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Assessing the ecological effectiveness of protected areas in Cambodia: A quasi-experimental counterfactual estimation of avoided deforestation.

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Author's declaration

No portion of the work referred to in this thesis has been submitted in support of an application for another degree or qualification of this or any other university or other institute of learning.

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ABSTRACT OF THESIS submitted by:

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In the face of global environmental change, protected areas (PAs) have become an increasingly important tool in modern conservation, and as such there is a clear imperative to maximise the benefits they provide. In this regard a growing field of interest is the quantification of PA ecological effectiveness, often expressed in terms of avoided deforestation achieved relative to unprotected areas. However, such assessments are confounded by biases in both the non-random siting of PAs within landscapes as well as differential pressure upon their resources. These biases can be overcome by the use of quasi-experimental counterfactual study designs, that evaluate the impact of PAs against control areas of 'similar' biophysical and socio-economic characteristics. To contribute towards this knowledge domain this study presents an assessment of PA effectiveness for the Southeast Asian nation Cambodia, which, in light of its history of natural resource management, represents a pertinent case study. PA effectiveness was analysed using propensity score matching for three different outcome periods between 2010-2018 with the results finding significant positive treatment effects in each, with forested land in PAs being as much as 8% less likely to be deforested than similar unprotected forest. In addition to this a significant positive spillover effect of PAs was observed in 5km buffers zones adjacent to their boundaries, resulting in a maximum of 4% reduction in probability of deforestation. Furthermore, the effectiveness of PAs in Cambodia was found to vary under differential deforestation pressure as well as with regards to the duration of time since PA establishment.

Keywords: Protected area effectiveness, avoided deforestation, matching methods, propensity score, counterfactual, quasi-experimental, spillover effects, conservation

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List of Abbreviations

ADB	Asian Development Bank
AIC	Akaike Information Criterion
ASMD	Absolute standardized mean difference
ATE	Average Treatment Effect
ATT	Average Treatment Effect on the Treated
BACI	Before-After-Control-Impact
CBD	Convention on Biological Diversity
CCF	Community Conservation forest
CEM	Coarsened exact matching
CF	Community Forest
CLT	Communal Land Title
COC	Complement of the overlapping coefficient
COP	Conference of the Parties
CPA	Community Protected Area
CPP	Cambodian People's Party
DDF	Deciduous Dipterocarp forest
DPSIR	Drivers, Pressures, State, Impact, Response
ELC	Economic Land Concession
eQQ	Empirical quantile-quantile
ESIA	Environmental and social impact assessment
EU	European Union
FA	Forestry Administration of the Ministry of Agriculture, Forests and Fisheries of the Royal Government of Cambodia
FCA	Forest cover assessment
FCL	Forest Cover Loss
FLEGT	Forest Law Enforcement, Governance and Trade
GDANCP	General Department of Administration for Nature Conservation and Protection, Ministry of Environment, of the Royal Government of Cambodia
GD-PAME	Global Database on Protected Area Management Effectiveness
GFW	Global Forest Watch
GLAD	Global Land Analysis & Discovery lab of the University of Maryland
GLM	Generalized Linear Model
HARKing	Hypothesizing After Results are Known
HDI	Human Development Index
IFSR	Independent Forest Sector Review
IUCN	International Union for the Conservation of Nature
KS	Kolmogorov-Smirnov
LICADHO	Cambodian League for the Promotion and Defense of Human Rights
MAFF	Ministry of Agriculture Forests and Fisheries of the Royal Government of Cambodia
MDM	Mahalanobis distance covariate matching
MEA	Millennium Ecosystem Assessment
METT	Management Effectiveness Tracking Tool

MoE	Ministry of Environment of the Royal Government of Cambodia
MSB	Median standardized bias
NGO	Non-governmental organisation
NPASMP	National Protected Area Strategic Management Plan
NRM	Natural resource management
NTFP	Non timber forest product
ODC	Open Development Cambodia
PA	Protected area
PADDD	Protected area degazettement, downgrading and downsizing
PAME	Protected Area Management Effectiveness
PC	Principal component
PCA	Principal Component Analysis
PES	Payments for ecosystem services
PF	Protected Forest
PSM	Propensity score matching
RAPPAM	Rapid Assessment and Prioritization of Protected Area Management
RBC	Regression based conditioning
RDPA	Royal Decree for Protected Areas
REDD+	Reducing Emissions from Avoided Deforestation and Degradation
RGC	Royal Government of Cambodia
SAC	Spatial autocorrelation
SATT	Sample Average Treatment Effect on the Treated
SD	Standard deviation
SDM	Standardized mean difference
SE	Standard error
SEM	Structural equation modelling
SITA	Strongly Ignorable Treatment Assignment
SLC	Social Land Concession
SUTVA	Stable Unit Treatment Value Assumption
THPI	Temporal Human Pressure Index
UN	United Nations
UN FAO	Food and Agriculture Organization of the United Nations
UNCED	United Nations Conference on Environment and Development
UNDP	United Nations Development Program
UNEP	United Nations Environment Program
UNEP-WCMC	United Nations Environment Program - World Conservation Monitoring Center
VIF	Variance inflation factor
WCPA	World Commission on Protected Areas
WCS	Wildlife Conservation Society
WDPA	World Database on Protected Areas
WWF	Worldwide Fund for Nature

Thesis structure

In the interest of cogency, the first portion of this thesis has been divided into two chapters merging both introductory material and literature review. The first chapter (1) serves as a macroscale exposition of the field of protected area (PA) effectiveness assessment, focusing on its evolution alongside the global context of PAs and the different thematic areas and techniques encompassed within it. The purpose of this is to offer context to the form of assessment and methodology that will be employed by this study, namely a quasi-experimental, counterfactual, matching methods analysis of PA effectiveness in avoided deforestation.

The second introductory chapter (2) will present the case study that will be used for this analysis namely the Southeast Asian nation of Cambodia. This will highlight the rationale behind why such an assessment of PA effectiveness is pertinent on the basis of the socio-politico-economic changes that have occurred over the last several decades and the impact of these for the future of conservation in the country.

Following this, the thesis will observe the typical scientific convention, presenting sequential chapters on: the aims and objectives of the study (3); methodological and analytical techniques employed (4); results (5); discussion (6) and conclusions (7).

Introduction

1.1 The evolution of protected areas: Purpose and importance

The act of societies demarcating areas for the protection of biophysical assets on the basis of their value to either specific groups or the wider population has been occurring for thousands of years (Watson *et al.* 2014). The underlying impetus for the establishment of such protected areas (PAs) is summarized aptly by McNeely (1998, 189) as: “a cultural response to perceived threats to nature”, however the motivations, modalities and agents by which it has been realized have always been varied.

For example, some of the earliest recorded PAs come from the Sumerian civilization in the third millennium B.C and whilst their establishment was prompted by conservationist intent it was intertwined with religious mythology (Grove 1995). Other historical examples such as reserves established by the monarchy for the purpose of hunting in various European countries highlight the origins of the exclusionary nature of PAs that still persists to some extent today (Mulder and Coppolillo 2005).

Many consider the establishment of North America’s Yellowstone National Park in 1872 as the earliest example of a PA in the form that we typically associate them with, namely a nationally owned area managed under some intent for conservation (Heinen 2012). Whilst this was undoubtedly a pivotal development, in perspective it should be seen as only part of the totality of collective societal experience that has led us to the current situation of PAs as a fundamental component of the global conservation effort seen today.

