Guidelines for evaluating the performance of road mitigation measures

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Table of contents

Executive summary

National road administrations increasingly make use of Design & Construct contracts in road building. In these types of contracts the constructor not only builds but also designs the desired road or road modification. Although such contracts are not yet widely used to construct mitigation measures for wildlife, some road administrations are experimenting with these procurement approaches and they are attracting increasing interest. This implies that procurement documents no longer present detailed prescriptions on the technical design and dimensions of road mitigation measures, such as wildlife crossing structures or wildlife fences, but provide descriptions on what the measures should achieve, i.e., what the desired outcome should be. Hence, there has been a shift in procurement from detailed design specifications to outcome-based specifications describing the intended functions. It is the task of the contractor to translate these outcome-based specifications into technical solutions and to prove that the solutions are functional. With such a transition from design specifications to outcome-based specifications for road mitigation measures, new methods are needed to assess whether the intended outcome has indeed been achieved.

The objective of this study is to develop a framework to evaluate road mitigation performance, i.e., guidelines to assess whether such measures function in line with the outcome-based specifications provided. We do not intend these guidelines as a ‘cookbook’ for all mitigation evaluations as decisions on, for example, study design, sampling scheme or survey methods highly depend on the mitigation goals, target species, local situation, etc. The guidelines should be regarded as a checklist that helps to address all relevant issues in preparing a scientifically sound plan to evaluate whether or not the desired outcome for road mitigation has been achieved. We illustrate the use of the guidelines with two examples: evaluating road mitigation performance for amphibians and for large mammals. Furthermore, we provide a few recommendations for road administrations on how to implement the use of the guidelines presented here to evaluate road mitigation performance. The elaboration of a plan to evaluate mitigation performance may, in some cases, require additional research as current knowledge may not be sufficient to make well-informed choices. To illustrate this need and simultaneously contribute to better decisions in preparing an evaluation plan, we present three case studies in which specific questions are addressed that relate to study design, sampling scheme and survey methods respectively. These case studies are based on analyses of existing datasets of road mitigation studies carried out in the Netherlands.

We identified a set of ten guidelines for evaluating road mitigation performance:
1. Select performance indicators that are most closely related to the desired outcome;
2. Select a study design that incorporates the assessment of reference values;
3. Select a study design that incorporates data collection at control sites;
4. Select survey methods that are the most accurate, informative and efficient;
5. Select an appropriate spatial scale for data collection;
6. Time data collection on the basis of the mitigation goals, life cycle of the target species and moment an effect is expected;
7. Base study duration on the expected sampling time needed for adequate statistical power;
8. Use a sampling frequency that allows for rigorous estimates of the performance indicator;
9. Measure explanatory variables that may affect mitigation performance;
10. Make the evaluation report and raw data widely available.

The use of outcome-based specifications and a corresponding framework for evaluation will put much more emphasis on the goals for road mitigation and the extent to which these goals have been achieved. To do this, a strong set of performance indicators and a transparent

evaluation method are needed. Our recommendations for implementing the use of the guidelines presented here for evaluating road mitigation performance are:

- involve ecologists in the procurement process of road mitigation works by having them prepare outcome-based specifications, organise the collection of baseline information and judge evaluation plans proposed by contractors;
- contract an independent contractor to evaluate road mitigation performance;
- form an independent advisory board, consisting of experienced road ecologists, to assist the road agency in reviewing outcome-based specifications as well as advising on performance evaluations that meet good science;
- make the preparation of a plan to evaluate planned road mitigation measures an inseparable part of the legal processes that must be followed during the road planning and procurement stages;
- develop a strategy for systematic assessments of baseline conditions and reference standards;
- secure all necessary resources to evaluate road mitigation performance beforehand in close collaboration with other stakeholders, such as environmental agencies;
- make the outcome of all evaluations, including research reports and raw data, available to all stakeholders through an open access database;
- test whether this set of guidelines to evaluate road mitigation performance is practical and effective.

The three case studies illustrate the need for more understanding of our research methods as current knowledge may not be sufficient to make well-informed choices in preparing an evaluation plan for road mitigation works. The first case study shows that we should carefully select our study design and the spatial scale for data collection. Without controls it will be difficult to attribute a measured effect to the mitigation works. And certain positive effects at a mitigation site may be nullified by negative effects in adjacent areas, for example as a result of fence end effects. The second case study emphasises that all assumptions on the reliability of certain research approaches should be tested. It shows that data on crossing structure use collected in only a few months of the year - as now seems to be the practice in some countries - does not necessarily result in representative outcomes for the full year. The third case illustrates that different survey techniques may produce significantly different results and that rigorous comparing and testing of techniques is needed previous to the start of any evaluation.

Evaluating mitigation works based on the guidelines presented here will require more efforts and resources than most current approaches. On the other hand, such an evaluation will provide much more feedback on what we do right and wrong and is strongly linked to the reasons of why the mitigation was designed, i.e., the mitigation goals. If these goals, in turn, are strongly linked to (inter)national legal and policy plans, the outcome of the evaluations will help to improve mitigation works and effectively contribute to overall biodiversity conservation.

1 Introduction

National road administrations (NRA) increasingly make use of Design & Construct (D&C) contracts in road building.[1] In these types of contracts the constructor not only builds but also designs the desired road or road modification. Although such contracts are yet not widely used to construct wildlife mitigation measures, some NRA (e.g. in the Netherlands) are experimenting with these procurement approaches and they are attracting increasing interest (Van der Grift & Seiler, 2016). This implies that procurement documents no longer present detailed prescriptions on the technical design and dimensions of road mitigation measures, e.g. wildlife crossing structures or wildlife fences, but provide descriptions on what the measures should achieve, i.e., what the desired outcome should be. Hence, procurement is shifting from detailed design specifications - with the focus on input - to more general functional specifications - with the focus on output. It is the task of the contractor to translate these functional or outcome-based specifications (OBS)[2] into technical solutions and - usually - to prove that the solutions are functional.

With a transition from design specifications to outcome-based specifications for road mitigation measures, new methods are needed to assess whether the intended outcome has indeed been achieved (see also Van der Grift & Seiler, 2016). Evaluations should no longer focus on the question of whether a requested number of road mitigation measures of type A or B have been properly installed, but have to answer the question of whether they facilitate pre-described functions. One of the advantages of a shift towards outcome-based specifications is that they will assumingly deliver more value within constrained budgets. Many current methods for inspection and evaluation, however, will no longer be sufficient. In general, methods should shift from merely technical evaluations - e.g. *'is measure X properly installed in accordance with the design specifications'* - towards evaluations that assess whether the goals for road mitigation, described as functions, have or have not been achieved.

The objective of this study is to develop a framework to evaluate road mitigation performance, i.e., guidelines to assess whether such measures function in line with the outcome-based specifications provided. This study can be seen as a follow-up to the study by Van der Grift and Seiler (2016), also part of the SAFEROAD project, in which guidelines were developed for defining outcome-based specifications that can guide civil engineers to produce functional road mitigation measures. Hence, in this study we base our recommended framework for evaluation, including performance indicators, on the set of guidelines for defining outcome-based specifications presented in the previous study.

To develop guidelines to evaluate road mitigation performance, we used the state-of-the-art knowledge on conducting scientifically sound evaluations, including recent publications on how to evaluate road mitigation functioning and effectiveness. As mentioned above, our point of departure was the recommended set of guidelines for defining outcome-based specifications (see Van der Grift & Seiler, 2016). We illustrate the use of the guidelines by two examples, which were also used to illustrate the use of the previous guidelines to define outcome-based specifications: road mitigation for amphibians (example 1) and for moose (example 2). The guidelines for road mitigation evaluations have been elaborated on a generalised level. They do not form a 'cookbook' for each situation as decisions on, for example, study design, sampling scheme or survey methods highly depend on the mitigation

[1] Or even the more advanced contract types Design, Build, Maintain (DBM) and Design, Build, Finance, Maintain (DBFM).
[2] Also referred to as performance-based specifications (PBS; see also Turley et al., 2014).

goals, target species, local situation, etc. Hence, the guidelines should be seen as a checklist that helps to address all relevant issues in preparing a scientifically sound plan to evaluate whether or not the desired outcome for road mitigation has been achieved. In some cases, the elaboration of such a plan may require additional research as current knowledge may not be sufficient to make well-informed choices. To illustrate this need and simultaneously contribute to better decisions in preparing an evaluation plan, we present three case studies in which specific questions are addressed that relate to study design, sampling scheme and survey methods respectively. These case studies are based on analyses of existing datasets of road mitigation studies carried out in the Netherlands. Finally, we provide a few recommendations for road administrations on how to implement the use of the guidelines presented here for evaluating road mitigation performance.

2 Guidelines for evaluating road mitigation performance

2.1 Introduction

A key challenge in the use of an outcome-based approach in procuring road mitigation is the development of both a clear framework for evaluation and measurable performance indicators that are tied to the required outcomes. Only then is it possible to judge whether contractual obligations have been met by the contractor. As outcome-based specifications refer to the desired ecological functions rather than to design metrics, the former frameworks and indicators no longer apply.

The point of departure for any evaluation plan in procuring road mitigation should be the outcome-based specifications provided. As recommended by Van der Grift & Seiler (2016), such specifications should link directly to the mitigation goals, including a description of the target species, and answer to the SMART approach, i.e., they should be specific, measurable, achievable, realistic and have a clear time frame. Hence, if worked out well, the outcome-based specifications indicate what road impacts need to be addressed and what needs to be achieved, include clear thresholds or traits of success for each road impact that needs to be addressed based on baseline conditions or reference standards and provide a clear time frame for both the availability of the mitigation works and the time period over which the performance should be assessed to decide whether the specifications are being met.

However, which performance indicators should be selected? What would be the best study design to assess whether the desired outcome has been achieved? What sampling scheme should be used or which survey techniques should be applied? How can we be assured that the measured outcome is not biased by factors that do not directly relate to the road mitigation works? In this chapter we provide guidelines that help evaluate road mitigation performance. These guidelines are rooted in existing guidelines to evaluate road mitigation performance and effectiveness (see Van der Grift et al., 2013; Van der Grift et al., 2015; Van der Grift & Van der Ree, 2015), but have been adapted for use in an outcome-based procurement approach. The guidelines do not provide a 'recipe' for each situation, but should rather be seen as a checklist that helps to elaborate a plan or strategy to evaluate whether or not the desired outcomes have been achieved.

2.2 Guidelines

We have identified a set of ten guidelines for evaluating road mitigation performance:

1. Select performance indicators that are most closely related to the desired outcome
In most cases performance indicators can be directly derived from the outcome-based specifications. For example, if *"a reduction in roadkill of [target species X] by 90%"* is the requirement, roadkill numbers is obviously the performance indicator to use. If *"at least 90% of the between-population movements of [target species X] should be restored"* is the requirement, movement numbers should be the performance indicator. However, in some cases multiple performance indicators may suit and a choice has to be made; for example, when the specifications refer to maintaining or restoring population viability. Since population viability cannot be directly measured in the field, we either need to measure proxies that are linked to viability and influence the likelihood of population persistence. In this respect Van der Grift et al. (2013) present eight potential indicators. The most informative indicator in this respect is the trend over time in the size – or density – of the population. For example, if

existing roads have population-level effects and crossing structures are successful in mitigating those effects, we would expect to see an increase in population size after the structures have been installed. When it is not possible to estimate population size or trend, less indicative attributes may be measured, such as the number of road-kill, reproductive success, age structure, sex-ratio, between-population movements, genetic differentiation, or genetic variability within the population. However, conclusions about road mitigation performance will be harder to make the less closely these attributes are tied to population viability.

2. Select a study design that incorporates the assessment of reference values
The study design should include the collection of data on reference values, such as baseline conditions or reference standards (see also Chapter 3). Baseline conditions refer to the local conditions before mitigation. Reference standards may refer to, for example, the conditions at reference sites, standards generated by model simulations, or standards that have been derived from regulations or policies. Such reference values are essential to assess the extent to which road impacts have been mitigated and hence the extent to which the mitigation goals are achieved. Collecting data before and after installing mitigation measures allows the ‘before’ situation to be used as a reference. For example, if a reduction in roadkill numbers - expressed in a percentage - is the aim, it should be known what the average roadkill numbers were in the years prior to the mitigation in order to assess the threshold. Collecting data simultaneously at the mitigation site and a reference site allows the ‘reference’ situation to be used as a standard. For example, if restoring animal movements is the aim but ‘before’ data is lacking, animal movements at an undisturbed site - without a road but with similar population densities - may serve as a reference standard.

