all seem to have relatively small arable fields, and at least at Site B, a good deal of grass and woodland.)

At the other end of the spectrum, an insect could have a wide range of feed sources available, many of which are not treated with neonicotinoids, and where the flowering season for a treated crop could be short. Given how ubiquitous neonicotinoid seed dressings have become, such sites will now be rare in arable farming areas

I mention all this uncertainty, because it seems to me that anyone claiming to know what the likely dose of neonicotinoids ingested by a wide range of pollinating species in a wide range of natural conditions is not being accurate. The fact is that the doses used in laboratory studies need to be within what must be a very wide range of natural variation of possible dose levels.

The scientists I have consulted simply disagree with your statement that the recent laboratory studies have used 'unrealistically high doses, of neonicotinoids'.

I list below a series of papers showing the concentrations found in sunflower, OSR and maize pollen:

- Laurent & Rathahao, 2003 – sunflower pollen concentrations found at 13ppb;
- Scott-Dupree and Spivak 2001 – oilseed rape concentrations found at 4.4ppb to 7.6ppb;
- Bonmatin et al, 2005 - maize pollen concentrations varied between - 38% contained between 0.3ppb and 1ppb, 45% between 1 and 10 ppb and 4% contained a concentration above 10 ppb with a maximum of 18ppb.

Two of the studies you mention as being unrealistic used concentrations of 10ppb (Gill) and 6ppb and 12ppb (Whitehorn). Cresswell used pollen concentrations of 6ppb and sugar water concentrations of 0.7ppb, representing the levels found in foraging on treated oilseed rape. Also, in contradiction to what you said in your message to me, the most recent study (Palmer) uses dose rates of 2.5 – 2.6ppb, which is well within the realistic field doses. It is clear that none of these dose levels are unrealistically high. It is not correct for Defra to say that they are, especially given the real uncertainties about actual doses delivered in the field which I have discussed above.

I think all of us agree that some earlier studies used neonicotinoid doses far higher than the average levels now known to contaminate pollen, nectar, hive materials and food stores. Advances in analytical equipment have now allowed researchers to measure and detect levels of neonicotinoids at 1-10 ppb. As a result, as the detailed figures above show, more recent studies do use realistic doses, which fall within the ppb concentrations found in field sampling. Do you accept this?

Results from field trials

Cresswell (2011) looked at the differences between clear lab results and inconclusive or negative field results, and he concluded, as I am sure you know, that half the field studies did not have enough statistical power to show sub-lethal effects within accepted levels of certainty. Researchers would need much larger sample sizes to be able to conclude even the largest sub-lethal effects as 'significant' from these studies under scientific norms.

In addition, a number of the field trials referenced in the Defra report which show no effect on bees have significant shortcomings. For example:

- the bumblebee study (Thompson H, Harrington P, Wilkins S, Pietravalle S,

Sweet D, et al; 2013) has significant flaws in the way the statistics are (or are not) presented;

- German beekeepers withdrew from the German monitoring project (Genersch E, von der Ohe W, Kaatz H, Schroeder A, Otten C, et al; 2010) as pesticide residues were not even being tested for;
- the Blacquiere study (Blacquiere T, Smagghe G, van Gestel CAM, Mommaerts V; 2012) is a review of other studies and not new research;
- the study in Alsace, France (Hecht-Rost S; 2009) is not published science – it is a secret study funded by a pesticide company;
- the Chauzat study (Chauzat MP, Carpentier P, Martel AC, Bougeard S, Cougoule N, et al; 2009) looks at a number of insecticides, not just neonicotinoids, and there is no data on the levels of pesticide use on the surrounding land.

The FERA study suffers from limitations in study design, as you acknowledge, but even so there are some potentially worrying patterns in the data. These include lower brood mass increase and colony fitness at one NN-treated site (C), fewer worker bees at another NN-treated site (B), many more nectar cells at the control site (A), and apparently negative correlations between colony performance and levels of thiamethoxam in pollen.