The definition of PAs in the context of conservation has changed substantially over the last 50 years in line with the evolution of the dominant opinions surrounding their purpose (Naughton-Treves *et al.* 2005). These changes can be loosely characterized into different eras, starting with the ‘fortress conservation’ mentality of the early to mid-twentieth century whereby the purpose of PAs was focused very narrowly on species and habitat management (Brockington 2002). This approach is what Phillips (2003) dubs as the ‘classic model of PAs’ which because of its roots in colonialist ideology typically involved the exclusion of local land users, both physically and participatorily (*p 11.*). Due to macro-level trends in global societal development this mindset gradually diminished towards the end of the last century and conservation decision makers and practitioners steadily adopted a more inclusionary approach to PAs (Brockington *et al.* 2008). A substantial driver of this change was the recognition of the value that PAs have beyond being simply a refuge for species of declining populations, particularly the socio-cultural-economic values of PA land to local populations as well as the benefits of incorporating indigenous traditional ecological knowledge into PA management activities (Kothari *et al.* 2013).

In a sense these developments have resulted in a raising of the conservation 'aspirations' for PAs in the 21st Century (Watson *et al.* 2014: Dudley *et al.* 2018) which is reflected clearly in perhaps the most oft-cited definition provided by the International Union for the Conservation of Nature (IUCN) as: "a clearly defined geographical space, recognized, dedicated and managed, through legal or other effective means, to achieve the long-term conservation of nature with associated ecosystem services and cultural values." (Dudley 2008, *p. 8*).

The IUCN along with the World Commission on Protected Areas (WCPA; originally the National Parks Commission in the 1950's) have played a crucial part in this evolution of the purpose of PAs by not only cataloguing and synthesising knowledge from around the globe but also by advocating to keep PAs at the forefront of the conservation discourse (Galvin and Haller 2008). A pivotal moment in this work was the organisation of the First World Conference on National Parks in 1962 which set the stage for the United Nations (UN) General Assembly to announce that it would implement a process for the periodical review of the number and extent of the worlds PAs under the banner of the 'UN-list of protected areas' program (Chape *et al.* 2005). In 1981 this program was expanded into the World Database on Protected Areas (WDPA), which today is managed by the United Nations Environment Program - World Conservation Monitoring Center (UNEP-WCMC). With the WDPA data being publicly available through UNEP-WCMC's 'protected planet' web platform since 2010 (UNEP-WCMC 2020a).

A second crucial development that occurred in parallel to this was the establishment of clearly defined categories of PA management objectives. Early work on these took place as a joint effort between the IUCN and the WCPA throughout the 1970's with the intention of complementing and facilitating the ease of information gathering for the WDPA (Dudley 2008). Simultaneously they prompted further recognition that certain objectives of PA management are better suited to differing socio-political and institutional settings which is an important consideration in terms of planning the establishment or expansion of PA networks (Naughton-Treves *et al.* 2005).

These were finalized as the system of 'IUCN management categories' in 1994 (IUCN and UNEP-WCMC 1994). Further description this system is unnecessary in the scope of this introduction although readers should refer to Dudley (2008) for an overview. It is important to note that the IUCN categories did not achieve instant widespread adoption by national institutions, rather this is something that progressed slowly throughout the 2000's (Chape *et al.* 2005) and indeed many countries today do not explicitly categorise all of their PAs under the IUCN system (Heinen 2012).

Whilst this macro-level transition towards an expanded role of PAs as well as a more unified and consistent documenting of their activities can be attributed in part to social change processes, it must of course also be viewed as a product of the increased knowledge and recognition of the severity of global environmental problems that have developed over the past three decades (MacKinnon *et al.* 2020) amongst these in the minds of conservationists is the unprecedented rate of species extinctions observed over the last century (Elewa 2008). This has led many to proclaim that we are in the midst of an anthropogenic induced 6th mass extinction event, referred to as either the Holocene or the Anthropocene, which will possibly lead to a loss of 75% of all living species (Ceballos *et al.* 2015; Payne *et al.* 2016).

As a response there have been an expansive array of international policies and conventions declared to coordinate strategies to address biodiversity loss (Heinen 2012). Possibly the most well-known of these is the Convention on Biological Diversity (CBD) which was ratified at the United Nations Conference on Environment and Development (UNCED) in 1992 (Glowka *et al.* 1994). The most pertinent article of the CBD related to PAs was Article 8, under which encouraged all signatories to: "(a) Establish a system of protected areas or areas where special measures need to be taken to conserve biological diversity" and "(b) Develop, where necessary, guidelines for the selection, establishment and management of protected areas or areas where special measures need to be taken to conserve biological diversity" (UN 1992, 6). The goals of the CBD have been operationalized through 'strategic plans' issued for 2002-2010 (CBD 2002) and subsequently 2011-2020 (CBD 2010b). The latter quantified the desired extent of global PAs under its Aichi Targets, principally target 11, which called for formal protection of 17% of the earth's terrestrial surface and 10% of coastal and marine areas (*p. 6*).

Prior to this the conservation sector had already been witnessing a substantial expansion of the global PA estate over the previous 20 years. However, Aichi Target 11 and the ongoing reporting of UNEP-WCMC undoubtedly galvanised this effort (Geldmann *et al.* 2019). For context, Zimmerer *et al.* (2004) estimated that in 1985 only 3.48% of global land was under formal protection. Whereas as of April 2020 the WDPA contains a total of 245,133 PAs across 245 countries and territories, covering a total of 15.2% of terrestrial surface area and 7.4% of the marine area (UNEP-WCMC 2020d).

However, biodiversity loss is but one component alongside others such as climatic change, that collectively constitute the macro-scale phenomenon of global environmental degradation resulting from anthropogenic activities. In this regard, the last two decades have also seen increased acknowledgement of the potential of PAs to contribute to minimizing and mitigating other issues such as the loss of ecosystem services (Dudley and Stolton 2010: Watson *et al.* 2014; Melillo *et al.* 2015). The Millennium Ecosystem Assessment (MEA 2005) was pivotal in characterising these services, with Rodríguez-Rodríguez (2012) providing an apt summary of the diverse services provided by PAs specifically: "raw materials; food; genetic, medicinal and ornamental resources; water purification; air quality regulation; erosion prevention; mitigation of extreme events; pollination; biological control; carbon sequestration; soil formation; primary production; and nutrient cycling" *(p. 3)*. Furthermore Quintela *et al.* (2004) estimated that a global network of effective PAs could safeguard ecosystem services to the value of $38 trillion a year. Indeed, the operationalisation of this to benefit conservation is well underway through the implementation of payments for ecosystem services (PES) schemes specifically suited to PAs such as the UN's Reducing Emissions from Avoided Deforestation and Degradation (REDD+) program (Scharlemann *et al.* 2010; Soares-Filho *et al.* 2010).

Collectively, these developments make it clear that as a conservation intervention, PAs are more important than ever. This is exemplified by the fact that unlike some other environmental policies the establishment of PAs has, arguably, successfully spanned the global north/south divide (Kashwan 2017). Whilst PA degazettement, downgrading and downsizing (PADDD) is still prevalent (Mascia *et al.* 2014), no country has witnessed a net reduction in its total land area demarcated as legally protected between 1990 and 2017 (Kashwan 2017).

Importantly though, researchers and conservation practitioners alike have often highlighted that the rush to increase PA coverage around the world has come somewhat at the expense of addressing the crucial question of how successful PAs are at actually achieving conservation goals in real-world settings (Chape *et al.* 2005; Mora and Sale 2011; Watson *et* *al.* 2014; Eklund and Cabeza 2016). Part of the reason for this failure is the fact that answering this is problematic on many levels. Not only from the practical perspective of objectively devising metrics of evaluation (i.e. what to assess?) but also the conceptual issues around how to robustly identify success from failure (how to assess?). To this end, the following section (1.2) will provide an overview of some of the key themes and issues within the sphere of assessing PA effectiveness, before narrowing down to the specific method of assessment that will be employed in this study.