3. Select a study design that incorporates data collection at control sites
The optimal study design for evaluating road mitigation performance includes the collection of data before and after road construction (see previous guideline), at the road sites where mitigation is installed (mitigation sites) and at road sites without mitigation (control sites). We refer to such a study design as a Before-After-Control-Impact (i.e. BACI) design. Collecting data at control sites ensures that measured changes can be attributed to the mitigation (see also Chapter 3). For example, mitigation measures - such as wildlife fences - may reduce road-kill, but an observed reduction in road-kill could also be caused by other factors, such as a decrease in population density or an increase in traffic volume (Seiler et al., 2016). In practice, it may be hard to find suitable control sites. Potential control sites must be similar to the mitigation sites with respect to the properties and characteristics that will be controlled. If no suitable controls can be found, the study design will be limited to a Before-After (i.e. BA) design. Although a BA design allows for less rigorous conclusions than a BACI design (see also Roedenbeck et al., 2007), it may still provide valuable information on road mitigation performance. This is especially true when, besides the performance indicator(s), explanatory variables are measured (see guideline 9). For example, if (changes in) population densities are measured and populations seem to be stable, a decrease in population density can be excluded as a possible explanation of a measured reduction in road-kill.

4. Select survey methods that are most accurate, informative and efficient
The survey method depends on the selected performance indicator and target species (Table 2.1 and 2.2). If more than one survey method is available, the most accurate, informative and efficient one should be selected (see also Chapter 5). For example, if compared with track beds, camera traps usually result in better estimates of large mammal crossing rates at crossing structures; they provide not only data on species, direction of movement and gait, but also on the time of crossing, weather conditions and, depending on the species, sex, age class, behaviour and unique markings. Furthermore, camera traps may be more efficient as they require fewer man-hours and operate 24 hours a day, seven days a

week (see also Ford et al., 2009). If multiple target species are surveyed, survey methods that monitor multiple species simultaneously are recommended because they provide more information at similar effort and cost. Consistent use of the same methods and personnel over time is important to decrease bias and provide comparable results.

Table 2.1. Potential survey method(s) for each performance indicator, categorised over the three main drivers of road mitigation: human safety, animal welfare and wildlife conservation. The list provides some examples of frequently used survey methods and is not intended to be complete. Source: Adjusted from Van der Grift et al. (2013).

Performance indicator	Potential survey methods
Human safety	
Number of humans killed or injured due to WVC or to collision avoidance	Accident statistics, police reports, questionnaire
Insurance money spent on material/immaterial damage due to WVC	Insurance statistics, questionnaire
Number of hospitalisations due to WVC	Accident statistics, police reports, questionnaire
Number of WVC with species that potentially impact human safety, regardless of whether they resulted in human injury or death	Road surveys, police reports, hunter reports
Animal welfare	
Number of animals killed or injured while crossing roads	Road surveys, police reports, hunter reports, public wildlife reporting systems
Number of animals killed or in ill-health due to isolation from needed resources through the barrier effect of roads	Field surveys, biological sampling through e.g., hunting or life-capture
Wildlife conservation	
Trend in population size / density	Capture-mark-recapture, point/transect counts or calling surveys, pellet counts, nest/den counts, tracking arrays, e.g. photo/video cameras, track pads
Number of animals killed	Road surveys, radio tracking, road-kill statistics
Reproductive success	Counts of eggs / young / nests, etc.
Age structure	Capture, direct observation
Sex ratio	Capture, direct observation
Between-population movements	Capture-mark-recapture, radio-tracking, direct observation, tracking arrays
Genetic differentiation	Invasive DNA sampling after capture, non-invasive DNA sampling, e.g. through hair traps, scat collection, antler/skin collection
Genetic variability	Invasive DNA sampling after capture, non-invasive DNA sampling

Table 2.2. The suitability of commonly used survey methods for each species group. ** = highly suitable; * = suitable; 0 = registration of species group, but not able to identify species; - = not suitable; ? = unknown. Source: Adjusted from Van der Grift & Van der Ree (2015).

Survey method	**Species group**										
	Large mammals	Medium-sized mammals	Small mammals	Bats	Non-flying birds	Flying birds	Reptiles	Amphibians	Non-flying insects	Flying insects	Other invertebrates
Track bed (coarse sands)	**	**	0/-[1]	-	**	-	0/-[1]	-	-	-	-
Track bed (fine sands)	**	**	0	-	**	-	0	0	-	-	-
Track plate	-	**	0	-	-	-	0	0	-	-	-
Snow tracking	*	*	-	-	-	-	-	-	-	-	-
Photo/Video camera	**	**	*/-[2]	-	**	?	?	*[3]	-	-	-
Infrared trail monitor	0	0	0	0	0	-	-	-	-	-	-
Artificial shelters	-	-	*	-	-	-	**	**	*	-	*
Bat detector	-	-	-	**	-	-	-	-	**	**	-
Survey of animals by direct observations (sight or acoustics)	-	-	-	*	-	**	**	**	**	**	-
Survey of animal signs (e.g. browsing, droppings, nests)	*	*	*	-	*	*	-	-	*	*	-
Hair trap - Hair identification	*	*	*	-	-	-	-	-	-	-	-
Hair trap - DNA analysis	*	*	*	-	-	-	-	-	-	-	-
Capture-mark-recapture	-	*/-[2]	**	-	-	-	**	**	**	**	*
Capture-mark-monitor (e.g. PIT tag, ear tag)	*	*	*	-	*	-	*	*	*/-[2]	*/-[2]	-
Capture-tracking (e.g. radio tracking, GPS/satellite tracking)	*	*	-	*	*	-	*/-[2]	*/-[2]	-	-	-
Capture-release (e.g. live-trap, pitfall trap, mist net)	-	-	**	**	*	-	*	**	**	**	**
Capture-kill (e.g. pitfall trap, light trap, photo-eclector)	-	-	-	-	-	-	-	-	**	*	*

[1] Registration, but not at species level, for only some species within this species group.
[2] Suitable for only some species within this species group.
[3] If used in small wildlife underpasses.

5. Select an appropriate spatial scale for data collection

The spatial scale for data collection should match the spatial scale of the road effect being mitigated and of the chosen performance indicators linked to the species of concern and the local situation. For example, wildlife-vehicle collisions need to be sampled on and near the road in question, but also on intersecting roads and on road sections adjacent to the targeted section that might be indirectly affected by the mitigation (see also Chapter 3). Barrier effects on populations usually need to be sampled over a larger area surrounding the road. Similarly, data collection for species with small home ranges and limited dispersal capacity can be done on a smaller spatial scale than for species with large home ranges and high dispersal capacities. The spatial scale of a study is also closely related to the local situation as, for example, topography, land use and available habitat types influence the delineation of the study area. Consequently, a proper spatial scale can only be selected if baseline information is available or appropriate assessments can be made on the extent to which road effects may affect the chosen indicators for each target species. For example, if changes in between-population movements are studied, knowledge on mean individual dispersal distances may be helpful as they well indicate the effect-distance as individuals that live farther from the road than the dispersal distance will less likely reach the road corridor and be affected by the mitigation measures. In case demographic or genetic features of the population are used as a performance indicator, data collection should take place over a large enough area that covers the full area of the (local) population.

6. Time data collection on the basis of the mitigation goals, lifecycle of the target species and moment an effect is expected

The timing of data collection should be based primarily on the mitigation goals. For example, if the aim is to restore access to seasonal habitats (e.g. breeding ponds, wintering habitat), the sampling can be limited to the period in which those migrations - to and from these habitats - occur. The life cycle of the target species may affect the timing of sampling if predictable periods of presence/absence (e.g. migratory species) or inactivity (e.g. hibernation) can be identified. Data collection should preferably take place for the full period in which the performance indicator is relevant. An alternative approach may be to sample only for a part of that period, for example, if resources are limited. However, most wildlife species show different activity and movement patterns throughout the year; hence, a shorter sampling period may result in less rigorous evaluations. Values for performance indicators may be easily over- or underestimated, or the target species may be missed all together (see also Chapter 4). Data collection after the mitigation structures have been installed should not begin before an effect of the mitigation is expected to have occurred. In most cases this may be shortly after installation, e.g., wildlife fences should mitigate road-kill as soon as they are erected. In some cases, however, it may take time for the mitigation works to function as desired. For example, animals that need cover will use wildlife overpasses only after the vegetation on and around the structure has matured. Other species may be less dependent on vegetation development but still need time to habituate to a crossing structure. It may also be true that the selected performance indicator makes data collection immediately after installation needless. For example, when genetic indicators are used to evaluate mitigation performance.

7. Base study duration on the expected sampling time needed for adequate statistical power

The duration of data collection should allow for sufficient statistical power to determine whether or not the mitigation results in a significant change in the performance indicator of concern. Consequently, study duration is closely related to the chosen performance indicator and the characteristics of the studied species. For example, a species with a low reproductive rate and long lifespan would require monitoring over a longer period to assess a change in population density compared to a species with a high reproductive rate and short lifespan. Study duration also relates to the number of data points that are expected to be collected in each year or sample. For example, if in the pre-mitigation situation road-kill numbers are low, it will take more years after mitigation to assess whether a specific aimed-for percentage of road-kill reduction has been reached. However, even if yearly data sets are relatively large it may be advisable to collect data for multiple years, as some performance indicators may vary considerably across years. The need for data collection across multiple years also increases if one or more explanatory variables are known to vary considerably from year to year, e.g., population size of the target species. We recommend conducting power analyses to determine appropriate study duration for each of the performance indicators of interest, based on the aimed for effect size and desired power.

8. Use a sampling frequency that allows rigorous estimates of the performance indicator

The frequency of sampling should allow rigorous estimates of the performance indicator. For example, in most cases surveying road-kill just once a month will not be sufficient to calculate rigorous estimates of mean road-kill a year. And estimates on between-population movements will likely be more accurate if, for example, track beds are sampled daily instead of once a week (see also Van der Grift et al., 2015b). Pilot studies may be needed to assess the optimal sampling frequency in which sampling effort is minimised without jeopardising accuracy.

9. Measure explanatory variables that may affect mitigation performance

Variables other than the performance indicators of interest should also be measured to control for changed parameter settings and improve interpretation of the results. Especially if data collection at control sites is lacking, measuring explanatory variables will allow stronger inferences concerning the causes of observed differences. For example, if reductions in road-kill through the installation of wildlife fences are the aim and the measured reductions seem to be disappointing, breeches in the wildlife fence can be excluded as possible explanation if, simultaneously with the collection of data on road-kill, mitigation defects are monitored. We recommend documenting spatial and/or temporal variability in: (i) features of the road and traffic; (ii) features of the road mitigation works; (iii) features of the surrounding landscape; and (iv) weather conditions. Road-related variables include road width, whether the road is in a cut or elevated on fill, presence and type of pavement, streetlights, fences, noise screens, median strip and road verges, and type and frequency of road management. Traffic-related features are primarily volume and speed, which should be documented on several temporal scales, for example, daily, seasonally or annually. Road mitigation variables include the design and size of the mitigation structure(s), the type and frequency of management, the type and frequency of defects and the presence and frequency of use by non-target species, humans, domestic animals and livestock. Important landscape variables include altitude, topography, land use, type and amount of vegetation and the occurrence of important landscape elements, such as hedgerows or ponds. Finally, weather conditions during data collection should be documented, including, where relevant, temperature, cloudiness, precipitation, snow cover depth and wind speed. Of course, this list of variables must be adapted to the chosen indicators, species and effects to be monitored.

10. Make the evaluation report and raw data widely available

Evaluations of the performance of mitigation works, based on an outcome-based procurement approach, will likely result in an exponential increase in our knowledge on mitigation functioning and effectiveness. Studies that address road mitigation effectiveness are currently viewed by most road agencies as ‘optional’ and are hence rare. Incorporating such studies into the process of procurement and contract evaluation will make them a routine activity and hence numerous. In order to learn from each other and make sure that all findings can be easily accessed and used, new methods to report and share the data should be developed. We recommend developing a standard protocol for archiving the collected data across projects, including all relevant meta-data. We also recommend focusing on peer review of technical reports and publication in scientific journals to improve the quality and rigor of the applied methods as well as improve access to the findings. In addition, monitoring data should not be owned by the contracted companies but made available publicly for independent scrutiny and later meta-analysis. This will help to ensure that future road mitigation projects can build on evidence-based knowledge.

2.3 The guidelines applied: examples

We illustrate the use of these guidelines by two hypothetical examples of road mitigation projects. These examples are identical to the ones presented in SAFEROAD Technical report 2 (Van der Grift & Seiler, 2016), where they were used to illustrate the use of the guidelines for developing outcome-based specifications. Hence, here we extend the examples with recommendations on evaluating mitigation performance. The first case addresses the mitigation of a road where large numbers of toads are being killed during spring migrations, and consequently the survival of the local toad population is at stake. The second case addresses the mitigation of a road on which moose are frequently killed, and consequently road safety is in jeopardy. These examples have been selected arbitrarily: similar examples could have been selected for other vertebrate groups or invertebrates.