Peer reviewed published science – the gold standard

Turning to the question of whether the Defra study should be relied on by Minsters, given that it has not been peer-reviewed and published in the scientific literature, I am afraid I find your arguments unconvincing. As you know, the point of peer-review and publication is that it is an independent process, there to guarantee the quality and integrity of science. The idea that government departments, or indeed anyone else, can invent and adopt what you describe as 'the same approach' would drive a coach and horses through a key element of scientific integrity. That is exactly why previous government Chief Scientists have insisted on independent peer review and publication as the gold standard for the scientific evidence on which Ministers rely. In this case, one unpublished study is in fact being referred to by Ministers and others in public statements as being especially significant because it has just been completed.

I am afraid it is wrong to suggest that the criticisms from the reviewers of this paper were responded to satisfactorily. When the initial paper was submitted to the Advisory Committee on Pesticides, a number of expert scientists were present. For example, Dave Goulson said then that the 're-analysis of the findings considering individual colonies rather than a site by site comparison would assist interpretation' and he has stated publically since then that the way the new analysis has been undertaken does not meet his original criticism.

The difficulties with field studies

I don't want to get distracted from the key point about dose levels and the relative reliability of laboratory and field studies, by other issues raised by the Defra study, but I do think there are other valid criticisms, for example over the impact of removing outliers (doing so without reason is just bad science, and it does change

the conclusions from not being able to rule out an effect to 'there being no significant statistical effect'). Fundamentally, it seems to me that what that study shows is the extreme difficulty of conducting scientific research in the farmed environment in a way which is likely to produce results which could helpfully inform Ministers when they come to make political decisions about whether neonicotinoids should be suspended, as European scientists have suggested they should.

The fact is that the farmed environment where bees and other pollinators are likely to forage on neonicotinoid-treated crops are now so universally contaminated with neonicotinoids that no effective control sites which represent good proxies for typical arable areas using neonicotinoids are available. Do you think it would be practicable to ban neonicotinoid use over large areas for several years to provide adequate control sites, bearing in mind honey bees forage for up to six miles, and that neonicotinoids appear to persist in the nevironment? In addition, the large number of variables present in the natural environment, including the weather, mean that to gain any real statistical power, very extensive studies would need to be done, certainly over a period of three to five years, to give any clear results, and even that assumes that it really would be possible to control for all the other disruptive factors. (Defra recognised this in setting up the Farm Scale Evaluations of GM crops back in 1999 – they insisted on a five year research programme, involving multiple sites, with absolutely clear, non-GM controls.) Even if such a project were to be undertaken (and no one in Defra has suggested this), I think you would agree, such a large-scale, long-term study would only be able to look at a small number of species unless huge resources were put into it, so you and I would still be left theorising about the impact on all the wild pollinators that we have not yet studied at all.

If this is the additional evidence that you are advising the UK Government is needed, I think the least you could do is be clear to the public about the timescale and cost of this further research, and explain how adequate controls could be organised.

Conclusion

As you know, no one contests the fact that neonicotinoid dust from treated seeds directly kills bees, and that treated seeds kill birds. Anyone who suggests that drilling crops on farms is a precise operation, with no spills, no poorly covered seeds, and no possibility of dust, is not being accurate.

Ian – I have to tell you that having talked extensively to many of the key scientists involved in the arguments about DDT and their effects on birds of prey in the 1960s, that there is an uncanny similarity in the position being adopted both by the chemical companies and the government over neonicotinoids now, and the arguments adopted in favour of DDT back then. I was going to say that I am glad that there has not been the same vilification of the scientists presenting evidence against neonicotinoids as there was against those critical of DDT in the 1960s. However, this week, copies of the correspondence from Syngenta and Bayer, to the EFSA, the Commission and others, have been released. So now we do also have the personal attacks and threats that occurred in the 1960s being repeated. For example, Syngenta have targeted the EFSA's Director, accusing her of not including Syngenta's comments on the draft press release saying: 'you took the personal responsibility to overrule the internal EFSA proposal to rectify the incorrect press release', and that

'Syngenta would appreciate further explanations from you' before 'deciding on the legal options available to it and the identity of specific defendants in any possible court action'. Will the Government condemn such threats to scientists involved in this debate?