1.2 Assessing PA effectiveness

1.2.1 Why is assessment important?

The need for the broad-scale evaluation of PAs has, historically, been widely acknowledged and so too has the dearth of empirical techniques for achieving this goal (Kleiman *et al.* 2000; Pullin and Knight 2001; Sutherland *et al.* 2004; Ferraro and Pattanayak 2006). Beyond the realm of scientific literature it has been highlighted broadly in multi-national conventions as one of the main messages of the MEA (2005) and as a non-quantitative component of the CBD's Aichi target 11 (Adams *et al.* 2019) which is of increasing relevance given that the renegotiation of this target under the 2050 Vision of the current Strategic Plan for Biodiversity is soon approaching (CBD secretariat 2018).

This is not to say that progress within the field of PA effectiveness evaluation has not been made over the last decade, albeit relatively slowly (Eklund and Cabeza 2016). However, given the increased expectations placed on PAs, the risks they face due to the confluence of global environmental issues (described in the preceding section), and that conservation as a societal endeavor remains systemically underfunded (Balmford *et al.* 2003; Mansourian and Dudley 2008), then making further progress towards the creation and application of comprehensive methods for assessing PA effectiveness remains imperative (Chauvenet and Barnes 2016; Watson *et al.* 2016; Dudley *et al.* 2018; Coad *et al.* 2019).

Another factor that attests to an increased requirement for the assessment of PA effectiveness is that in many developing countries (who have often exhibited the most expansion in their contribution to the total global PA estate over the last quarter of a century: Naughton-Treves *et al.* 2005), the establishment and management of PAs has been largely funded by multi-national development (The World Bank; Asian Development Bank (ADB); United States Agency for International Development) and conservation organizations

(Worldwide Fund for Nature (WWF); Conservation International; Wildlife Conservation Society), or divisions of supranational bodies such as the United Nations Environment Program (UNEP) and Food and Agriculture Organization (UN FAO) (Mansourian and Dudley 2008). Whilst this is not inherently problematic a trend that has emerged is that donors are increasingly allocating funds on a conditional basis, i.e. dependent upon the achievement of pre-agreed conservation outcomes (Eklund and Cabeza 2016). The reasons behind this are manifold, chief amongst them is that the global financial crisis of 2007-2009 has resulted in decreased expenditure by both domestic and international development institutions (Caldecott and Jepson 2014). Whilst no recent efforts have been able to quantify the total annual global spending on PAs (Coad *et al.* 2019) it is likely that it does not meet the $76 billion annual requirement as estimated by McCarthy *et al*. (2012), which itself is now outdated given the expansion of designated PA extent since it was posited.

In addition to this funding organizations face increased scrutiny due in part to the expanded reach of the global media. This allows the publication of examples of ineffective use of funding, or worse, negative outcomes of PAs, to reach wider audiences and thus become even more damaging to their credibility and morale (e.g. the implication of WWF funded PA rangers in criminal activities reported on a global internet news agency: Warren and Baker (2019)).

Regardless of the explanation, the response is the same, donors are increasingly selective about what they fund and look to maximize the 'return' on their conservation investments and thus those advocating for PAs must provide more evidence of their effectiveness to justify continued or expanded expenditure (Lopoukine *et al.* 2012).

However, the positive implication of this is that there has been a renewed focus for conservation practitioners to develop viable alternative means of funding for PAs (Githiru *et al.* 2015). Progress has been made toward implementing examples such as PES schemes; debt for nature swaps; private-public partnerships; and biodiversity offsets (Mansourian and Dudley 2008). Nevertheless, it has been contended that these new forms of funding should not be used to displace responsibility from national governments and rather should be considered as temporary solutions (Pilgrim and Bennun 2014). Debate aside, most of these alternate modalities also inherently require rigorous assessments of the variables on which they depend, for example the income from PES schemes such as REDD+ is conditional upon the amount of carbon sequestration achieved within the project area and if these initiatives take place inside PAs this creates an explicit requirement for the evaluation of their effectiveness.

Beyond these unempathetic issues of funding there remains a moral obligation behind the assessment of PA effectiveness and their outcomes. This is because despite the paradigm shift towards a more people-centered approach to planning and management (Roe 2008), PAs are still incontrovertibility responsible for creating or exacerbating intra- and international conflicts over land rights and access to resources (West *et al.* 2006; Coad et al. 2008 Kashwan 2017;). Given that the complete mitigation of these negative aspects of PAs remains unachievable then conservation practitioners and planners must ensure that at the very least they are achieving other stated outcomes of biodiversity and habitat retention.

1.2.2 What to assess: Coverage, capacity or outcomes

One of the principle factors that has hampered progress in the evaluation of PAs is the ongoing discussion that bisects the realms of conservation policy, science and practice around what best constitutes a measure of PA effectiveness. The research around this subject has mirrored the evolution of the contemporary debate around the expanded purpose of PAs and as such there are some distinctions that can be drawn in terms of thematic areas.

Early work focused primarily around the effectiveness of PA planning and establishment through assessments of ecological representativeness and coverage, encapsulated within the field of gap analysis (Scott *et al.* 1993) and the broader domain of systematic conservation planning (Margules and Pressey 2000). Whilst the results of this type of analysis can be insightful (such as the seminal global study of Rodrigues *et al.* (2003)) the limitation of this approach as a measure of effectiveness is self-evident as the inclusion of a habitat or species inside PAs does not inherently decrease the probability that they will be conserved (Wang *et al.* 2013).

This prompted attention to instead move towards assessing the ‘effectiveness of implementation’ of PAs, more commonly referred to as assessment of Protected Area Management Effectiveness (PAME). As PAME represents a substantial field of study in its own right the overview provided here will be necessarily brief seeking primarily to reflect the extent of its application and conceptual shortcomings.

The origins of PAME assessments began in the 1990’s and whilst the methods employed by early examples were often devised *ad hoc* and independently (Hockings and

Philips 1999), a uniting feature is that they typically drew upon theory from the field of adaptive management (Holling 1978). Indeed, this was something that was perpetuated in the definitive framework for PAME assessments formalized by the IUCN and WCPA in 2000 (Hockings *et al.* 2004; Courrau *et al.* 2006). Although the framework is not a methodology of assessment in and of itself, it does provide guidelines on how to evaluate PAME by grouping indicators of effectiveness under the specific elements of the management cycle: "context, planning, inputs, processes, outputs and outcomes" (Leverington *et al.* 2010).

As the exact requirements for PAME assessment are still not fixed and indeed they have been expanded considerably over the last decade through the proliferation of methodologies such as RAPPAM (Ervin 2003) and METT (Stolton *et al.* 2007), it is helpful to visualize the process under general categories that influence the capacity to manage (Figure 1).