2.3.1 Case 1: Toad on the road

A local road crosses toad habitat and separates their land habitat from their breeding ponds. Hence, the toads have to cross the road twice a year, during spring migration and when they return to their land habitat after breeding. Each year, especially in spring, many toads are killed on a 1-km road stretch due to traffic. The population size is still considerable, but shows a negative trend. To prevent the deaths of toads on the road and a further decrease of population numbers, the road agency initiated a road mitigation project. The ambition is to install a number of crossing structures that should bring the toads safely across the road and keep the population healthy.

The following set of outcome-based specifications were proposed (Van der Grift & Seiler, 2016):

1. The mitigation measures will allow at least 90% of the migrating toads to get across safely.
2. The mitigation measures will ensure that no more than 5% of the migrating toads will be killed in traffic.
3. The mitigation measures will ensure that the survival probability of the toad population is >99%, calculated over a 100-year period.
4. The mitigation measures will be in effect year-round.
5. The mitigation measures will meet the requirements of specification 1 to 4 in the first year after installation.
6. The mitigation measures and population will be monitored for a period of 5 years to determine whether specifications 1 to 4 are being met.

We hypothesise that, based on these specifications, the contractor installs amphibian fences over the full road length (1 km) where migrating toads - dead or alive - have been detected and constructs five amphibian tunnels, evenly distributed within this road stretch, that should allow the toads to cross safely.

To evaluate the performance of these mitigation measures we propose the following approach, structured by the recommended guidelines:

1. Select performance indicators that are most closely related to the desired outcome
We select four performance indicators that reflect the outcome-based specifications: (1) percentage of successful crossings, (2) percentage of unsuccessful crossings (road-kill), (3) percentage of road-kill reduction and (4) change in trend in population size. The percentage of successful crossings is calculated on the basis of a comparison between the number of toads that try to cross the road and the number of toads that actually cross through the tunnels. The percentage of unsuccessful crossings is calculated on the basis of a comparison between the number of toads that try to cross the road and the number of toads that, despite the mitigation, still end up as road-kill on the pavement. The percentage of road-kill reduction is calculated on the basis of a comparison of road-kill numbers before and after the mitigation works were installed. The change in trend in population size is derived from a comparison of pre- and post-mitigation population counts. The outcome-based specifications state that "the mitigation measures ensure that the survival probability of the toad population is >99%, calculated over a 100-year period". To assess whether this will be achieved, long-term survival probability will be estimated on the basis of successful and unsuccessful crossing numbers, with the help of a population model provided through the Road Mitigation Calculator (see roadmitigationcalculator.eu). The empirical population counts will serve as input to validate the model.

2. Select a study design that incorporates the assessment of reference values
The first two performance indicators use the number of toads that try to cross the road as a reference value. Hence, these performance indicators make use of reference values derived from measurements at a 'reference site'. The third and fourth performance indicators make use of reference values derived from measurements of baseline conditions, i.e., the number of road-kill and trend in population size before the mitigation works were installed.

3. Select a study design that incorporates data collection at control sites
Control sites cannot be identified as the road mitigation covers the full road length over which migrating toads, and road-kill, have been observed.

4. Select survey methods that are the most accurate, informative and efficient
To assess the percentage of successful crossings, we select a capture-mark-recapture survey method. Toads that approach the road are captured and provided with a unique marking, e.g., through fitting coloured rubber bands around their legs, attaching numbered stickers on their back or injecting Passive Invasive Transponders (PIT tags). Toads that pass through the tunnels are recaptured with the help of a pitfall at the end of each tunnel. The unique marking allows us to infer what percentage of the approaching toads made it across. To assess the percentage of unsuccessful crossings and percentage of road-kill reduction we use daily road-kill surveys. To assess changes in trends in population size we select two survey methods: counts of females that approach the road during spring migration and counts of egg-strings in the breeding ponds during the reproduction period.

5. Select an appropriate spatial scale for data collection
The study site includes the mitigated road stretch, two 100-m unmitigated road stretches beyond the fence-ends, the land habitat zone adjacent to the road where the toads approach and the breeding habitat (ponds) on the opposite side of the road where the toads reproduce.

6. Time data collection on the basis of the mitigation goals, lifecycle of the target species and moment an effect is expected

Post-mitigation data collection will start in the first year after the mitigation works have been installed. The timing of data collection in each study year is linked to the start and end of the spring migration as well as the start and end of the post-breeding migrations of both adults and juveniles in the opposite direction.

7. Base study duration on the expected sampling time needed for adequate statistical power

The duration of data collection after the mitigation works have been installed is five years, as prescribed in the specifications. Through power analysis, we assess whether or not this will be sufficient for adequate statistical power. If not, we will recommend a different study duration to the road agency. Data collection should preferably begin at least two years before the mitigation works are being installed to estimate the percentage of road-kill reduction and at least three years before mitigation to estimate the change in trend in population size.

8. Use a sampling frequency that allows for rigorous estimates of the performance indicator

The necessary surveys to assess successful and unsuccessful crossings and road-kill reduction are done on a daily basis over the time that the migrations occur. The necessary surveys to assess changes in trends in population size are done respectively on a daily basis (counts of females) and a weekly basis (counts of egg-strings) over the time that the spring migrations and reproduction occur.

9. Measure explanatory variables that may affect mitigation performance

We document all spatial and/or temporal variability in: (i) features of the road and traffic; (ii) features of the road mitigation works, including possible defects; (iii) features of the surrounding landscape; and (iv) weather conditions.

10. Make the evaluation report and raw data widely available

We archive all collected data and meta-data in a database that can be accessed through a web portal. The study results will be published in a peer-reviewed report and made accessible through the same web portal.

2.3.2 *Case 2: Moose on the loose*

A highway crosses moose habitat. Suitable feeding areas occur on both sides of the highway and hence moose cross the road frequently. Over the past five years an average of ten moose-vehicle collisions occurred each year on a 4-km stretch of the highway - hereafter referred to as 'hotspot'. All collisions resulted in the death of the animal, but only a few caused human injuries, and one collision resulted in a human fatality. The populations on both sides of the road are sufficiently large and not seriously affected by the number of traffic-related mortality. Moose movements across the highway also occur elsewhere; however, they rarely result in accidents outside the collision hotspot due to differences in road design and the presence of bridges and tunnels that moose serve as safe passage. To increase road safety, the road administration initiates a mitigation project. The ambition is to take measures that will keep the moose off the road and reduce the number of collisions.

The following set of outcome-based specifications were proposed (Van der Grift & Seiler, 2016):

1. The mitigation measures will reduce the number of moose-vehicle collisions at the collision hotspot by at least 80%, compared to the mean number of collisions at the hotspot over the past five years.
2. The mitigation measures at the hotspot will not cause an increase in the number of moose-vehicle collisions on adjacent highway stretches without mitigation, compared to the mean number of collisions at these stretches over the past five years.
3. The mitigation measures will be in effect year-round.
4. The mitigation measures will meet the requirements of specification 1 to 3 in the first year after installation.
5. The mitigation measures will be monitored for a period of 5 years to determine whether specifications 1 to 3 are being met.

We hypothesise that, based on these specifications, the contractor installs wildlife fences that keep moose off the road over the full road length of the collision hotspot.

To evaluate the performance of these mitigation measures we propose the following approach, structured by the recommended guidelines:

1. Select performance indicators that are most closely related to the desired outcome
We select one performance indicator that reflects the outcome-based specifications: the number of moose-vehicle collisions. The percentage of collision reduction is calculated on the basis of a comparison of collision numbers before and after the mitigation works were installed.

2. Select a study design that incorporates the assessment of reference values
The selected performance indicator makes use of reference values derived from measurements of baseline conditions, i.e., the mean number and the annual variance of collisions reported before the fences were installed.

3. Select a study design that incorporates data collection at control sites
As controls, two known hotspots - 3 km in length - at other roads in the same region but not in the immediate vicinity of the targeted hotspot are selected and monitored with respect to traffic volume, moose-vehicle collisions and hunting statistics as indicators for moose population densities. Relative changes in mean collision frequencies are used as an index that compares with the observed relative change on the target road.

4. Select survey methods that are the most accurate, informative and efficient
To assess the percentage of collision reduction we rely on police-reported incidents and on reports made by hunters who have visited the accident sites.

5. Select an appropriate spatial scale for data collection
The study site includes the mitigated road stretch (4 km) and 1-km unmitigated road stretches beyond the fence-ends, 1-km road stretches along connecting or intersecting private roads and the two 3-km control sites.

6. Time data collection on the basis of the mitigation goals, lifecycle of the target species and moment an effect is expected
Post-mitigation data collection will start during the year of fence installation. Data will be collected continuously over the following five years.

7. Base study duration on the expected sampling time needed for adequate statistical power
The duration of data collection is five years before and five years after the mitigation works have been installed, as prescribed in the specifications. Through power analysis we assess whether this will be sufficient for adequate statistical power given the annual variance in accident numbers. If not, the study period will have to be extended.

8. Use a sampling frequency that allows for rigorous estimates of the performance indicator
The necessary data collection to assess collision reductions is done annually for the duration of the study.

9. Measure explanatory variables that may affect mitigation performance
On both the target road and the control roads, we document all spatial and/or temporal variability in: (i) features of the road and traffic; (ii) features of the road mitigation works, including possible defects; (iii) features of the surrounding landscape; (iv) hunting statistics, and (v) snow cover.

10. Make the evaluation report and raw data widely available
We archive all collected data and meta-data in a database that can be accessed through a web portal. The study results will be published in a peer-reviewed report and are made accessible through the same web portal.

2.4 Recommendations for implementation

The use of outcome-based specifications and a corresponding framework for evaluation will put much more emphasis on the road mitigation goals and the extent to which these goals have been achieved. To accomplish this, we need a strong set of performance indicators and a transparent method to judge whether aimed for performances have been achieved. Here we provide a few recommendations for road administrations on how to implement the use of the guidelines presented here for evaluating road mitigation performance.

- Successful evaluations of road mitigation performance will require close collaboration, from the earliest stages of a road mitigation project, between ecologists and those who plan, design, construct and manage the road. We recommend that ecologists in road agencies become more involved in the procurement process of road mitigation works, for example, by writing SMART outcome-based specifications, organising the collection of baseline information and judging evaluation plans proposed by contractors. The researchers need to inform the road agency of the essential components of good study design for road mitigation evaluations.

- Contract an independent contractor to evaluate road mitigation performance. It is not advisable to put both the designing/constructing and evaluating of the mitigation measures - whether or not the objectives are being met - in one contract. Besides possible conflicts of interests, this approach allows a contractor to be selected for the evaluations solely based on their ecological knowledge and experience. If designing/constructing and evaluating the mitigation works is put in one contract, the ecological knowledge and experience may play a much smaller role in selecting the contractor as the evaluation activities are usually only a small percentage of the total budget and hence relatively little weight is given to the ecological expertise.

- We recommend establishing an independent advisory board, consisting of experienced road ecologists, to assist the road agency in reviewing proposed outcome-based specifications as well as advising on performance evaluations that meet good science. Such an advisory board may also play a key role in ensuring that acquired knowledge and best-practices will be available to all stakeholders.

- We recommend that the preparation of a plan to evaluate planned road mitigation measures is made an inseparable part of the legal processes that must be followed during the road planning and procurement stages. Evaluations of mitigation performance should not be optional, but rather a statutory duty that form an integrated whole with the procurement of the works.

- Develop a strategy for systematic assessments of baseline conditions and reference standards. Baseline conditions should be known at the start of procurement and this also applies to certain reference standards that the road agency may want to prescribe. This implies that a new way of working should be adopted as currently such systematic assessments are often lacking at the start of procurement.

- We recommend that all necessary resources to evaluate road mitigation performance are secured beforehand in close collaboration with other stakeholders, such as environmental agencies. Road mitigation evaluations based on outcome-based specifications require significantly more resources than evaluations based on design specifications. Therefore, early insight into the costs of evaluation studies is required, and these costs have to be treated as an integral part of the road or road mitigation construction project.

- We recommend that the outcome of all evaluations, including research reports and raw data, is made available to all stakeholders through an open access database. Research methods, results and conclusions should be documented systematically, thus allowing for quick reference and proper comparisons between projects. Furthermore, all data should be analysed and reported in a timely manner to ensure existing structures can be modified within an adaptive framework and the design of future mitigation measures can be improved.

