



# Wild Animal Initiative

P.O. Box 1866  
Long Island City, NY 11101

wildanimalinitiative.org  
info@wildanimalinitiative.org

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## Research Agenda — 2019

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## Introduction

Wild animal suffering is [massive in scale](#). The field is highly neglected, and methods for addressing it require research not only in biology and ecology, but also in the fields of social and political science. Much of the work we've planned 2019 and 2020 will help us better prioritize research in the future. If we were to be asked today about the highest priority welfare biology projects, we are, admittedly, fairly uncertain; no comprehensive effort to identify these projects has occurred. This gap in our knowledge base captures the essential direction of Wild Animal Initiative's work — a major strategic shift toward a twofold goal: first, *identifying and prioritizing* early welfare biology projects, and second, completing the heavy lifting necessary to *lower the costs* of early welfare biology.

As the movement to improve wild animal welfare grows, we have reached a point in our organizational development where talent exceeds available funds. This means that there is a significant need to carefully use our limited research capacity for the highest priority projects. Over the next two months, our research team will grow to be five staff strong. This team, to the best of our knowledge, will bring more expertise in biology and ecology to this space than ever before. We also believe that this research agenda, in conjunction with our [strategic plan](#), reflects the most ambitious effort to launch the field of welfare biology to date.

We encourage any individuals interested in this work to [support Wild Animal Initiative in completing this agenda](#) as we continue to develop welfare biology as an academic field and work to understand how to reduce the suffering of wild animals.

## Foundational Questions

Our research is guided by the following foundational questions:

*Which animals have welfares?* Before we begin to improve wild animal welfare, we must identify which animals are moral person or who have welfares. There is some [scientific consensus](#) that mammals, birds, and even some invertebrates, such as octopi, are likely to be conscious. However, this does not preclude other animals, including arthropods, zooplankton, and animals with physiologies very dissimilar to mammals and birds from also having welfares. Identifying which animals we should care about will help us get a better sense of the scale of wild animal suffering.

*What are the lives of those animals like?* As we identify which animals are morally relevant, we also need to learn more about what the lives of those animals are like. What negative stimuli do they face, and how frequently? What is the manner of their death? How long do they live? We aim to build a more accurate picture of the welfare of wild animals in order to prioritize interventions.

*What can we do to improve the welfare of those animals?* Once we have identified animals that face immense suffering, we must develop methods to improve their welfare, and tools to assess the impact of those methods. We also need to understand the long-term effects of our interventions on ecosystems and animal welfare, and also assess the cost-effectiveness of pursuing projects to improve wild animal welfare.

## **Research Agenda Development Process**

Addressing the above questions requires a significant amount of empirical information. In order to identify research projects that would be valuable for us to pursue, our first objective is to gather existing information and work to generate new information on these topics. Therefore, some of our projects focus on collecting and analyzing existing scientific research to address welfare-relevant goals, and others function to establish welfare biology as an academic field. In developing our agenda, we prioritized research projects that fulfill both these goals.

To identify appropriate projects, we contacted over 40 researchers and thinkers within the wild animal welfare space and asked them to suggest relevant, high priority research projects. We also worked with our internal research team to identify which of the proposed projects we may be particularly well-positioned to complete. Primarily, such projects will take the form of literature reviews, surveys, and prioritization research, rather than experimental scientific research. Where high-value academic research targets are identified, we hope to be able to direct funding to appropriate researchers to complete such projects.

## **Evaluation & Criteria**

Prior to selecting projects, we established four main criteria that would inform our decision to pursue a certain project. These criteria will also aid in prioritization efforts after project selection.

### *Leveragability and Prioritization for the Future*

Projects are more leverageable if they serve either to establish the academic field of welfare biology, or to move forward the state of research in that field. Projects get a good score in this category if,

for example, they are likely to reduce the time or cost of relevant future scientific research. One particular challenge for grant-makers in welfare biology is identifying which scientific research projects are highest priority, so projects that help identify valuable future work also score well on leveragability.

### *Scale*

Some projects apply to *all* animals, and others to specific species or populations. We prioritize projects that impacted many species, species with a higher number of animals (insects, fish, etc.), or species for whom the average member is likely to experience significant suffering (prey species).

### *Neglectedness*

We do not want to intentionally duplicate efforts in the early stages of this work, especially while resources are limited, and therefore we prioritize more neglected projects. We have requested feedback from individuals at similar organizations to identify risks of duplicated effort, and attempted to assess the possibility of this work being conducted in the near-future by non-wild animal welfare researchers. In evaluating the prevalence of other research topics in this space, we were able to determine if a suggested project was in a neglected area or not.

### *Tractability*

We want to pursue projects that seem likely to return useful conclusions, given the existing evidence, and so we have prioritized projects that appear tractable in this way. We also believe it may be the case that conducting some projects will help clarify more specific research questions for future work, even if the project itself doesn't produce any meaningful conclusions about a topic.