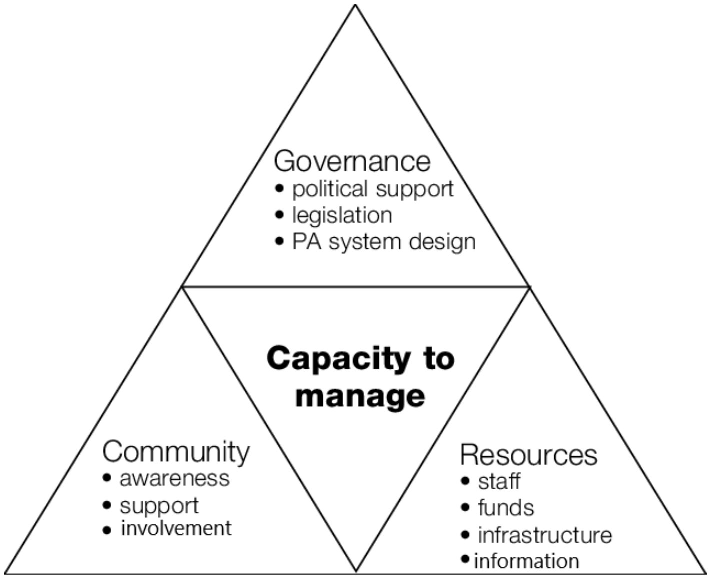


Figure 1: Components of protected area management capacity (adapted from Hockings and Philips 1999)

Of course, these are broad headings and, in reality, all consist of a plethora of components, for example assessments of 'staff' typically consider multiple factors such as training; motivation; retention; capacity levels etc.

Since its beginnings the application of PAME assessments has expanded considerably across the global PA estate. The mandate for this began with the ambitious commitment within the CBD Program of Work on PAs at the 7th Conference of the Parties (COP) in 2004 that all parties would actualize systems for monitoring, evaluating and reporting PAME, with assessments being completed for 30% of the total area of all PAs by 2010 and 60% by 2015

(CBD 2010a). As of 2013, Coad *et al.* (2013) found that considerable progress towards this target had been met with 29% of global PA area having been assessed and 23% of countries exhibiting over 60% areal assessment coverage. As of May 2020, the Global Database on Protected Area Management Effectiveness (GD-PAME) contains records of 27657 PAME assessments (UNEP-WCMC and IUCN 2020c).

Macro-scale reviews of the results of PAME assessments have been fairly consistent in the trends they have identified: the management of the majority of PAs ranks as "clearly inadequate"; with 13% of PAs lacking any evidence of management activities; and the weakest aspects being the community components and the funding of resources (Leverington *et al.* 2010; Coad *et al.* 2013). Whilst reviews also present evidence that PAME scores do improve with repeated assessments, critics have pointed out that the qualitative nature of the process, along with the fact that PAME scores have increasingly begun to be made a conditional requirement of PA funding (Eklund and Cabeza 2016) creates significant potential for bias or under/over-exaggeration of scores (Cook and Hockings 2011; Cook *et al.* 2014). In addition to this Anthony (2014) highlights a number of other critiques such as is issues of mutual exclusivity and ambiguity in commonly used surveys.

Regardless of this it is unequivocal that the propagation of PAME assessment has induced positive change in the effectiveness of PA implementation even it defies explicit quantification (Geldmann *et al.* 2015). Although at a conceptual level, the principal critique remains that successful implementation of conservation projects does not necessarily equate to successful conservation outcomes (Kapos *et al.* 2009).

This segues into the final lens through which researchers have sought to assess PAs namely through the effectiveness of their outcomes. As a field this has very much developed in tandem with assessment of PAME and indeed there is overlap between the two as many PAME methodologies do explicitly seek to quantify outcomes as part of the management cycle. However, by comparison the assessment of effectiveness of PA outcomes is both less researched and less widely implemented in applied settings (Eklund and Cabeza 2016). Part of the reason for this is the fact that there are numerous possible conceptions of PA outcomes in accordance with their broad range of objectives. For example, some studies focus on evaluating the socio-economic outcomes of PAs in terms of reducing localized poverty or improving human well-being (Andam *et al*. 2010; Clements *et al.* 2014; Oldekop *et al.* 2016). Alternatively, others have pursued assessments of reduction in anthropogenic pressure (Geldmann *et al.* 2019) or threats (Anthony 2008; Milatovic *et al.* 2019) as measures for successful outcomes. However, the majority of research has relied upon measures of 'ecological outcomes' that focus directly on the more traditionally perceived goals of habitat and biodiversity conservation.

The notion that ecological outcomes resulting from PA establishment and operation should be considered as a measure of their effectiveness is by no means a new idea, indeed Margules and Pressey (2000) established it as a key tenet within their conception of systematic conservation planning almost two decades ago. The question then is why has the implementation of this by PA management not been achieved at a significant scale?

In terms of biodiversity specifically the reasons for this are three-fold: firstly at a macro level devising methods to quantitatively capture elements of biodiversity is inherently difficult as it is essentially an abstract multifaceted concept and whilst this has been the subject of much research no single method has been comprehensively endorsed (Tucker 2005). This is interdependent upon the second reason which is that historically there has been, and continues to be, a large disparity between the abundance of data collected on certain biological taxa versus others which stems from not only a bias in conservation priorities (Western *et al.* 2009) but also the fact that some groups of species are difficult to observe and monitor. Finally, overarching these two explanations is the issue of cost as long-term biological monitoring programs are often unfeasible under limited PA budgets (Lindenmayer and Likens 2018; Coad *et al.* 2019)

Nevertheless, numerous studies have attempted to evaluate PA effectiveness on the basis of aspects of biodiversity, principally bird and mammal population data, with a review by Geldmann *et al.* (2013) highlighting no fewer than 35 separate publications, with Barnes *et al.* (2016) and Geldmann *et al.* (2018) being noteworthy additional examples given their more ambitious scales. However, the fact that few studies have chosen to focus on other measures of biodiversity indicates that the aforementioned problems of a paucity of data and the lack of unified metrics continue to limit progression in this form of analysis (Barnes *et al.* 2017).

As a result, much of the focus of research has instead been on quantifying PA ecological effectiveness in terms of their capacity to retain favorable habitat types or the antonymous measure of avoidance of undesirable land cover changes. The primary form that this has taken is analysis of PAs capacity to avoid deforestation or forest cover loss (FCL). Although some studies have opted to use different aspects of forest change dynamics, including forest gain as well as loss (Chai *et al.* 2009; Andam *et al.* 2013), forest degradation

(Blackman *et al*. 2017), carbon stocks (Scharlemann *et al.* 2010) or the prevalence of forest fires (Wright *et al.* 2007).

In comparison to assessing impacts of PAs on biodiversity these measures of habitat/land cover changes are not only easier to conceptually delineate, and thus quantify, but have also become increasingly feasible given the substantial improvements in the availability and quality of remote sensing data (Blackman 2013). Additionally, it is contended that the effectiveness of PAs in retaining desirable land cover also provides an implicit indication of their effect on biodiversity given prevailing theories on the relationships between species diversity and habitat types, as well as the effect of landscape connectivity on diversity (Gaston *et al.* 2008).

For these reasons there has been a considerable growth in the number of studies over the last decade that have analyzed PA effectiveness through permutations of forest related ecological outcomes. Overall, the consensus is that there is weak but positive evidence that PAs reduce deforestation in particular (Naughton-Treves *et al.* 2005; Gaveau *et al.* 2009; Geldmann *et al.* 2013; Nolte *et al.* 2013; Pfaff *et al.* 2013; Jones and Lewis 2015; Eklund *et al.* 2016).

However, studies in this domain exhibit pronounced differences with regards to the strength of causal inference between their chosen measure of ecological outcome and PA effectiveness. Given that this approach to PA effectiveness assessment is the one that will be employed by this study, an understanding of the causal models that can be utilized is crucial. For this reason, section 1.2.3 explains the underpinnings of causal inference and complications associated with it in assessments of PA ecological outcomes in more detail.