- Evaluate and improve our set of guidelines to evaluate road mitigation performance.

3 Case study 1: Study design

3.1 Introduction

Study design is critical in any evaluation of road mitigation performance. The optimal study design for evaluating road mitigation performance is a BACI design (Before-After-Control-Impact), which includes the collection of data before and after road construction, at the road sites where mitigation is installed (mitigation sites) and at road sites without mitigation (control sites) (see also Chapter 2; Roedenbeck et al., 2007; Van der Grift et al., 2013). Such a study design will provide baseline information on the before situation and will allow for corrections of response variables, based on the measurements at the control sites. In this chapter we use a real-life case to illustrate the importance of a BACI design in road mitigation evaluations and to show how such evaluations can be done. Further, we use the case to illustrate the importance of selecting an appropriate spatial scale for data collection, more specifically, the importance of including data collection at fence-ends. The case study presented here concerns the construction of a wildlife overpass and wildlife fences at a two-lane highway to reduce road-kill numbers of roe deer. Besides the research findings, this case study is primarily included here to illustrate the need for careful decisions on study design in order to provide strong inferences on road mitigation performance.

3.2 Methods

Study site

The study site is an approximately nine-kilometre road stretch of highway N227 in the Netherlands. This road stretch crosses prime roe deer habitat, consisting of mixed forests, heathlands and grasslands (Figure 3.1). In the years 2005-2008 an average of about three roe deer were killed per kilometre of road each year. Early in 2009 a wildlife overpass was installed, accompanied by wildlife fences on both sides of the road (Figure 3.2). The fences had two objectives: to keep roe deer (and other wildlife) off the road and to guide the animals towards the overpass. The fences were installed along 3.2 km of road length: 2.8 km at both sides of the road and 0.4 km at only one side of the road. Different types of fences were used, depending on landscape features: (1) two-metre high wildlife fences where forests were crossed or bordered and (2) one-metre high wildlife fences, in combination with a one-metre deep ditch with a steep edge on the side of the road, where open areas, such as heathlands, were crossed or bordered (Figure 3.3). In effect, the second fence type also forms a two-metre high barrier.

Study design

We used a BACI design to evaluate the effect of the mitigation works on roe deer road-kill numbers. We used road-kill data from both before and after the installation of the mitigation measures. We distinguished two types of mitigation sites: (1) the road stretch where wildlife fences were installed on just one side of the road, hereafter referred to as treatment site 1 and (2) the road stretch where wildlife fences were installed on both sides of the road, hereafter referred to as treatment site 2. The overpass is located within treatment site 2. Further, we selected two control sites elsewhere on the N227 that were very similar to the treatment sites in terms of road, traffic and landscape features. The control sites covered a road length of 1.9 km (control site 1) and 1.7 km (control site 2). In addition, we distinguished two fence-end sites (fence-end site 1 and 2), each covering a road length of 0.9 km. Figure 3.4 provides a schematic overview of the treatment, fence-end and control sites.

Figure 3.1 Highway N227 bisecting roe deer habitat at the Utrechtse Heuvelrug in the Netherlands.

Figure 3.2 Wildlife overpass 'Treeker Wissel' and exclusion fencing at the N227.

Figure 3.3 Wildlife exclusion barriers along highway N227: two-metre high wildlife fences without a ditch (left) and one-metre high wildlife fences in combination with a one-metre deep ditch with a steep edge on the side of the road (right).

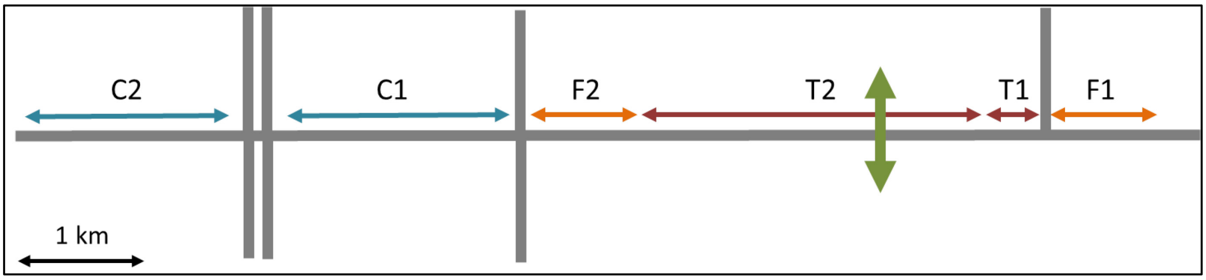

Figure 3.4 Schematic overview of the study set up, including treatment (T), fence-end (F) and control (C) sites. Roads in grey. Green arrow indicates location of the overpass.

Data collection

The Stichting Valwild Utrecht (SVU) collected road-kill data of roe deer at highway N227 between April 2005 - December 2014. The data included road-kill counts by SVU volunteers, notifications of dead roe deer by the general public and road-kill reports of the road police. For each road-killed roe deer at least date and time of the observation, location, sex and age group were registered.

Data analysis

For each survey year and site (treatment, fence-end, control) we calculated the mean roe deer road-kill numbers per kilometre of road. Because data collection in 2005 did not start until April, we extrapolated the calculated means for this survey year to a full year through multiplying the means by a correction factor of (12/9=) 1.33. After that we calculated the mean yearly roe deer road-kill numbers per kilometre of road for the before (2005-2008) and after (2009-2014) situation at each site. We were then able to calculate the mean reduction (in %) in road-kill per kilometre per year after mitigation. In the latter calculation we corrected for changes in road-kill that could not be attributed to the mitigation. To do so we calculated a correction factor, based on the measured differences in road-kill before and after mitigation at the control sites. We tested for differences between the means before and after mitigation through the two-sample Poisson test as the data are discrete counts that follow the Poisson distribution.

3.3 Results

After mitigation the average reduction of road-kill of roe deer was 51% (range 32-62%) at treatment site 1 and 88% (range 83-90%) at treatment site 2 (Figure 3.5). At treatment site 1 and 2, mean road-kill per kilometre per year decreased respectively from 2.7 to 1.3 and from 2.5 to 0.3 deer (Figure 3.6). Only the road-kill reduction at treatment site 2 was found statistically significant (two-sample Poisson test, $p<0.01$).

After mitigation the road-kill of roe deer increased by 157% at fence end site 1 and decreased by 27% at fence end site 2 (Figure 3.5). At fence end site 1 mean road-kill per kilometre per year increased from 0.8 to 2.0 deer (Figure 3.7). At fence end site 2 mean road-kill per kilometre per year decreased from 5.4 to 3.7 deer (Figure 3.7). The measured changes in road-kill numbers at both fence end sites were not statistically significant (two-sample Poisson test, $p=0.104$ (site 1), $p=0.234$ (site 2)).

After mitigation had been installed at the treatment sites mean road-kill numbers decreased by 33% at control site 1 and increased by 22% at control site 2 (Figure 3.5). This results in a correction factor of 0.95, i.e., 5% of the measured road-kill was attributed to factors other than the mitigation measures.

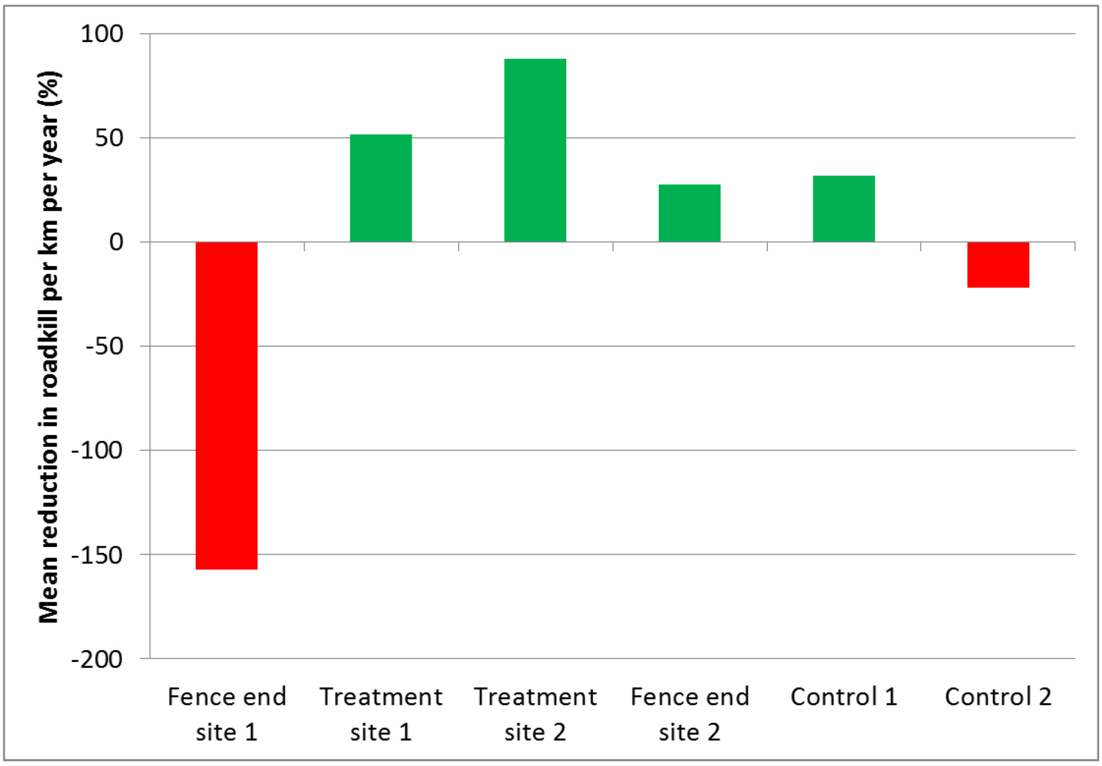

Figure 3.5 Proportional mean reduction in road-kill after mitigation per kilometre of road per year at treatment, fence-end and control sites. Negative reductions indicate an increase in road-kill after mitigation, here emphasised by red bars.

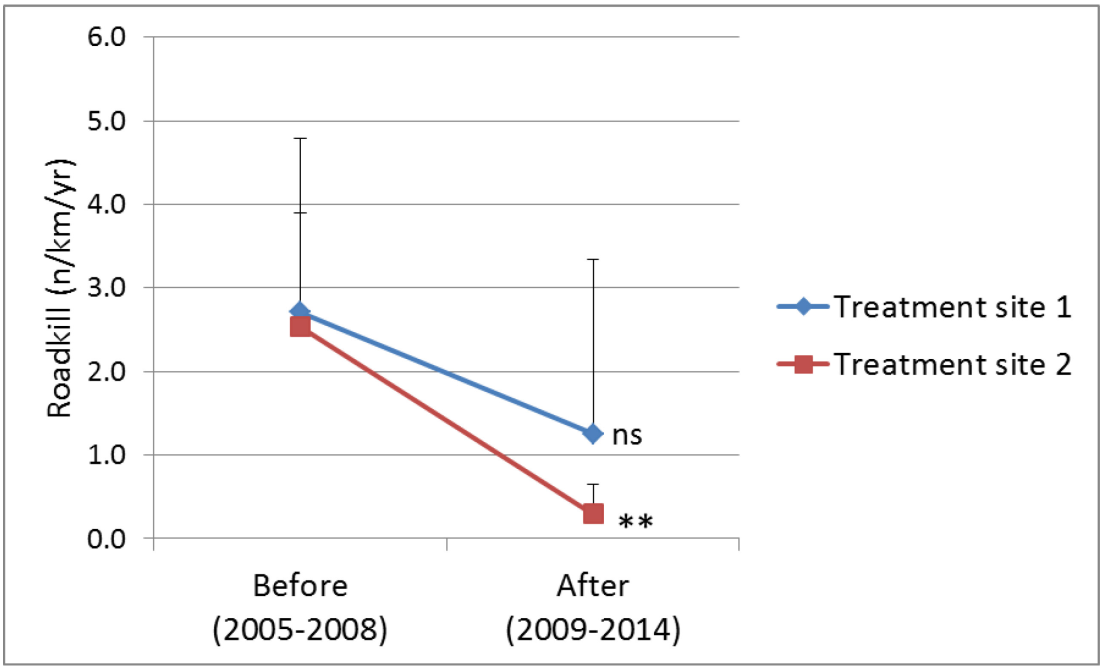

Figure 3.6 Mean road-kill before and after mitigation per kilometre of road per year at the two treatment sites. Error bars represent standard deviations. ns = not statistically significant ($p>0.05$); ** statistically significant ($p<0.01$).