## Prioritization Between Criteria

Our work falls into two distinct categories — **near-term** and **long-term** outcomes for wild animals. Our *near-term work* primarily consists of assessing the cost-effectiveness of various proposed human interventions to reduce wild animal suffering, and assessing the impact of existing systems and human intervention on wild animals. Our *long-term work* is focused on establishing welfare biology as an academic field, prioritizing between welfare biology projects, and conducting foundational research in the wild animal welfare space.

In our solicitation of potential projects for the next year, we determined that many basic projects in this space have not yet been completed. Our agenda heavily emphasizes reviews of current literature on specific topics. We hope that these reviews will both decrease the cost of future welfare biology research by presenting an overview of current research, and bring unanswered questions to light.

Across our evaluation of project proposals, our team prioritized projects that we believe will efficiently guide us toward the most important early welfare biology projects. As part of this effort, we intend to develop a framework for prioritizing welfare biology research (in particular, field research and novel research that cannot be conducted simply through literature review), and a database of proposed welfare biology projects that we can encourage interested researchers to take on.

## Research Agenda

The following projects are those that Wild Animal Initiative will be initiating or completing in the next year. This agenda is a living document and will continue to be updated, as we develop our strategy. The projects are grouped into four categories (*Information Gathering & Literature Review, Intervention Research, Welfare Biology Field Development, and Social Change & Outreach*), but many projects serve multiple purposes in the wild animal welfare and welfare biology spaces. The Wild Animal Initiative [research agenda page](#) has been updated with current contacts for ongoing projects as a resource for anyone interested in getting involved in the work.

### *Information Gathering & Literature Review*

#### **Building a List of Welfare-Relevant Traits**

Which traits are welfare-relevant, and what is the current state of research regarding each trait? This work will likely inform some of our literature reviews and help identify areas where significant early welfare biology research is needed. In particular, the following sub-projects may help generalize the results of an improved understanding of welfare-relevant traits:

#### **Phenotypic Plasticity/Standing Genetic Variation in Welfare-Relevant Traits**

We aim to compile a list of traits that are likely to respond quickly via natural selection to habitat changes, including intentional or unintentional human interventions. This research will help us gauge the plausibility of different interventions, as well as potential indirect welfare effects of processes like climate change or habitat fragmentation (which may lead to greater brood sizes over investment in individual fitness, for example).

### **Taxonomic Heterogeneity of Welfare-Relevant Traits**

Following the development of a list of welfare-relevant animal traits, we also want to understand how heterogeneous the distribution of those traits is within and between taxa. This may help us understand the extent to which we can generalize about the welfare of animals with specific life history strategies.

### **Animal Age and Manner of Death**

We will review existing literature on animal death to determine what kinds of deaths are most frequent at different ages. Variations in the pain experienced at death may depend on the manner in which an animal dies. If there are significant differences in pain between manners of death, we may be able to identify welfare tradeoffs in life history strategies, especially if the most likely manner of death varies according to the age of the animal. This work could also inform high-priority welfare case studies.

### **Life History Strategies and Welfare**

We intend to review the existing literature on life history strategies in wild animals, and begin evaluating to what extent we can correlate them to welfare, and what research is still needed to make generalizations between life history strategies and welfare.

### **Trade-Offs and Inferring Animal Interests**

Animals often make trade-offs, in which they perform behaviors that cause some negative outcome in order to achieve a positive outcome. While trade-offs that animals choose to make [may not directly correlate to welfare](#), it is possible that these choices may lead us to have a better understanding of animal interests. We will conduct a review of literature of willingness-to-pay studies, which could help identify which stimuli, for example, are particularly painful for different animal species. Conducting this literature review early will clarify whether or not this topic area is worth pursuing. If there seem likely to be generalizable results, the review could also help identify key species for which additional field studies are required.

### **Wild Animal Numbers**

We will review existing evidence on [wild animal population size](#), and determine if there is a need to build and maintain a database on the number of wild animals. This would likely be broken down with greater specificity and by region. This project would only occur if we had evidence that suggested we could significantly improve and expand on previous work.

### **Testing and Refining Models of Animal Wellbeing**

We intend to loan some of our research time, in 2019 or 2020, to a proposed project that will develop models of animal wellbeing. This project will use a novel approach to animal wellbeing, and

will be a collaborative effort by economists, psychologists, neuroscientists, and evolutionary biologists.

### **Further Research of HANPP**

Persis Eskander has [previously evaluated](#) wild animal welfare considerations through the lens of *Human Appropriation of Net Primary Productivity (HANPP)*. Her evaluation indicated several areas of research that may inform us about the downstream effects of HANPP. We plan on assessing this area for further study, and recommending research projects on the basis of that assessment.