To summarize, there remains no unified means of assessing PA effectiveness although attempts have been made to propagate a more holistic understanding to bridge the gaps between the three strands of assessment described. For example, Eklund and Cabeza (2016) conceptually mapped the relationship between PAME and PA ecological effectiveness by adapting the European Environmental Agency's Drivers, Pressures, State, Impact Response (DPSIR) framework. The most comprehensive example though is the recently announced IUCN Green List Project (Hockings *et al.* 2019) of which the intention is to serve as a new global standard for PA effectiveness evaluation by requiring that PAs that seek inclusion on the list demonstrate success across four components: "Good governance, sound design and planning, and effective management, which work together to lead to successful conservation outcomes" (*p. 59*).

Whilst this is a promising development the creators acknowledge that it will require PA management to set “explicit ecological thresholds that represent success in conservation of their major values.” (*p. 60*) and yet the documentation provides scant additional details as to how this can be achieved in a comprehensive, reproducible and verifiable manner. This reaffirms that there remains a dearth of research on practical methods for assessing PA ecological outcomes and unless this knowledge gap is addressed it seems unlikely that projects like the Green List can meaningfully set a standard for PA effectiveness. On this basis sections 1.3 and 1.4 will highlight the progress and the specifics gaps that exist within this domain before a novel case study is conducted for Cambodia.

1.2.3 How to assess PA outcomes: Incorporating counterfactuals and biases

At the highest level of abstraction, assessing PA effectiveness through empirical outcomes typically takes the form of analyses of relational data (Blackman 2013). As alluded to in the preceding section such assessments often focus on varied outcomes which may be represented by either quantitative or qualitative data that is related in either spatial or temporal dimensions. Thus the *modus operandi* of such analyses is to examine the data for correlations or patterns by comparing across these dimensions in such a manner as to maximize the causal inference of the conclusions (Ferraro and Hanauer 2014).

This process shares many similarities with the evaluation of impacts in any given field, and as such, it is pertinent to apply some generalized terminology to ensure clarity. In this regard Blackman (2013) provides a useful glossary whereby the *unit of analysis* is the smallest element being analyzed which may be defined in spatial (areal) terms i.e. a sample plot or pixel in GIS representation of size *X* or by its anthropogenic designation such as an administrative area or single PA. The *treatment* is the condition or phenomena of which the impact is being investigated: in PA assessments this is typically the presence or establishment of the PAs themselves (i.e. units of analysis inside PAs are referred to as *treated units*). By contrast those units not receiving the treatment are referred to as the *controls*. The *outcome variable* is that which is used to quantify the impact of the treatment for example, in studies of PA ecological effectiveness, avoided deforestation is most often used as an outcome for which the proximate variable would be forest cover change. Hence the *outcome period* is the temporal duration over which changes in the outcome variable and hence the effect of the treatment is being analyzed.

Given the possibility of analyzing the outcome variable in either or both the temporal and spatial dimensions this means that numerous types of comparisons can be performed and again in the interest of clarity it makes sense to define and group these together. Firstly, it is possible to only compare the outcome variable within the treated group over time, for example by contrasting FCL rates within a protected area at two different time points. This is often referred to as a 'naïve' comparison as it only allows for conclusions with weak causal inference as any changes observed in FCL rates cannot definitively be attributed to the effect of 'protected status' of the unit of analysis (Ferraro and Hanauer 2014).

As an alternative to this, a counterfactual comparison is that which analyzes observed changes in the outcome variable between treatment and control groups. This can be performed in a spatial sense by identifying control units in proximity to, but not contiguous with, the treated units in what can be referred to as *with-versus-without* comparisons. Or this can be in a temporal sense where the treated units are analyzed both before and after the treatment is applied, referred to as *before-after-control-impact* (BACI) comparisons (Conner *et al.* 2016).

The usage of the term counterfactual stems from the notion that we can directly quantify the outcome of the treatment on the treated units but cannot observe the outcome on them in the absence of the treatment and, vice versa, we can quantify the outcome on the control units in the absence of the treatment but cannot observe the outcome with treatment. These unobservable outcomes are non-real (i.e. counterfactual) and, hence, must be estimated through the comparison of the quantifiable outcomes exhibited by the treated units and controls, the product of which is known as the Average Treatment Effect (ATE). This is an extension of the Rubin Causal Model (Ferraro and Hanauer 2014) which can be represented by the following equation:

Equation 1. $\mathrm{ATE} = E[Yi\,(1) - Yi\,(0)] = E[Yi\,(1)] - E[Yi\,(0)]$ (Ferraro and Hanauer 2014, 497)

ATE is the causal effect of the treatment on a randomly chosen unit (i) from a sample population of control and treated units. Where E is the expectation operator; $E[Yi\,(1)]$ represents the observed outcome in the presence of treatment and $E[Yi\,(0)]$ is the unobserved counterfactual. Whereas in the absence of treatment $E[Yi\,(0)]$ is the observed outcome and $E[Yi\,(1)]$ the counterfactual.

However, we are typically only concerned with the unobservable (counterfactual) outcomes for the treated units only as logically we want to know the effect of the treatment not of non-treatment. Thus, the measurement of the causal effect of the treatment is referred to as 'the average treatment effect on the treated' (ATT) (*p. 498*) which can be represented mathematically as the following:

Equation 2. $\text{ATT} = E[Yi\,(1) - Yi\,(0)|Ti = 1] = E[Yi\,(1)|Ti = 1] - E[Yi\,(0)|Ti = 1]$
(Ferraro and Hanauer 2014, 498)

This takes the same form as Equation 1. with the addition of '$|Ti = 1$' representing the condition that the unit i has been exposed to the treatment. This means that $E[Yi\,(0)|Ti = 1$ is the effect that the treatment would have had on the outcome for a unit not exposed to the treatment, again this is the counterfactual and as such $E[Yi\,(0)|Ti = 1 = E[Yi\,(0)|Ti = 0$ which represents the observable outcome for the control units (*p. 498*).

The fact that counterfactual comparison allows us to examine the difference in outcomes between the control and treated units not just at the level of individual units but also at the population level is one of the reasons that it results in greater causal inference as we are more certain that any observed differences are not heterogenous effects and indeed result from the treatment itself (Ferraro 2009). As a result of this counterfactual comparison has become substantially more commonplace in evaluations within the environmental sciences versus the naïve form of comparison of differences in treated units only (Schleicher *et al.* 2019).

However counterfactual comparisons are by no means a panacea in terms of the issue of causal inference as they often fail to adequately account for the influence of *control variables (covariates)* and *confounding variables (confounders)*. Control variables are defined as the factors that are expected, or have been demonstrated, to be correlated with **either** the outcome variable or the probability of a unit of analysis being assigned to either the treatment or control groups. Confounding variables are those that are likely correlated with **both** the outcome variable and treatment/control assignment. Both covariates and confounders clearly have the potential to introduce bias into the estimation of the ATT and they are represented in the formula as:

Equation 3. $\text{ATT}(x) = E[Yi\,(1) - Yi\,(0)|Ti = 1, \text{X}= x] = E[Yi\,(1)|Ti = 1, \text{X}= x\,] - E[Yi\,(0)|Ti = 1, \text{X}= x]$
(Ferraro and Hanauer 2014, 500)

Which takes the form of the standard ATT equation (2. above) with the addition of *x* representing a vector of all pretreatment covariates or confounders of the population and X the pretreatment covariates for unit *i* being a random vector of *x*. Hence from this equation it is clear that in order for t $E[Yi\ (0)|Ti = 1$ to be a valid counterfactual the value of X for the control units should be the same as the value for the treated units otherwise the estimate of the ATT will be affected.