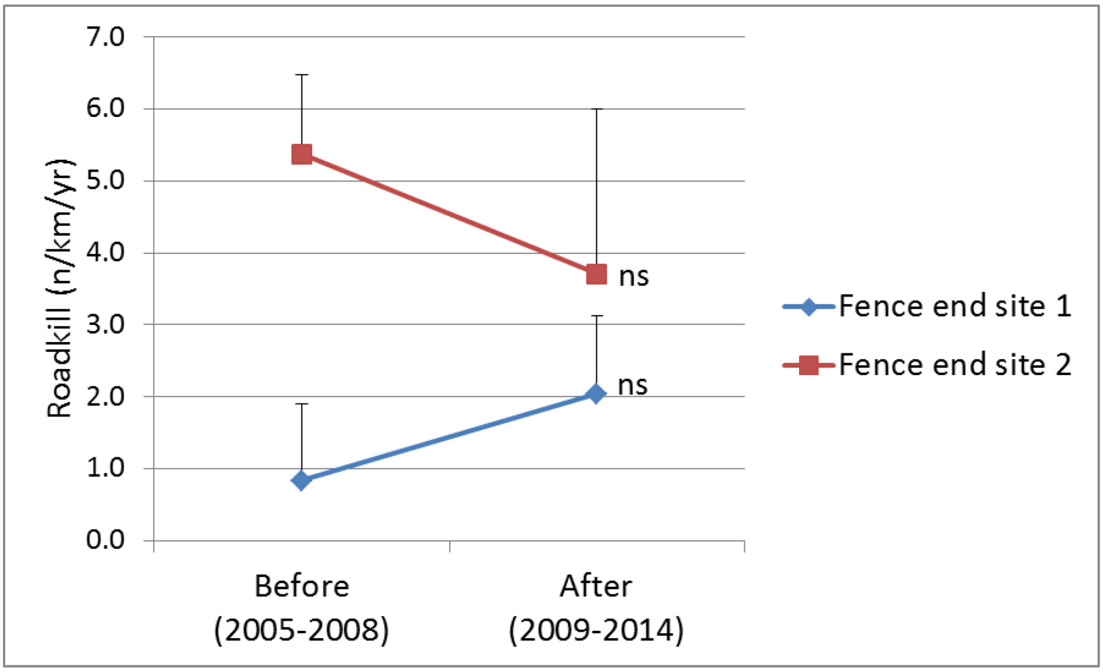

Figure 3.7 Mean road-kill before and after mitigation per kilometre of road per year at the two fence end sites. Error bars represent standard deviations. ns = not statistically significant ($p>0.05$).

3.4 Discussion

Treatment site 1 is characterised by a wildlife fence on only one side of the road. Treatment site 2 has a wildlife fence on both sides of the road. Although road-kill reductions are seen at both treatment sites, the reductions are considerably stronger and statistically significant only at treatment site 2. Although this may be partly the result of low sample size, we argue that there is no evidence in our study that erecting fences on just one side of the road results in road-kill reductions among roe deer. Such measures may, theoretically, even result in an increase in road-kill because deer that cross the road from the non-fenced side will encounter the barrier on the other side and will have to cross back again. Further studies should be carried out at multiple sites with fences on only one side of the road to learn more about the effectiveness of such a measure in reducing road-kill among roe deer.

We found an opposite trend in road-kill numbers at the fence end sites. At fence end site 1 road-kill numbers went down, while at fence end site 2 road-kill numbers went up. Although the changes in road-kill numbers are not statistically significant, possibly due to limited sample size, the data clearly illustrate that road-kill numbers beyond the fence ends may change after road mitigation measures have been taken. The case study presented here also shows that the direction of change may differ per fence end site. Potential changes in road-kill numbers - or other performance indicators, such as between-population movements - at fence ends will likely depend on a variety of factors, including landscape, land use and habitat features. Such changes will also strongly relate to the number and type of crossing structures present. Hence, fence end sites should be included in all road mitigation evaluations to avoid the effects of mitigation being over- or underestimated.

Changes in road-kill numbers at the fence end sites should be included in the evaluation of mitigation performance. After all, an increase in road-kill at the fence ends may be a direct result of the construction of the fence. This will change the inferences we make on the overall effectiveness of the mitigation measures. If we combine the two treatment sites and the two fence end sites into one ‘mitigation site’, the reduction in road-kill after mitigation is 50% and not statistically significant (two-sample Poisson test, $p=0.127$). Hence, while a significant road-kill reduction of 88% is achieved if the focus is on just treatment site 2, this reduction is much less and not statistically significant if fence end effects are taken into account.

Collecting data at control sites ensures that measured changes can be attributed to the mitigation. We found a different trend in road-kill numbers at the two control sites. At control site 1 road-kill numbers went up, while at control site 2 road-kill numbers went down. The use of a correction factor based on only control site 1 - instead of both control sites - would result in lower estimates for the road-kill reductions due to mitigation. On the other hand, the use of a correction factor based on only control site 2 would result in higher estimates for the road-kill reductions due to mitigation. Our data illustrate the variability that can be found between control sites. Such variability is not surprising as identical control sites can never be found. Hence, it is advisable to select multiple control sites, if available, to improve correction factor estimates.

3.5 Conclusions

The construction of wildlife fences and an overpass at highway N227 resulted in a significant reduction in road-kill numbers among roe deer at the road stretch where fences were erected on both sides of the highway. With the help of control sites, this road-kill reduction can be attributed to the mitigation works. Fewer road-kill reductions were seen where fences were erected on only one side of the road. However, these reductions in road-kill have been partly cancelled out by an increase in road-kill beyond one of the fence ends. The case study shows the importance of a BACI design and attention for data collection at fence ends in road mitigation evaluations.

4 Case study 2: Timing of monitoring

4.1 Introduction

Road mitigation evaluations use various sampling schemes depending on, for example, the research question, the type of mitigation measure, the target species and the available resources. An important part of the sampling scheme is the timing of data collection. Data collection should preferably take place for the full period in which the performance indicator, e.g., road-kill numbers or crossing rates, is relevant (see also Chapter 2). As is also stated in the Handbook of Road Ecology: *"If the aim is to evaluate a species' daily movements in an area that encompasses a crossing structure, monitoring throughout the year – excluding periods in which the species is inactive or does not occur – is recommended."* (Van der Grift & Van der Ree, 2015). Currently, however, there is a tendency to sample only for part of that period. The literature shows survey periods limited to, for example, 5 weeks in spring and 5 weeks in autumn (Emond & Brandjes, 2013; 2014a; 2014b; 2014c) 2013), 4-5 weeks in spring and 4-5 weeks in autumn (Lindenholz & Peterman 2014), 3-4 weeks in spring and 3-4 weeks in autumn (Peterman, 2014) and 5 weeks in spring (Kleinjan, 2015). The assumption in these studies is that such limited survey periods provide representative data on the number of species that use the crossing structures, the crossing rates of these species and, in some studies, the road-kill rates of these species. However, most wildlife species show different activity and movement patterns throughout the year. Hence, will such limited survey periods result in a complete list of species that actually use the crossing structures and in accurate estimates of crossing and road-kill rates? Can the assumption be justified and are the claims of representation true? What recommendations can be provided for the timing of data collection if outcomes of analyses based on data from a limited survey period are compared with analyses based on data from the full survey period? In this chapter we will address these questions on the basis of existing datasets on wildlife crossing numbers at five wildlife overpasses in the Netherlands. The focus is on medium-sized and large mammals. Besides the research findings, this case study is primarily included here to illustrate the need for careful decisions on the timing of data collection in order to assess what survey schemes are most reliable and efficient in road mitigation evaluations.

4.2 Methods

Study sites

Our study sites are five wildlife overpasses across transport corridors in the Netherlands. Table 4.1 provides an overview of the main features of these study sites. With one exception (bridged multiple transport infrastructure barriers), the study sites were selected on the basis of the availability of at least one year of data on mammal use of the crossing structure. Recreational use of the overpass is allowed at four of the sites. At three sites the overpass is open to pedestrians, cyclists and horseback riders, while at one site only horseback riders are allowed.

Table 4.1. Main features of the study sites. Legend recreational co-use: - = no recreational co-use allowed; P = pedestrians; C = cyclists; H = horseback riders.

Overpass ID	Overpass name	Bridged barrier	Year of opening	Overpass width (m)	Overpass length (m)	Recreational co-use	Connected habitat types
1	Groene Woud	Motorway and local road	2005	50	65	-	Wetland forest and grassland
2	Zanderij Crailoo Naarderweg	Highway	2006	50	30	P,C,H	Mixed forest of dry sandy soils and heathland
3	Zanderij Crailoo Spoor	Railway and industrial site	2006	50	130	P,C,H	Mixed forest of dry sandy soils and heathland
4	Slabroek	Motorway and highway	2003	15	100	P,C,H	Mixed forest of dry sandy soils, heathland, and grassland
5	Zwaluwenberg	Motorway and railway	2013	50	115	H	Mixed forest of dry sandy soils and heathland

Study design

The one-year datasets allow comparisons between parameter values based on the full survey period of one year and parameter values based on more limited survey periods. We distinguished 39 scenarios, each with a unique period of data collection, based on the variables season (spring, autumn, spring and autumn) and number of weeks surveyed (1-13 weeks). In these 'spring' refers to surveys in the months March-May, and 'autumn' to surveys in the months September-November. Within each season we used all possible starting dates. Our parameters of interest were (1) the number of species detected, (2) the number of crossings over all species per year and (3) the average number of days after which all species have been detected. The parameter values of each scenario were compared with the parameter values based on an analysis of the full - one-year - dataset.

Data collection

Table 4.2 provides an overview of the main features of data collection at each study site. At overpasses 1, 2, 3 and 4 the track beds covered the full width of the overpass except, if present, the recreational trails (Figure 4.1). The track beds were about 2.5 m wide and consisted of a 0.15 m layer of loam-poor sands. Impermeable root canvas was put underneath to prevent quick invasions of weeds. The track beds were inspected two to five times a week (Table 4.2). During an inspection all mammal tracks were recorded, including the direction of movement. After each inspection the track beds were raked to maximise readability during the next survey and avoid double counts.

At overpass 5 we used four Reconyx PC900 Hyperfire Professional High Output Covert IR cameras: digital cameras with an infrared illuminator and Passive InfraRed (PIR) motion detector. The field of view of the cameras is 40 degrees and their detection range is up to about 30 metres depending upon temperature and sensitivity settings. The cameras were placed in a row - about 0.4 m above ground level - and covered the full width of the overpass, including the recreational trail. Once a week the battery status of the cameras was checked and pictures taken were collected.

Table 4.2. Main features of data collection at each study site and mammal species detected.

Overpass ID	Survey period			Survey method	Sampling frequency (days/week)	Number of species detected	Species detected
	Duration (months)	*Start date*	*End date*				
1	12	1-1 2007	31-12 2007	Track bed	2	7	Badger, Hedgehog, Polecat, Rabbit, Red fox, Roe deer, Stoat
2	12	1-6 2007	31-5 2008	Track bed	5	9	Badger, Hare, Hedgehog, Pine marten, Polecat, Rabbit, Red fox, Red squirrel, Roe deer
3	12	1-6 2007	31-5 2008	Track bed	5	9	Badger, Hare, Hedgehog, Pine marten, Polecat, Rabbit, Red fox, Roe deer ree, Stoat
4	12	1-9 2007	31-8 2008	Track bed	3-4	8	Badger, Hare, Hedgehog, Polecat, Rabbit, Red fox, Red squirrel, Roe deer
5	12	1-3 2014	28-2 2015	Camera traps	7	5	Badger, Hare, Rabbit, Red fox, Roe deer

Figure 4.1 Roe deer buck crossing a track bed at wildlife overpass Zanderij Crailoo.

Data analyses
For each overpass (except overpass 5) and scenario we calculated the mean number of species detected, averaged over all starting days within a scenario. We calculated the fraction of detected species by dividing this mean number by the assessed number of species detected over the full year. Further, we averaged the fractions per scenario over all overpasses. For each overpass (except overpass 5) and scenario we calculated the detection probability of all species, averaged over all starting days within a scenario. Detection probability of all species is here defined as the percentage of all starting dates that result in the detection of all species. Further, we averaged the detection probabilities per scenario over all overpasses. This analysis could not be done for overpass 5 due to the theft of cameras and consequently the lack of data for three weeks in autumn.

For each overpass (except overpass 5) and scenario we calculated the mean number of crossings per year over all species, averaged over all starting days within a scenario. We extrapolated these numbers to mean number of crossings per year, after which we calculated the fraction of detected crossings per year by dividing these values by the assessed number of crossings detected over the full year. Further, we averaged the fractions per scenario over all overpasses. This analysis could not be done for overpass 5 due to the theft of cameras and consequently the lack of data for three weeks in autumn.