As I say, the arguments themselves could be a re-run of the 1960s. Yes, it is hard to gather data in the environment, as you know only too well. Yes, it is hard to show low-level, indirect or long term impacts on wildlife populations, as again you will be all too well aware. Yes, at the end of the day, holes can always be picked in the science which suggests such impacts. But I have to say to you that if the scientific evidence is sufficient to convince the EFSA, which has taken a consistently sceptical and often anti-environmental position on a range of issues since it was set up, it really is very unlikely that the case is anything like as weak as you have been suggesting. Indeed when this issue was originally referred the EFSA the pesticides lobby group ECPA wrote to the Commission that 'as an industry, we welcome the fact that EFSA is carrying out a detailed evaluation on the use of these seed treatments'.

In the 1960s, as now, as the evidence against DDT built up, the response from industry and governments was to call for 'more research'. In the case of neonicotinoids, calling for further research is unrealistic – research in the farmed environment would be impossible unless neonicotinoids are restricted over large areas to act as controls, would certainly take many years, and would be extremely expensive.

I do hope you will now agree that, relying on published science, the evidence it is clear that the recent lab studies showing adverse impacts of neonicotinoids are good science, that the field studies generally suffer from severe methodological shortcomings, and that neonicotinoids do pose real threats to farmland wildlife. Whether the UK Government or other Member States choose to act on this evidence is a political question of risk management, but the science of the risk assessment is clear.

Best wishes,

Peter

Peter Melchett
Policy Director

From: Boyd, Ian (Defra) [mailto:Ian.Boyd@defra.gsi.gov.uk]
Sent: 03 April 2013 10:07
To: Peter Melchett; [redacted]
Subject: RE: Letter for Professor Ian Boyd on peer review and publication

Dear Peter

Thank you for your email.

The study you refer to is not used as the sole basis of decisions made by Defra. The weight of evidence in the case of bees and neonicotinoids is finely balanced. However Defra has used a weight of evidence approach to reach its current position (see attached). The version of this document does not yet include the new study you referred to but it will be updated accordingly in due course. Defra is entirely open to changing this assessment as new evidence emerges.

Regarding the "Defra field trials" study, this has been peer reviewed using the same approach as would be applied if it was submitted to a scientific journal, i.e. independent, anonymous and authoritative using, in this case, eminent individuals in the field. Criticisms from the reviewers were responded to satisfactorily by the authors. It would have been preferable to allow this paper to have gone through a journal publication process but, since the paper had been requested by the ACP, it would undoubtedly have been released under FOI and in any case it contains information relevant to the evidence assessment and would also have been releasable for the same reason.

The study itself has strengths and weaknesses. On the side of its strengths, the colonies survived, grew and reproduced apparently normally within three landscapes of intensive OSR production. I'd be happy to see evidence to the contrary on this issue. On the side of its weaknesses there is a question about the statistical power to show effects. The apparent effect you have pointed to is weak, statistically speaking, and with such a weak effect it is entirely right that the authors test it for robustness and provide the details of that test. In addition to their doubts, I consider that the relationship seen is further weakened because of the requirement for a lower fudicial limit when multiple tests are used. Overall, in my view it would not be safe to conclude a significant effect from this analysis of these data.

Some of the issues you raise about difference between sites were dealt with statistically within the study. One of my criticisms of the study is that the authors did not do enough to account for these differences, but I have to point out that if they had then it is more likely that these difference would have reduced, rather than increased, any differences between the sites and it would most likely have led to the slowest-growing site (Site C) becoming more aligned with the other two sites. Nevertheless the kind of modelling that would have been required to compensate for the difference in starting conditions would have been complex and I can understand why the authors, on balance, did not think it wise to go down that path.

However, as the evidence assessment shows, this study is simply one of several that have trouble showing an effect of neonicotinoids and these need to be balanced against those that do show effects. There is quite a lot of evidence that those studies showing effects have used unrealistically high doses. Again, I would be very keen to see the evidence showing that this is not the case – one of the studies (Gill) actually states that it used a high dose, another (Henry) uses a totally unrealistic dosing regimen, and the other (Whitehorn) does not measure dose at all and uses a feeding regime that is likely to have overdosed. The most recent study (Palmer et al in *Nature Communications*) sadly suffers from the same problems as other lab-based, or dosing, studies. It is self-evident that neurotoxic insecticides kill insects; the problem I have to deal with is whether the insects in question are exposed to levels in the field that are sufficient to cause reductions in performance at the level of populations. None of the evidence to date has fully addressed this question although, as I say, I am keen to see this evidence. The lower statistical power of the field studies, relative to that of the lab- or semi-lab based dosing studies, has to be tensioned against the greater relevance of the field studies to the central question that needs to be answered.