In the case of assessments of PA outcomes, the influence of bias from covariates and confounders has been well documented, particularly with regards to the ecological outcome of avoided deforestation. The seminal studies on the subject were completed by Andam *et al.* (2008) and Joppa and Pfaff (2009) whose work is referenced and built upon by all subsequent counterfactual PA assessments (a review of which is provided in section 1.4). Both studies arrive at two key conclusions. First, PAs themselves are not placed randomly within the environment, i.e. the locations they are established in are biased along a suite of bio-physical and socio/politico-economic factors. Second, many of these same factors are significant predictors of the locations in which FCL occurs and thus there is a bias in deforestation pressure that exists across landscapes. The obvious implication of this latter point for the former is that different PAs will experience differential pressures on their resources based upon their location.

The nature of these biases is quite clear. The title of Joppa and Pfaff's (2009) study was "High and Far" referencing the fact the PAs tend to be located in areas of high elevation and on land of low value for agricultural or commercial use due to both environmental conditions and distance from roads and human population centers. As for the bias in FCL the opposite is true in that it is more likely to occur closer to roads and settlements, on land with higher productive potential and at lower elevations with less slope (Geldmann *et al.* 2019).

Of course, many other proximate factors beyond these have been posited and tested as both covariates and confounders (detailed in section 1.4.2) and indeed the same types of biases are also relevant when assessing other measures of PA effectiveness beyond avoided deforestation. For example, Eklund and Cabeza (2016) highlight that differential pressure on PA resources can confound evaluations of effectiveness using PAME data as a given PA may display good management but due to high pressure exhibit high resource extraction and thus appear ineffective and by comparison a poorly managed PA under low pressure may appear effective.

The general consensus is that any counterfactual comparisons (either *with-versus-without* or *BACI*) that fail to account for either or both of the types of bias highlighted above will likely overestimate the effect of protected status as a treatment (Ribas *et al.* 2020). This was demonstrated experimentally by Andam *et al.* (2008; 2013) who found that "naïve empirical methods" i.e. those that do not take biases into account, resulted in an overestimation of the ATT of protection (inclusion in a PA) by as much as 65%. Although contrary to this, Ren *et al.* (2015) found comparable results between a naïve random sampling approach and counterfactual bias inclusive approach in their study of the effectiveness of China's nature reserves.

The question of how to encompass the effects of covariates and confounders (a process referred to as 'conditioning') and thereby maximize causal inference has led to the development of a suite of techniques that constitute quasi-experimental approaches (Jones and Lewis 2015). In practical terms the purpose of these is to artificially select control groups that best minimize any differences in variance with respect to the covariates, essentially mimicking a random probability of assignment of a given unit to either the treated or control groups (Stuart 2010). In the simplest sense this is attempting to ensure that the only observable difference between the treated and control units is that the former has been exposed to the treatment, thus allowing for 'apples to apples' or *with-versus similar without* comparisons (Blackman 2013).

Critically though these methods must contend not just with the observable biases but also the presence of unobserved bias or hidden confounders, and the fact that some treatments can induce changes in the outcome variable within the control units themselves (spillover effects) (Andam *et al.* 2008). These techniques as well as issues associated with their use are the subject of detailed discussion in section 1.3.

1.3 Quasi-experimental techniques for assessing PA ecological outcomes

The term 'quasi-experimental' refers to the fact that observational data is being analyzed in an experimental context whereby the independent variable is controlled (the treatment) but the assignment of the units of analysis to treatment and control groups is non-random. The implication of this as described in the preceding section is that the techniques must condition (account) for the biases that are created as a result of this non-random assignment in order to get an accurate estimate of the effect of the treatment. This is achieved through the balancing (minimization of variance) of the covariates and confounders associated with each unit of analysis that are known or believed to be significant.

Much of the theory behind the design of quasi-experimental studies was developed in the domain of statistical research (Morgan and Harding 2006), and only over the last two decades have the techniques begun to gain prominence within the field of conservation impact evaluation (Jones and Lewis 2015) and the assessment of PA ecological outcomes in particular (section 1.4).

There are several families of techniques that are used for the conditioning of covariates and confounders in quasi-experimental studies: structural equation modelling (SEM); Bayesian inferential statistics; regression-based conditioning (RBC); and matching methods. Of these SEM and Bayesian techniques have seen the least application in the sphere of assessing PA effectiveness, with Brun *et al.* (2015) being the only published example of the latter. In contrast, studies utilizing either RBC or matching methods techniques are comparatively abundant (section 1.4). Thus, it seems pertinent to forgo further exposition of the two former techniques and instead provide a more detailed background and critique of the latter groups. Of course, it should be noted that the use of each of these techniques does not preclude the use of the other and indeed regression models are often employed in synergy with matching methods which will also be expanded upon.

1.3.1 Regression based conditioning

Conceptually speaking RBC techniques in a quasi-experimental context are those that incorporate the effects of covariates and confounders by first quantifying the relationship that exists between them and the outcome (dependent) variable for both the treated and control groups and then using this to adjust the estimated treatment effect (Rubin 1979). Given the scope of this thesis it is not possible to provide a detailed description of the extent of different RBC techniques that have been developed and, in that sense, readers should refer to Morgan and Winship (2015).

Instead it seems more pertinent to highlight some of the most commonly used examples with respect to quasi-experimental assessments of PA effectiveness which Blackman (2013) divides into three broad categories: simple; instrumental variable; and fixed effects regression.

Simple regression conditioning techniques can be characterized as those relying on various iterations of generalized linear models, for example logistic or probit models when the dependent (outcome) variable is dichotomous in nature (for example forest cover being lost or not) (Cropper *et al.* 2001), or multinomial logistic models for polychotomous outcomes (Mertens *et al.* 2002). Of course, the limitation to this approach is that if the model is mis-specified then it can have the opposite of the intended effect by increasing the bias on the estimated treatment effect (Stuart 2010).

By contrast the instrumental variables approach incorporates bias by quantifying the relationships of only the covariates that are predictors of being assigned to either the treatment or control and specifically do not predict the outcome variable for a unit except through the treatment (referred to as 'instruments'). Thus, for this to be a valid approach the instruments cannot be correlated with any unobserved confounders. This is not only hard to achieve from a practical perspective and indeed if it is then the results of such a conditioning only hold for the subset of units for which the assumption is met and hence the end result is not to the ATT but rather the local ATT only (Sims 2010).

Fixed effects regression conditioning is arguably the most effective of these RBC techniques because it utilizes panel data (repeated observations of the units of analysis over time) which allows for every unit of analysis to be included as a separate 'dummy' variable in the resulting regression model making it possible to quantify the unobserved variance both across the units and across time. However, in reality this is hard to implement as panel data for all covariates is rarely available prior to the treatment taking place (Wendland *et al.* 2015) i.e. PA management rarely collect historic data for sites that they intend to establish as PAs prior to doing so.

1.3.2 Matching methods

The development of matching methods, also referred to as statistical matching techniques, began in the 1940s (Stuart 2010) with most of the ground-breaking developments being made courtesy of Rubin and Rosenbaum in the 1970s and 80s (Rubin 1973;1974;1977;1979;1980; Rosenbaum and Rubin 1983; 1984; 1985a; 1985b). Historically this group of techniques has been less utilised in the environmental sciences than RBC (Ferraro and Hanauer 2014) although it is now seeing increasing use in diverse applications such as fisheries management (Costello *et al.* 2008), butterfly farming (Morgan-Brown *et al.* 2010), farm land abandonment

(Alix-Garcia *et al.* 2012), and evaluating payment for ecosystem service schemes (Arriagada *et al.* 2012).