For each overpass and time period (month, season, year) we calculated the mean number of days after which all species have been detected, averaged over all starting days within the period of concern. Each season consisted of three months: spring (March-May); summer (June-August); autumn (September-November); winter (December-February). Further, we averaged the mean number of days per time period over all overpasses.

At the overpasses 1-4 more than one track bed had been installed. For this study we selected the track bed where most species had been detected over the one-year survey period or, if the number of detected species was equal between track beds, where the most crossings had been registered. In the analyses we included only events where animals appeared to fully cross the track bed (overpass 1-4) or centre line of the camera trap (overpass 5). To allow proper comparisons between seasons and/or sites, we excluded from analyses measurements done on May 31 and, whenever applicable, February 29 (intercalary day). Hence, each season consisted of exactly 91 days and each year of 365 days. Also excluded from the analyses were events in which crossing animals could not be identified at species level, e.g., “small marten”.

4.3 Results

Number of species detected
On average, surveys conducted in spring or autumn result in the detection of respectively 88% and 77% of all species (Figure 4.2) if the surveys take place over the full season (13 weeks). Shorter survey periods result in considerably fewer species detected. For example, if the current trend of surveying only five weeks within a season is used, on average only 72% and 63% of all species have been detected in respectively spring and autumn. The fraction detected of all species improves when surveys are conducted in both spring and autumn. In that case, on average all species have been detected if the surveys cover a minimum of twelve weeks in each season. Shorter survey periods may again result in considerably fewer species detected. If five weeks are surveyed in each season, on average 82% of all species have been detected. If three weeks are surveyed in each season this percentage decreases to 72%.

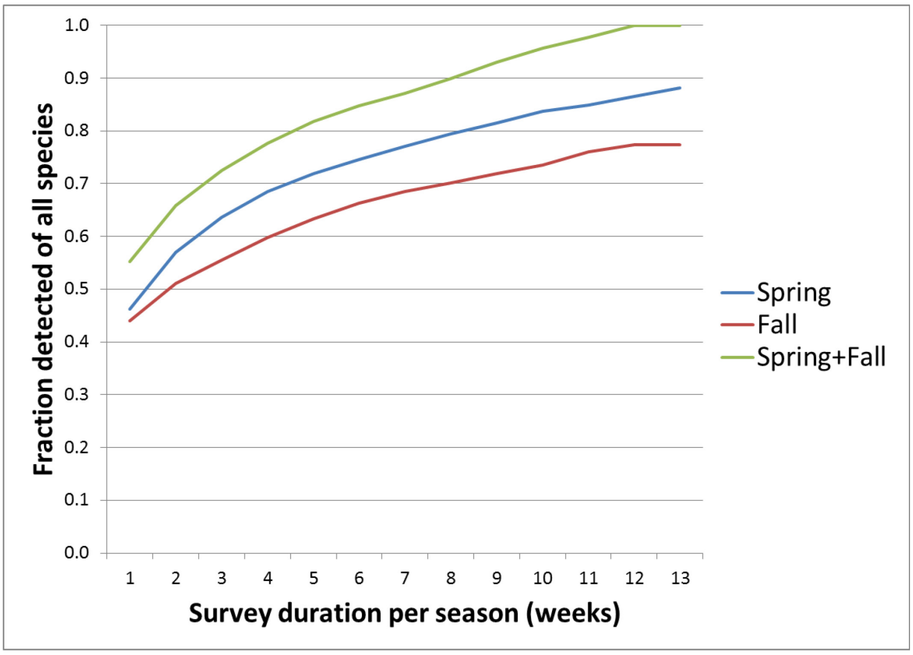

Figure 4.2 Relative difference in number of species detected over all study sites (n=4), depending on season in which the survey takes place and number of survey weeks per season.

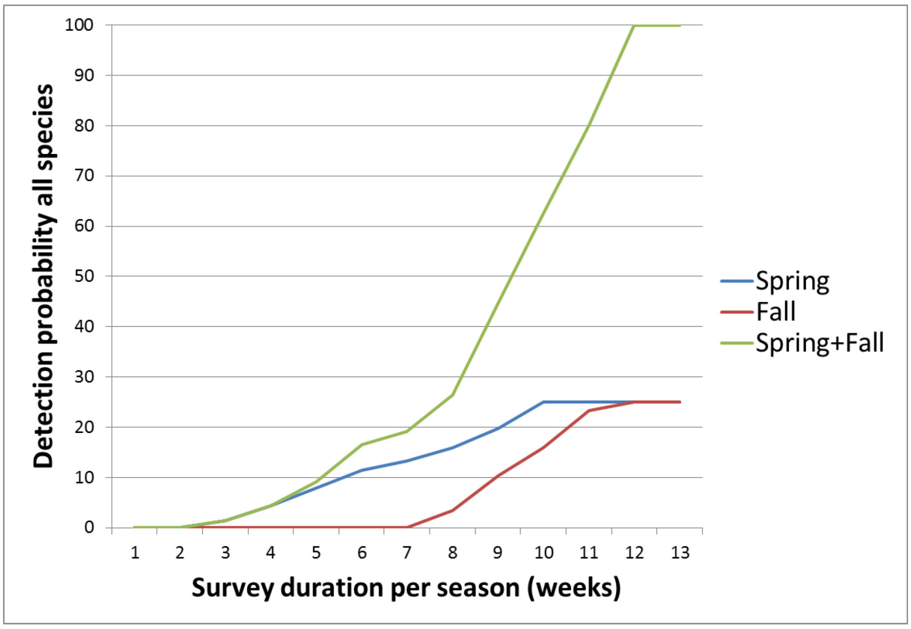

Figure 4.3 Detection probability of all species over all study sites (n=4) and starting dates, depending on season in which the survey takes place and number of survey weeks per season. Detection probability of all species is here defined as the percentage of all starting dates that result in the detection of all species.

The average detection probability of all species is significantly lower for surveys conducted in only spring or autumn if compared to surveys conducted in both seasons (Figure 4.3). On average, in spring or autumn there is a 25% chance of detecting all species if the surveys take place over the full season (13 weeks). Shorter survey periods result in considerably lower detection probabilities. For example, if five weeks are surveyed in each season, on average 8% and 0% of all species have been detected in respectively spring and autumn. The detection probabilities improve significantly when surveys are conducted in both spring and autumn. In that case, average detection probability is 100% if the surveys cover a minimum of twelve weeks in each season. Shorter survey periods may result in considerably fewer detection probabilities. If five weeks are surveyed in each season, on average 9% of all species have been detected. The strongest increase in detection probability takes place between surveys that cover eight weeks and surveys that cover twelve weeks in each season: an average increase of about 18.5% for each additional survey week.

Yearly crossing numbers

On average, surveys conducted in spring result in overestimating yearly crossing numbers by about 40%, while surveys conducted in autumn result in underestimating yearly crossing numbers by about 20% (Figure 4.4) if the surveys take place over the full season (13 weeks). Surveys conducted in both seasons (13 weeks) overestimate yearly crossing numbers by about 10%. In all cases the number of survey weeks do not heavily affect the yearly crossings estimates; shorter survey periods result in only slightly different values.

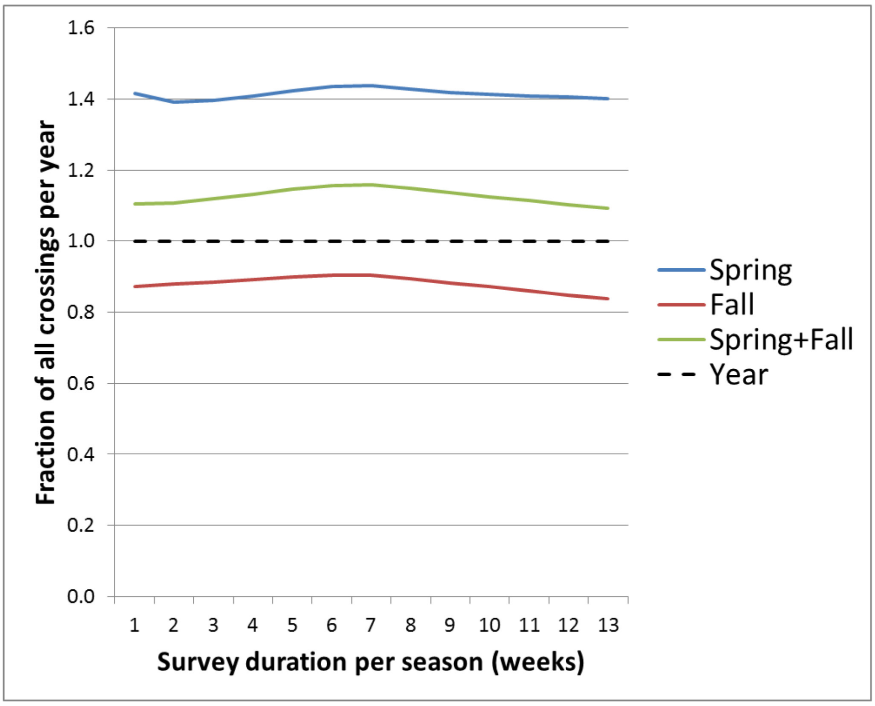

Figure 4.4 Relative difference in yearly crossing rate estimates (all species) over all study sites (n=4), depending on season in which the survey takes place and number of survey weeks per season.

Minimum number of survey days needed to detect all species

On average, it takes 240 survey days until all species have been detected if each day of the year is a possible starting date (Figure 4.5). This minimum number of survey days needed to detect all species is somewhat lower if the starting date is in spring (219 days). However, there are large differences in the spring season. If the starting date is in March, an average of 169 survey days are needed (Figure 4.6). If the starting date is in May, an average of 290 survey days are needed. The highest number of survey days is needed if the starting date is in autumn (268 days). Surveys that start just before autumn (August), however, need considerably fewer survey days (215 days).

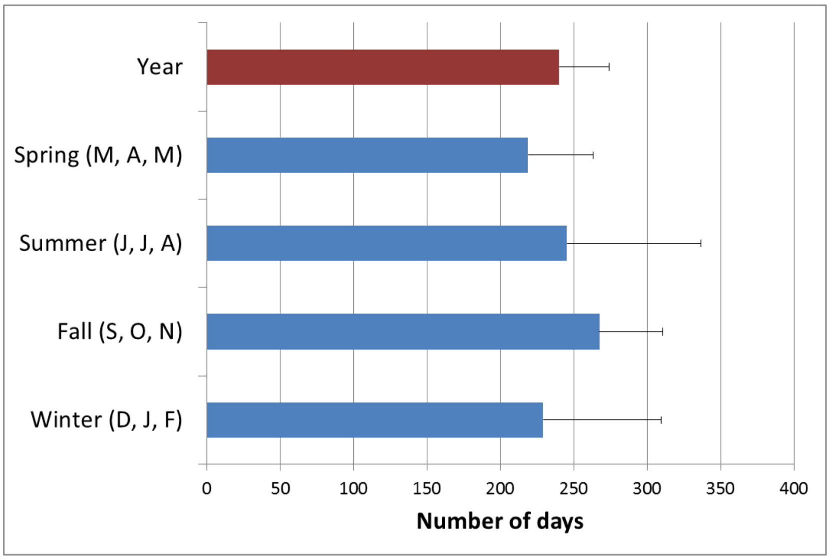

Figure 4.5 Average number of days until all species have been detected over all study sites (n=5) and starting dates within the year and each season. Error bars represent standard deviations.

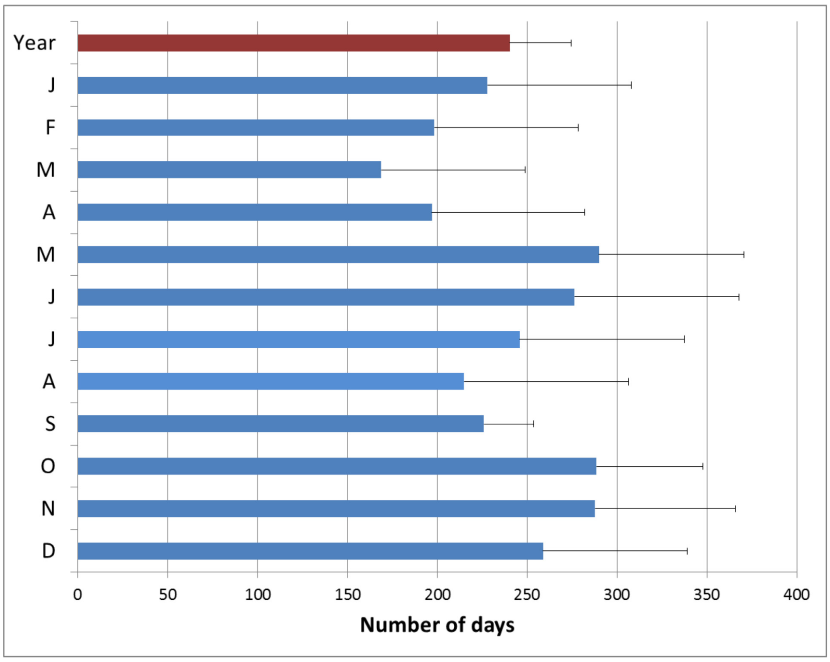

Figure 4.6 Average number of days until all species have been detected over all study sites (n=5) and starting dates within the year and each month. Error bars represent standard deviations.