Regarding your question about the data presented in the study conducted by Fera, I am sure they would be happy to send you the data if requested to do so.

Kind regards
Ian

From: Peter Melchett [mailto:PMelchett@soilassociation.org]
Sent: 02 April 2013 18:16

To: Boyd, Ian (Defra); [redacted]
Subject: Letter for Professor Ian Boyd on peer review and publication

Dear Ian,

I am looking forward to resuming our discussions about wider issues, and thank you very much for suggesting this. I am writing now about a more specific issue, namely the strong government science policy on the importance of peer-reviewed science. As I understand it, successive Government Chief Scientists have reaffirmed the importance of peer review and publication, and have repeatedly reaffirmed that unpublished research should not be referred to as a basis for the scientific evidence on which government policy should be based. I apologise for bothering you with this, but as very significant decisions are being taken later this month, I hope it is OK to do so.

Last week, Defra Farm Minister David Heath MP announced that Defra field trials had not conclusively found evidence of a link between neonicotinoid use and bee deaths. However these studies have not been subject to peer review, nor of course published in the scientific literature, so it seems quite wrong for a Defra Minister apparently to base conclusions about the impact (or lack of evidence for impact) of neonicotinoid pesticides on bees using such unpublished material.

This is particularly the case as these studies have already been subject to criticism by other experts in this field, and it seems likely in view of these comments that in fact these Defra studies would not meet the basic requirements for publication in the peer reviewed literature. Crucially, the studies appear to have been unable to use any honey bee colonies as controls for the bee colonies feeding on neonicotinoid treated crops, as the scientists found contamination with neonicotinoids even at the sites that were not treated with neonicotinoids. The absence of controls does, of course, render any claimed results about the impact of neonicotinoids meaningless.

A number of other significant problems with the research have been highlighted which no doubt would have been discussed and if possible resolved during peer review. For example, there were several variables between the three sites used in these studies, including different colony sizes, possible different climatic conditions, different alternative feed sources for the bees, different pesticide use on surrounding crops and different timings of exposure to neonicotinoids. All these variables, and there may be others, suggest that comparisons between sites, or accumulation of data from different sites, is simply not possible, and no meaningful conclusions can be drawn from the data gathered. So despite the fact that the studies include many pages of analyses in the appendix, in which the sites are simply compared, this has no value since the sites differ in so many ways. All that this data can tell Ministers is that bee colonies grow differently if you put them in different places.

For reasons which I do not understand, the crucial tests, which cover neonicotinoid levels per nest as covariates, are not in the appendix, nor anywhere else in any detail in the published paper. In the paper, the researchers do say that they found a highly significant negative effect of thiamethoxam on colony growth and queen production, but they then state that they “removed outliers”, repeated the analysis, and found no effect. I am unclear what possible justification there could be in these circumstances to ‘remove outliers’, which seem to me to be the colonies which received the biggest dose of neonicotinoids, and which were therefore the ones that performed worst. To

be frank, this seems to be an completely unacceptable approach to analysing the data, and something that would never have made it through any sort of professional peer review for even a low impact journal.

My point is that for Defra or the UK Government to base any policy or action on these studies, whether they are deeply flawed or not, would be to ignore basic scientific principles. I am particularly concerned by what seems to be a determination by Ministers to do this at any cost to scientific integrity, because last week another research study was published, this time in the peer reviewed literature, which found that exposure to a combination of pesticides (including neonicotinoids) negatively affects the brains of bees. The study is the first to show that 'field relevant' levels of these pesticides have a direct impact on pollinator brain physiology. In that study, the researchers used levels of pesticides that bees experience in the wild and discovered that information flow in bee's brains was affected. They also found that bees exposed to combined pesticides were slower to learn or completely forgot important associations between floral scent and food rewards. But crucially for the purposes of this letter, that study was peer reviewed and published, so it is science that Ministers can legitimately refer to, while according to Government scientific policy, the unpublished Defra studies are not. I hope you can confirm this.

Best wishes,

Peter

Peter Melchett
Policy Director
Department for Environment, Food and Rural Affairs (Defra)
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