The process of statistical matching involves the creation of a control group *ex post* by selecting control units that are as similar as possible (display the least variance) to the treated units across the range of covariates (Andam *et al.* 2008). Typically, the aim is to find matches (control units) for every single treated unit, although this is sometimes not possible depending on the restrictions of the technique employed.

In this regard matching can be conducted on an 'exact' or 'approximate' basis, with the former option requiring that covariate distributions of control units exactly match those of a given treated unit. Whereas the latter creates matches on the basis of which control units have the closest possible covariate distribution and hence is often referred to as 'nearest neighbour' matching. Logically, exact matching is the more robust approach with regards to minimizing variance between the treated and control groups, but it's use is limited as it becomes increasingly difficult to identify matches for larger numbers of treated units as well as with increasing covariate dimensionality (Imai *et al.* 2008).

Under approximate matching there are several different techniques that can be employed: propensity score matching (PSM); covariate matching; and coarsened exact matching (CEM). In terms of assessing PA outcomes the former two techniques have been widely applied whereas no studies appear to have used CEM, as such it does not warrant further description here, although for details readers should consult Iacus *et al.* (2011).

PSM was pioneered by Rosenbaum and Rubin (1983) and involves summarising the vector of covariate values for all observations using a regression model and then assigning each a scalar propensity score the based upon the probability of assignment to treatment. Treatment and control units are then matched based on the proximity of their propensity scores (Iacus *et al.* 2011).

Covariate matching is similar although the vector of covariates is calculated using a metric of multi-variate distance. The most commonly applied of which is the Mahalanobis distance (Rubin 1979) as it reduces the impact of any collinearity that may exist between the covariates (Abadie and Imbens 2006). Matches are then generated on the proximity between treated units and control units within the defined multi-variate space (also referred to as the variance covariance matrix).

Whilst it is possible to critique each of these matching approaches on an individual basis most of the common complaints are also cross-cutting with the RBC techniques and hence in the interest of cogency these will be presented in section 1.3.4. Irrespective of these, both Andam *et al.* (2013) and Ferraro and Hanauer (2014) outline specific rationales as to why matching methods are preferable over RBC techniques. First, they contend that matching and the results it generates are easier to communicate to non-scientific audiences who may lack in-depth statistical knowledge. Second, the fact that matching makes the researcher manually assess the resulting covariate balance forces them to acknowledge whether there is sufficient overlap between the treated and control groups, something that regression models, particularly those implemented 'blindly' through statistical software packages, can often fail to highlight (Stuart 2010). Most importantly, the researcher must attempt to achieve the best covariate balance before being able to view the estimated treatment effect, effectively guarding against the possibility of poor research conduct through the intertwined phenomena of 'P-hacking' (selective reporting of only statistically significant results; Head *et al.* 2015) and HARKing (Hypothesizing After Results are Known; Kerr 1998).

These endorsements are evidently influencing the research community as the number of studies implementing matching methods approaches to analysing PA ecological outcomes have been increasing relative to those using RBC alone (Appendix A: Table A1). Despite this there remains no concrete answer as to which particular method of matching should be used although several studies (Ho *et al.* 2007; Rubin 2007; Stuart 2010) simply suggest that the best method is that which produces the best covariate balance although as section 1.3.4 will point out, this is hardly the only consideration.

1.3.3 Combining matching and regression

Matching and RBC techniques are by no means mutually exclusive. In fact, many quasi-experimental studies have applied both separately to demonstrate the difference in the resultant estimated ATT (Ho *et al.* 2007), as well as in combination to increase the robustness of ATT estimates (Imbens and Wooldridge 2007). Combining the techniques typically takes one of two forms: either a regression model is used to test and refine the selection of covariates, as well as identify any outliers in the data prior to matching (Schleicher *et al.* 2017), or vice-versa regression is used to estimate the ATT post-matching (Ferraro and Miranda 2014; Bowker *et al.* 2017), with neither approach being demonstrably superior to the other.

1.3.4 Cross-cutting considerations and problems

In order to understand potential problems when implementing either RBC or matching methods, it is necessary to first detail the conceptual assumptions from which they arise. The first of these is known as the assumption of unconfoundedness (Rosenbaum and Rubin 1983) or conditional independence (Lechner 2002), and holds that the probability of a unit being assigned to either the treatment or control is independent (exogenous) of the outcome variable i.e. it is unconfounded with respect to the dependent variable (Abadie and Imbens 2006). The second is the assumption of overlap which holds that there is a positive probability of assignment to treatment or control for all values of the pre-treatment covariates or, put more simply, there is sufficient overlap between the distributions (variance) of covariates between the treated and control units (Abadie and Imbens 2006; Stuart 2010).

The original expounders of these assumptions, Rosenbaum and Rubin (1983), refer to them collectively as the assumption of “Strongly Ignorable Treatment Assignment” (SITA). However, it has since been acknowledged that violations of the SITA assumption, especially with regards to unconfoundedness does not constitute a critical flaw in analyses. Indeed, some authors have posited weaker versions of both of the underlying assumptions, such as Imbens (2004) who noted that in the case of estimation of the ATT the assumption of unconfoundedness should only be applicable with respect to the assignment of units to the control being independent of the outcome variable. Regardless, the SITA assumptions underpin numerous methods designed to ensure robustness at multiple stages of the quasi-experimental process. Details of these considerations with reference to assessments of PA ecological effectiveness are presented below, although it should be noted that whilst some of them are relevant under both RBC and matching designs (cross-cutting) some are applicable to only one in particular.

1.3.4.1 The requirement for sampling

The need to sample the total population of treated and control units in order to make a quasi-experimental analysis achievable will be determined by several factors: size of the analysis region; duration of the outcome periods; and size (resolution) of the units of analysis themselves. Assessments of PA avoided deforestation often utilize high resolution remote sensing data (e.g. 30x30m pixels) and hence even a region of analysis of a single country can lead to potential population sizes in the range of $1x10^6$ to $1x10^9$ units (Eklund *et al.* 2016).

Analyzing populations of this size using matching methods is rarely feasible without substantial computational resources especially given that the size of such datasets increases dramatically with additional covariate dimensionality (Blackman *et al.* 2015). Thus, sampling is a commonplace practice in these studies with some opting to use a simple randomized approach (Wang *et al.* 2013; Ren e*t al*. 2015; Abman *et al.* 2018). Whereas others have used random sampling with a minimum distance constraint to mitigate for spatial auto-correlation (discussed in detail below) (Bragina *et al.* 2015; Ament and Cumming 2016) and some instead using a systematic sampling approach (Beresford *et al*. 2013; Blackman *et al.* 2015).

Irrespective of the technique used an important consideration is the proportion of treated vs. control units that are represented in the samples. The rationale behind this is implicitly linked to the theoretical 2nd assumption of overlap that if either group is disproportionally represented then this increases the likelihood of there not being an overlap in covariate values (given the biases that exist for PAs specifically). Also, with matching methods a higher proportion of controls vs. treated is preferable as it can increase accuracy (Clements *et al.* 2014).

1.3.4.2 Validity of hypothesized covariates

Prior to the conditioning using either regression or matching there is a need to test the validity of the covariates that will potentially be included in the analysis. Often the preliminary selection is made on the basis of their prevalence in the wider literature and indeed it is important to recognize that any hypothesized relationships drawn from other studies may not hold true for the region under investigation especially if it is a novel one.