4.4 Discussion

The findings presented here on how survey timing and duration affect study results are based on a limited number of study sites. Consequently, potential irregular measurements at one study site will considerably affect the calculated means. To increase the rigor of estimates it is critical to increase the number of study sites, preferably across a variety of regions in Europe to account for possible geographical differences. This seems especially relevant in the calculations of the detection probabilities of all species. The detection probabilities of 25% found for surveys conducted over 13 weeks in spring or autumn were, in both cases, the result of a 100% detection probability at just one study site, while detection probability at the other three sites was 0%. Including more study sites would make clear whether such high detection probabilities are exceptional or a common phenomenon.

We grouped all detected mammal species to calculate yearly crossing numbers. As crossing numbers differ considerably between species, this implies that the findings are biased to the species that cross in the highest numbers. Hence, on the level of individual species the outcomes of the analyses may be significantly different, even opposite to what is found for all mammals grouped. For example, surveys conducted in spring underestimated rather than overestimated pine marten and hedgehog crossing rates at overpass 3. Furthermore, differences per species may occur between sites. For example, while red fox crossing rates based on surveys conducted in spring were overestimated at overpass 3 and 4, they were underestimated at overpass 1 and more or less equal to crossing rates based on year-round data at overpass 2. Detailed analyses of the scenarios per species can provide clear insights into how survey schemes can be optimised for particular target species.

The minimum number of days needed to detect all species highly depends on the frequency with which the species cross. Whenever one or more species cross only incidentally, average survey duration quickly increases, and surveys that are limited to only spring or autumn are likely to underestimate the number of species. On average over all starting dates, it takes as much as 183 days (6 months) to detect one species that crosses only once. In the case of more than one species, this minimum number of days needed to detect all species will quickly increase. Note also in this respect that standard deviations of the calculated means over the five study sites are considerable and even in the most promising month (March) to start a survey, the minimum number of days needed to detect all species may be close to 250 days.

4.5 Conclusions

There is low probability of detecting all species in scenarios where surveys are conducted in only spring or autumn even if the surveys cover the whole season. Detection probabilities are higher if the surveys take place in both spring and autumn; however, for a complete species list, the surveys must take place for at least twelve weeks in both seasons. Hence, survey schemes that are based on, for example, five weeks in spring and/or five weeks in autumn do not result in representative estimates of species numbers. There may be moments in the year that the number of species can be assessed accurately with such survey schemes; however, these moments cannot be identified with 100% confidence beforehand. On average, it takes 240 survey days to detect all species. This number of survey days can be lowered if the start date is in spring, especially in March. Yearly crossing rates are either overestimated or underestimated if survey periods are limited; however, there is a high variation between species. Consequently, we conclude that great care is required if one considers limiting surveys to certain weeks or months of the year. Our data supports the guideline by Van der Grift & Van der Ree (2015) to survey ‘throughout the year – excluding periods in which the species is inactive or does not occur’.

5 Case study 3: Survey methods

5.1 Introduction

In road mitigation evaluations various survey methods are used, depending on, for example. the research question, the type of mitigation measure, the target species and the available resources. Currently, most evaluations focus on assessing the use of wildlife crossing structures by the target species. In such studies the most commonly used survey methods for medium-sized and large mammals are track beds and camera traps. Which survey method is preferred in surveys of medium-sized and large mammals? Are there significant differences in crossing rate estimates between the use of one track bed versus two track beds or track beds versus camera traps? What recommendations can be provided for applying these methods to improve crossing rate estimates? In this chapter we will address these questions on the basis of an existing dataset on wildlife crossings at two wildlife overpasses in the Netherlands. Besides the research findings, this case study is primarily included here to illustrate the need for proper comparisons of survey methods in order to assess which ones are the most reliable and efficient.

5.2 Methods

Study area

The study area consists of two wildlife overpasses within the Goois Nature Preservation Area in the Netherlands. The first overpass is located across highway N524 and is 50 m wide and 30 m long (Figure 5.1A). The second overpass is located across the railway Amsterdam-Hilversum and is 50 m wide and 130 m long (Figure 5.1B). At both overpasses 2.5 m-high embankments have been constructed along the edges to prevent noise and light disturbance from the infrastructure below. Recreational co-use is allowed and facilitated through a semi-paved trail (width: 2.7 m) for cyclists and pedestrians and an unpaved trail (width: 1.6 m) for horse riders. Both overpasses were opened in 2006 and aim to restore habitat connectivity for wildlife species characteristic of mixed forests on nutrient-poor sandy soils and heathland (Van der Grift et al., 2009).

Study design

At both wildlife overpasses three track beds were installed, one in the middle and one at each end of the crossing structure. In addition, two camera traps were installed at the track bed at the eastern end of the overpass over the railway. This set-up made it possible to compare the results of crossings surveys with (1) the use of one track bed at the centre of the overpass versus the use of two track beds, one at each entrance of the overpass; and (2) the use of one track bed versus the use of camera traps (Figure 5.2). With regard to track beds, the approaches compared here are most common. The first approach with one track bed records animals halfway across the structure, and they can therefore be expected to continue and fully cross the structure. The second approach with two track beds provides better evidence for full crossings and simultaneously provides information on animal activity at the entrances, including activities that may not result in full crossings. With regard to the comparison of track bed versus camera traps, both methods are often considered when preparing evaluation plans, hence, information on their ability to register crossings is critical. The use of track beds is less sensitive to vandalism than are camera traps; however, track beds provide less information, usually require more man-hours and are more sensitive to weather conditions.

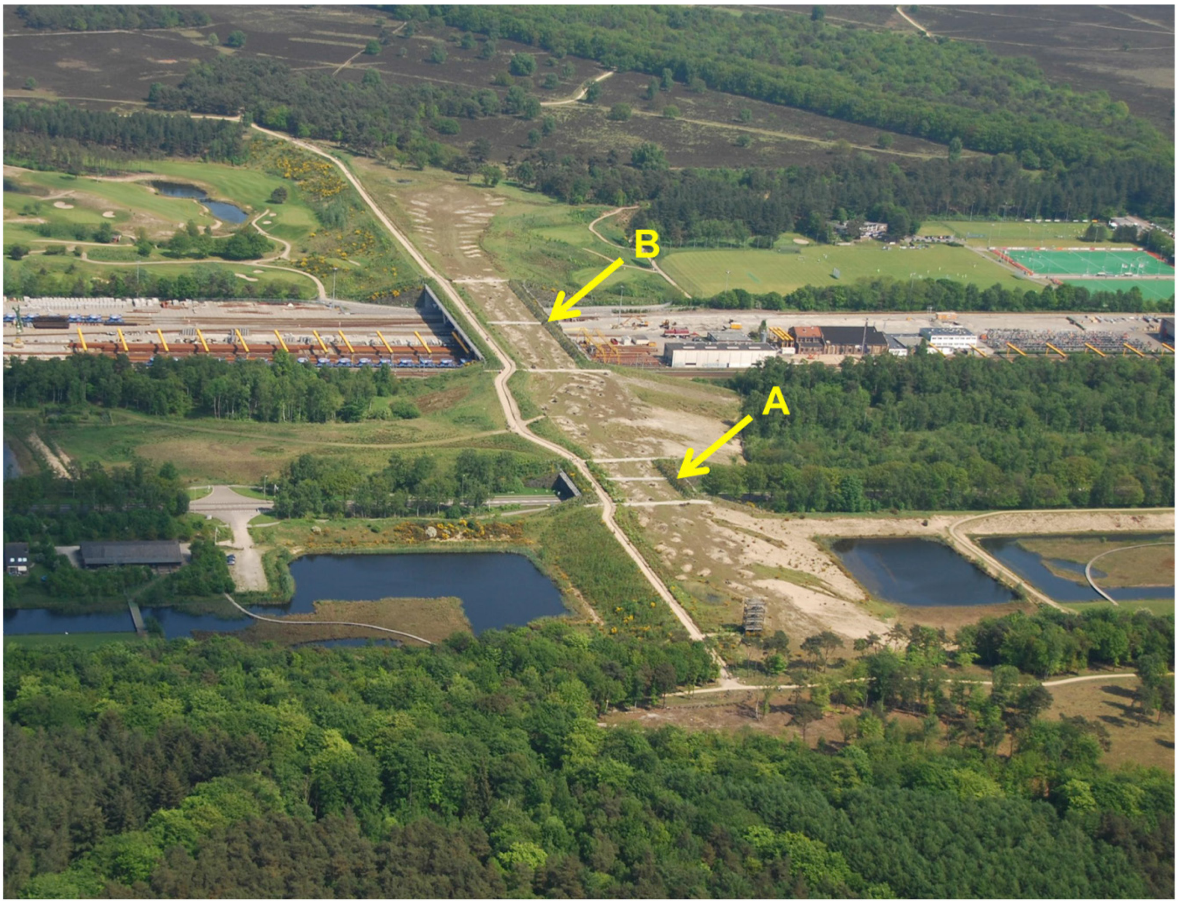

Figure 5.1 Wildlife overpass Zanderij Crailoo across highway N524 (A) and the railway Amsterdam-Hilversum (B) in the Netherlands. © Goois Natuurreservaat, the Netherlands / Photo: W. Metz.

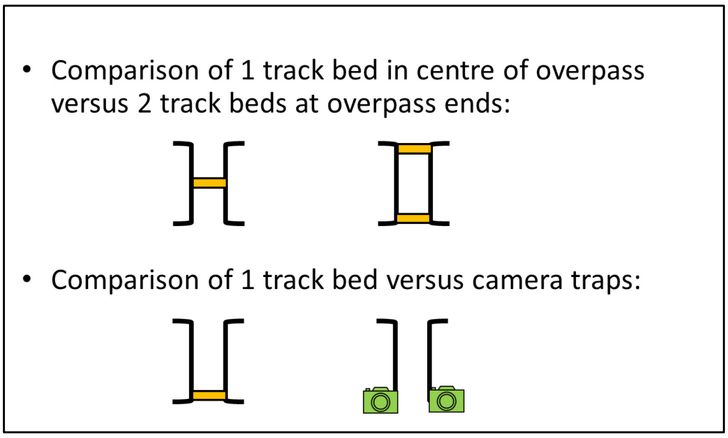

Figure 5.2 Schematic overview of the study design.

Data collection
The track beds at both overpasses were about 35 m long and 2.5 m wide (Figure 5.3A). They consisted of a 0.15 m layer of loam-poor sand. Impermeable root canvas was put underneath to prevent quick invasions of weeds. The track beds were surveyed for three months in the spring (April-June) and three months in the autumn (August-October) of 2009. The track beds were inspected five times a week (Monday-Friday). During an inspection we recorded all tracks of the target species, including the direction of movement (see also Van der Grift et al., 2009). After each inspection the track beds were raked to maximise readability during the next survey and avoid double counts. For camera traps we used Reconyx Silent Image Model RM30 cameras: digital cameras with an infrared illuminator and Passive InfraRed (PIR) motion detector. The field of view of the cameras is 40 degrees and their detection range is up to about 30 metres depending upon temperature and sensitivity settings. The cameras were placed about 0.6 m above ground level at each end of the track bed and facing each other (Figure 5.3B). Once a week the battery status of the cameras was checked and pictures taken were collected.