### **Insect Population Decline**

We will review the existing literature on the ongoing insect population decline, potential causes of that decline, and the scale of the decline. We will also review attempts to assess the downstream effects of these population declines. Given that many wild animal welfare advocates are particularly interested in the populations of arthropods, this population decline provides an opportunity to learn more about the downstream effects of a decreased insect population.

## *Intervention Research*

### **Downstream Effects of Wildlife Contraception Programs**

Wild-Animal Suffering Research previously conducted an [extensive literature review](#) of the effects of birth control and other contraception programs on the target wild animals. However, before pursuing contraception interventions, researchers must evaluate 1) whether or not contraception programs actually impact population sizes, 2) how ecological niches created by population decreases are filled, and 3) whether or not the contraception program has other downstream effects (such as increased hormonal birth control in food systems).

This research is directly relevant to efforts in reducing cat predation, like [Trap-Neuter-Release](#), and also informs the outcomes of work to reduce rodent populations in major cities. It will also provide some insight into the downstream effects of reducing populations using other means, such as with gene-drive policies.

### **Humane Insecticides**

WAI's major intervention research initiative will be in continuing the [humane insecticide research project](#), which we began in 2018. By December 2019, we hope to have recommendations for the substitution of specific insecticides that we are reasonably certain will cause less suffering for insects; or alternatively will understand the barriers to being able to make such recommendations.



Additionally, we will have a rough proposal of the costs of different types of advocacy programs to promote these substitutions and reduce insect suffering.

### **Cat Predation**

Currently, we are running a year long study of different [adoption-level advocacy](#) programs to update our cost-effectiveness estimates of methods to reduce cat predation. We also expect our literature review of the downstream or non-target effects of wildlife contraception programs to update our understanding of the cost-effectiveness of Trap-Neuter-Release programs. We will be conducting an analysis of the data collected in our [study](#) toward the end of 2019.

### **Gene-Drive Public Policy as an Advocacy Area for Wild Animal Welfare**

It is of our opinion that many future interventions to reduce wild animal suffering will involve genetic engineering-based policy. We will be conducting an assessment of the risks and opportunities for reducing animal suffering using gene-drives, particularly through public policy analysis and the regulation of gene-drive science.

### **Intervention Typology Review**

In 2018, Wild-Animal Suffering Research developed a comprehensive database of proposed interventions. We will be continuing this effort, as well as developing a classification system for those proposed interventions. Ideally, this system will allow us to develop tools for quickly estimating the cost-effectiveness of new interventions that fall within specific types, and determine whether or not they are worth assessing prior to proceeding with a project.

### **Intervention Resilience**

We will be conducting an initial evaluation of the frameworks on the resilience of human intervention policies. This will serve us in both helping to reduce the cost of future interventions (interventions that require constant maintenance will likely be significantly more expensive), and ensure that interventions withstand major events, such as value shifts or human extinction.

## *Welfare Biology Field Development*

### **Developing a Framework for Prioritizing Welfare Biology Projects**

We do not have a strong sense of which early welfare biology projects are the most important. We plan to develop a framework for assessing projects for future prioritization.

### **Developing a Database of Proposed Welfare Biology Projects**

As part of our academic field-building work, we would like to develop a database of proposed, high-priority welfare biology projects. This will serve as a resource for early welfare biology researchers on high priority directions for early field and lab work.

### **Environmental Impact Assessment and Downstream Effect Prediction**

We are still very uncertain about the downstream or non-target effects of efforts to reduce wild animal suffering. We plan to review the field of environmental impact assessment to learn about the extent to which it can improve our understanding of downstream effects.

### **Novel Ecosystems, Intervention Ecology, and Downstream Effect Prediction**

Related to the environmental impact assessment research, it seems plausible that the research fields of *intervention ecology*, *novel ecosystem ecology*, and *restoration ecology* may inform a research agenda for downstream effect prediction. We will review the existing literature in these fields and evaluate whether or not these fields are useful in our pursuit to improve the welfare of wild animals.

## *Social Change and Outreach*

### **Analysis of 2018 Utility Farm Messaging Surveys**

In 2018, *Utility Farm* conducted two [messaging studies](#) to improve the receptibility of outreach on wild animal welfare issues. In 2019, we will finish the analysis of these studies and publish them.

### **Outreach & Opinion on Welfare Biology**

As part of our academic field-building strategy, we plan on submitting letters to the editor, short reports, and opinions regarding welfare biology and improving wild animal welfare for publication in academic journals.

### **Prioritizing Wild Animal Welfare Within the Animal Advocacy Movement**

How much of a priority should wild animal welfare be within the broader animal advocacy movement? Considerations might be how many value-aligned researchers aren't being funded, how much need there is for work outside of direct research, and what the outcomes of wild animal welfare being a priority within animal advocacy would be.

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A 501(c)3 nonprofit based in New York City, New York

[www.wildanimalinitiative.org](http://www.wildanimalinitiative.org)

Contact: [info@wildanimalinitiative.org](mailto:info@wildanimalinitiative.org)