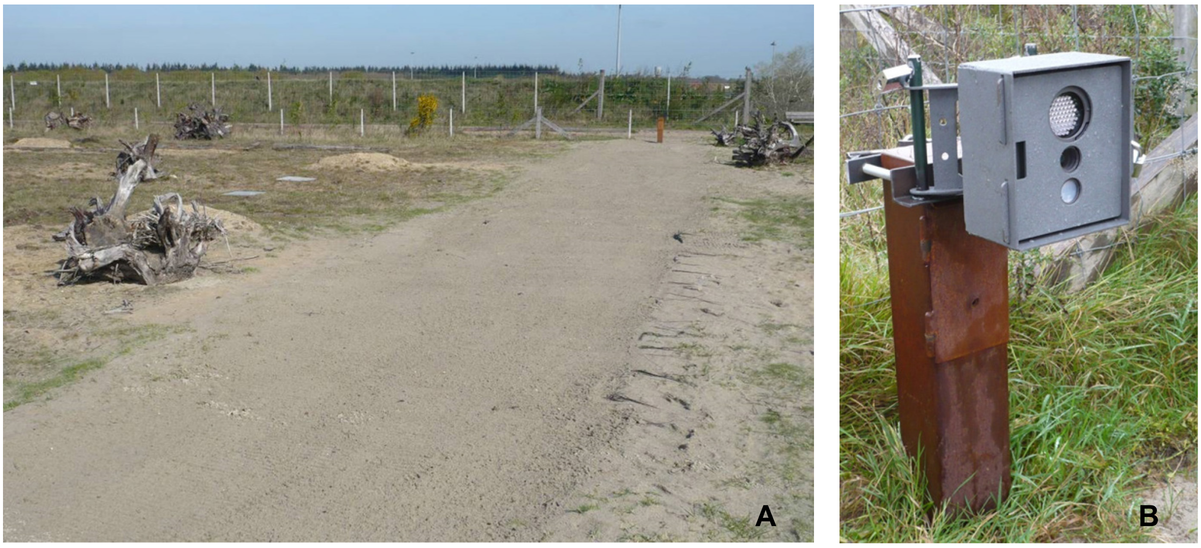

Figure 5.3 Track bed (A) and camera trap (B) at wildlife overpass Zanderij Crailoo in the Netherlands.

Data analyses
We selected three mammal species for analysis that differ considerably in body size: roe deer (*Capreolus capreolus*), red fox (*Vulpes vulpes*) and European hedgehog (*Erinaceous europaeus*). To compare one central track bed versus two track beds at the structure ends we summed up the number of tracks per species for each direction for the central track bed and we also summed up the minimum number of tracks per species for each direction that indicated a full animal crossing based on the two beds at the entrances of the structures. Survey days on which one or more of the track beds were unreadable (n=5) due to, for example, heavy rains during the previous night, were not included in the analyses. To compare track bed versus camera traps we summed up the number of tracks per species for each direction found on the track bed and summed up the number of crossings per species for each direction captured on camera. Survey days with an unreadable track bed or survey days with one or both cameras not working properly were not included in the analyses (n=13). We analysed the images from the two cameras simultaneously so that an animal crossing was only counted once, even if it was captured by both cameras.

5.3 Results

One versus two track beds
The estimated number of wildlife crossings based on two track beds (one at each entrance), was significantly lower if compared to estimates based on one track bed in the centre of an overpass (Figure 5.4). This applied to both overpasses. The mean differences in crossing estimates increased with the decreasing body size of the target species: roe deer 14%, red fox 29% and European hedgehog 61% fewer crossings based on two track beds, one at each entrance of a structure.

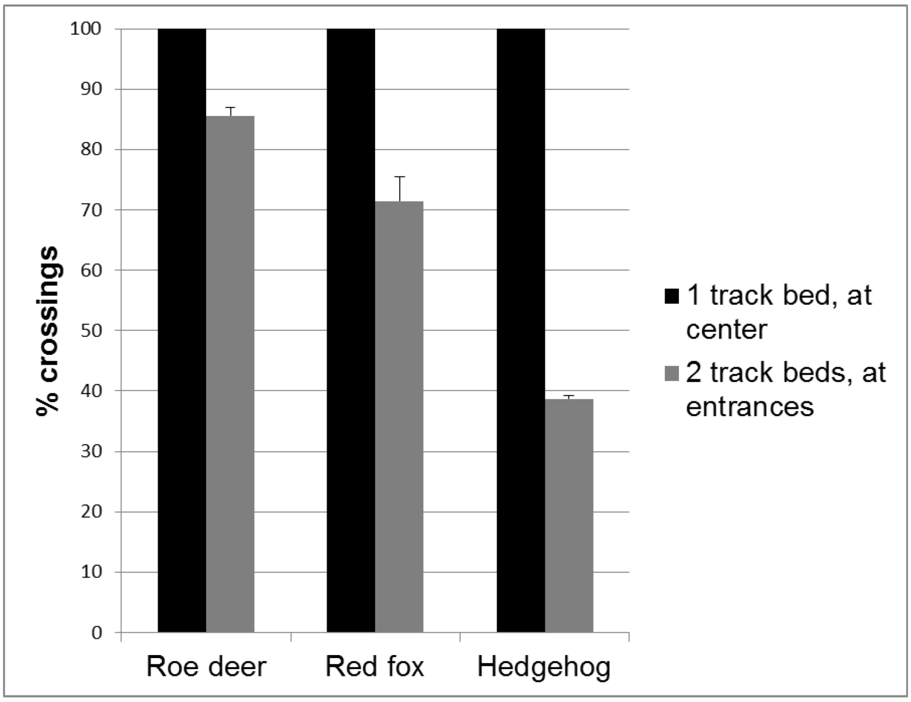

Figure 5.4 Percentage of crossings per target species detected by either one track bed in the centre of an overpass or two track beds at the overpass entrances. Error bars represent standard deviations (n=2).

Track bed versus camera traps
The estimated number of crossings based on camera traps was significantly lower if compared to estimates based on the track bed (Figure 5.5). Again, the differences in estimates increased with the decreasing body size of the target species: roe deer 11%, red fox 61% and European hedgehog 82% fewer crossings based on camera traps.

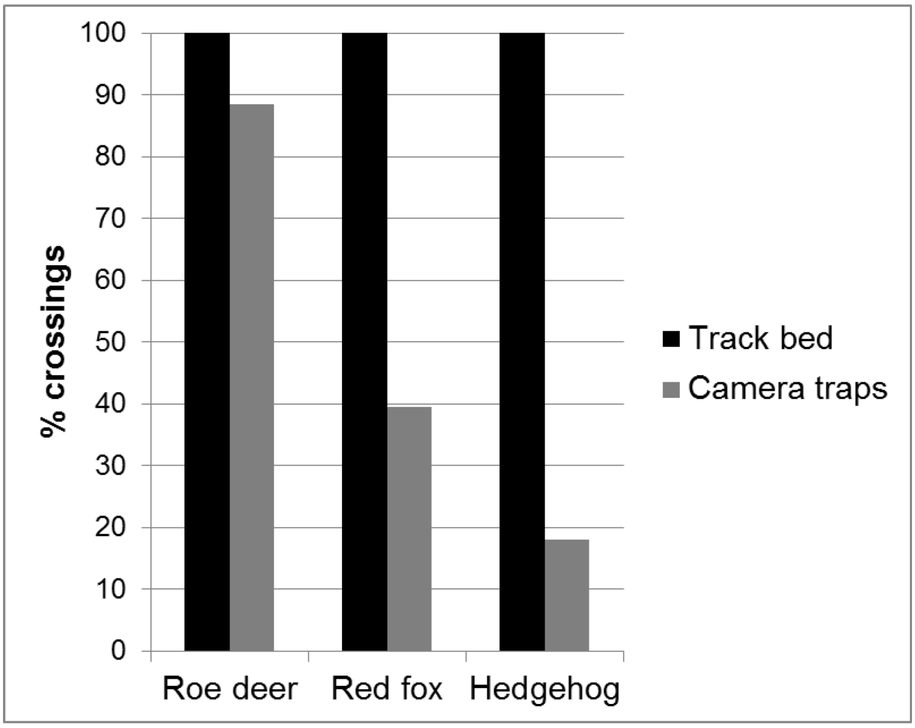

Figure 5.5 Percentage of crossings per target species detected by either one track bed or two camera traps at the entrance of an overpass.

5.4 Discussion

The reasons for lower crossing estimates if two track beds are used may simply be the result of the requirement that tracks should be detected on two instead of one track bed. As the readability of track beds highly depends on, for example, weather conditions and the number of animals that pass, tracks of crossing animals may frequently be detectable on just one of the track beds. Consequently, estimates based on two track beds are more conservative than estimates based on just one track bed. This is supported by the fact that, for all species, crossing numbers registered at the individual track beds at the overpass entrances are higher than crossing number estimates based on the rule that tracks should be registered on both track beds. The reasons for an increase in the differences in crossing estimates with the decreasing body size of the target species remain unclear. One explanation could be a decrease in detection probability of tracks from roe deer to hedgehog. Tracks could simply be more often overlooked in smaller mammals, which would result in lower crossing numbers if estimates are based on two instead of one track bed. A second explanation could be that animals frequently turn around after they have crossed the central track bed, and that such events occur more often in smaller mammals with smaller home ranges.

The differences between crossing numbers registered by track bed and camera traps can be explained by a significantly lower detection range of the cameras than suggested by the manufacturer. Animals crossing further away from the cameras were frequently missed, although we accounted for half the detection range that was provided by the manufacturer as the 35-metre-long track bed was covered by two cameras. For example, in the central seven metres of the track bed, 141 events of passing red foxes were registered while the camera traps detected only 8 events.

5.5 Conclusions

This case study illustrates that estimates of crossing numbers may differ significantly between survey methods, i.e. the use of one track bed, two track beds, or camera traps. The message here is not that two track beds are always better than one or that track beds are always better than camera traps, which is certainly not the case. Our findings merely illustrate that different survey techniques and differences in the way the techniques are applied (e.g. choice of track bed material, number of camera traps) may result in significantly different results so techniques should be rigorously compared and tested prior to the start of any evaluation.

Proper estimates of crossings can only be derived if survey methods are calibrated on the basis of true counts, such as those provided by video systems that record wildlife movements 24 hours a day and 7 days a week. The differences in outcome if different methods are used also implies that proper comparisons between data from different crossing structures - e.g., when assessing crossing structure features that impact the performance of the structures - can only be made if the applied survey methods are identical at all structures or if appropriate correction factors are used. The reliability of a method depends on a number of factors, some of whichcan be manipulated by the researcher, such as the frequency in which track beds are inspected and the type and number of cameras installed. More knowledge is needed on how such factors relate to the precision of the survey methods in detecting wildlife crossings.

6 Conclusions

The use of outcome-based specifications in procuring road mitigation works requires a new framework to evaluate road mitigation performance. Instead of merely technical inspections, more comprehensive evaluations are needed to assess whether or not the goals for road mitigation, described as functions, have been achieved. The framework presented here - consisting of ten guidelines - addresses all issues that are critical to successfully evaluating road mitigation performance. The examples - with the focus on amphibians and large mammals - illustrate how the guidelines may work in practice. Nevertheless, we recommend carefully testing these guidelines in real-life situations and adjusting them if necessary.

An evaluation of mitigation works based on the guidelines presented here will require more efforts and resources than most current approaches. On the other hand, such an evaluation will provide much more feedback on what we do right and wrong and is strongly linked to the reasons why the mitigation was designed, i.e., the mitigation goals. If these goals, in turn, are strongly linked to (inter)national legal and policy plans, the outcome of the evaluations will result in a better view on how the mitigation works contribute to overall strategies for biodiversity conservation. This will be even more so if the evaluation reports are standardised and empirical data – from both mitigation successes and failures - are stored in an open-access database. The acquired knowledge and data should not be the property of a contractor, but should become a public asset. This will allow a critical review by all stakeholders, better facilitate learning from the experiences of others and allow a more comprehensive meta-analysis that will help design better and more cost-effective mitigation.

Successfully evaluating road mitigation performance requires early and close collaboration between administrations and agencies, contractors and stakeholders. An independent advisory board, consisting of experienced road ecologists, may be an important asset as functional evaluations need to be scientifically sound. Similarly, hiring an independent contractor for the evaluation - who has not been involved in the planning and constructing of the mitigation works - will prevent conflicts of interests and allow selecting an evaluator based solely on their ecological knowledge and experience. Including road mitigation evaluations in the legal processes that must be followed during the road planning and procurement stages will ensure that the evaluations are done in time, that baseline conditions and reference standards are available and that the necessary financial resources have been secured.

The three case studies we have presented highlighted the need of a better understanding of ecological research methods as current knowledge among agencies and contractors may not be sufficient to make well-informed choices in preparing an evaluation plan for road mitigation works. The first case study showed that we should carefully select our study design and the spatial scale for data collection. Without controls it will be difficult to attribute a measured effect to the mitigation works. And certain positive effects at a mitigation site may be nullified by negative effects in adjacent areas, for example as a result of fence end effects. The second case study emphasised that all assumptions on the reliability of certain research approaches should be tested. The case showed that data collected during restricted periods may not result in outcomes that are representative of the full year. The third case illustrated that different survey techniques may result in significantly different results and that rigorous comparing and testing of techniques may be needed to provide representative and reliable data.

7 Acknowledgements

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