Covariates can be validated by quantifying their explanatory power using an appropriate regression model (Schleicher *et al.* 2017), or through other techniques such as: principal component analysis (Eklund *et al*. 2016), Pearson's r correlation analysis (Wang *et al*. 2013) or even algorithms such as 'Random forests' (Oldekop *et al.* 2016). At the same time such testing should also investigate the presence of any multicollinearity between the covariates which could be inflating explanatory power (Imbens and Wooldridge 2007). Additionally, the results can be used to demonstrate that sufficient overlap exists between the covariates of the treated and control groups in keeping with the 2nd assumption.

At a conceptual level testing the validity of the proposed covariates forces the investigator to question whether the data they have chosen adequately represents the phenomena it is intended to. This is particularly pertinent with regards to some of the well popularized covariates of PA avoided deforestation because the nature of the quasi-experimental analysis means that essentially time variant phenomena are being forced into a time-invariant context. For example, distance to roads is one of the most commonly used covariates (see Table A1: Appendix A) and in *with-versus similar without* comparisons this is typically intersected with other relational data of the units of analysis for the first year of the outcome period only. Hence, this ignores the fact that new roads may be built within the subsequent years of the outcome period and the excluding the potential influence of these on the observed outcome. Temporal partitioning of the data for such covariates can mitigates this effect to some extent but cannot eliminate it entirely. This is not to suggest that such covariates should be excluded from analysis although it is a potential explanation for why they may show poor or non-significant explanatory power in testing.

Overall refining the selection of covariates becomes a balancing act between defining a causal model with the best explanatory power whilst ensuring that the analysis remains computationally feasible. In this regard there are proponents who suggest that all potential covariates should be included as long as they positively contribute the model's power, criticizing those who use too small a set of covariates (Andam *et al.* 2008; Stuart 2010). Whereas others advocate that the selection should be refined to only those deemed to be the most important (Blackman 2013). Ultimately though the selection of covariates will largely depend on the subjective choices of researchers as well as the limitations of data availability. A final consideration is to test for the presence of unobserved or hidden covariates or confounders, again with respect to the 1st assumption of unconfoundedness (Stuart 2010). Existing studies primarily utilize Rosenbaum bounds sensitivity analysis to achieve this (Andam *et al.* 2008; 2010; 2013; Blackman 2013) although Ichino *et al.* (2008) present an alternative.

1.3.4.3 Quality of matching

As previously stated, the purpose of matching as an exercise is to create a control group with minimal differences in its covariate distribution with respect to the treated group (Ferraro and Hanauer 2014). In this regard there are a number of decisions that must be made as part of the matching process that can influence this. The first is whether or not to match using a caliper, i.e. a minimum possible difference in covariate distribution allowed for a match to be valid. This is often seen in examples of both PSM and covariate matching and is typically expressed in terms of a number of standard deviations (SD) away from either the propensity score or multivariate distance metric of the treated unit. For example, Clements *et al.* (2014) matched using the Mahalanobis distance with a distance of 0.5 SD's meaning that if the Mahalanobis value for a prospective control unit exceeds this value with respect to a given treatment unit then it is rejected as a possible match. Others have used more restrictive caliper values of 0.25 SD (Jones and Lewis 2015; Zhao *et al*. 2019).

A second consideration is the number of matches that should be found for each treated unit. Whilst the typical concept of nearest neighbor matching is to generate 1:1 treated to control matches, if this is not being done on the basis of exact matching of covariates (which, as was alluded to earlier, is rarely achievable) then it has the potential to obscure the fact that a treated unit may in fact have a large number of potential control unit matches that are very similar in their covariate distributions to it. Matching more than one control to each treated unit is often referred to as 'ratio matching' or '*k*-nearest neighbors (*k*:1)' and it's impacts in terms of the robustness of the ATE/ATT scores produced are still under debate. Stuart (2010) highlights that *k*:1 matching has the potential to both increase bias (as obviously all *k*>1 matches are less similar to the treated unit) but also reduce variance by leading to a larger matched sample size thereby 'smoothing out' the influence of individual covariates that might display more pronounced variance than others. Possibly the most revolutionary example of this amongst PA ecological effectiveness studies are those of Eklund *et al.* (2016) and (2019) who performed ratio matching on a 1:500 basis but allowed the matching of both treated to treated and treated to control.

A final consideration as part of the matching process itself is the decision whether to match with or without replacement. As the name implies matching without replacement means that each control unit can only be matched to one treated unit and at that point it is removed from the pool of potential matches for the remaining treated units. Matching with replacement is obviously the opposite and similar to the question of *k*-nearest neighbors there is no conclusive answer as to which option is most appropriate. Abadie and Imbens (2006) suggest that matching with replacement can generate higher quality matches (less variance) as every treated unit has a greater possible set of matches. Additionally Stuart (2010) highlights that this is particularly useful in circumstances where the total number of control units in the sample is low in proportional to the treated units, but also cautions that if care is not taken then the estimated treatment effect can be impacted if a relatively small number of controls end up in a substantial number of matches overall. Indeed, depending on the extent of this, it could be said that the ATE/ATT generated is in fact only a local treatment effect as opposed to being representative of the whole sample/population.

Of course, these considerations are without meaning if the actual quality of the matches generated are not tested post-matching. There are numerous methods of assessing the balance of covariates between the matched treatment and control groups: normalized difference in means (Wendland *et al.* 2015); mean differences in the empirical quantile-quantile (eQQ) plots (Brandt *et al.* 2015); median standardized bias (MSB) (Blackman *et al.* 2015); variance ratios (Austin 2009b); the Kolmogorov-Smirnov (KS) test (Greifer 2020b) and the complement of the overlapping coefficient (COC: Franklin *et al.* 2014). Each has its proponents and detractors and yet all remain widely used and hence it does not seem necessary to discuss their relative merits further.

Historically a lot of matching studies have utilized a wide range statistical tests to confirm the differences in these summary metrics following matching including: t-tests (two sample and paired), Wilcoxon signed rank tests, F-tests, chi- square tests, and the C-statistic (Austin 2009a; Clements and Milner-Gulland 2014b; Franklin *et al.* 2014; Ali *et al.* 2015). However, a number of methodological expositions have warned that many of these tests are in fact not appropriate for use in matching analyses, due to the fact that matching alters both sample size and structure (Ho *et al.* 2007; Imai *et al.* 2008; Austin, 2009a; 2011; Stuart, 2010; Thoemmes & Kim, 2011; Ali *et al.* 2015; Linden 2015). In the absence of such tests evaluating the differences in covariate balance achieved by matching becomes subjective upon the researcher although decisions can still be guided by setting thresholds to represent minimum requirements for improvements in covariate balance under different summary metrics (Greifer 2020b).

Importantly, the covariate balance is not the only thing that must be assessed in affirming the quality of matches. Another potential problem that must be considered is the phenomenon of spatial autocorrelation (SAC). This is the concept that variable values for a given unit are influenced or correlated with the variable values of units in spatial proximity to them (Mets *et al.* 2017). Obviously, the scale at which this occurs is dependent upon the specific variable and the resolution of the units of analysis themselves. For example, elevation is a covariate commonly used in PA ecological outcome studies, it is logical to expect that at a high resolution of 30x30m there is a strong likelihood that a given unit's elevation will be very similar to those that surround it as elevation is unlikely to show significant variability at this scale. However, the same cannot be said of all covariates as the extent of SAC can be difficult to define as it can be either exogenous (resulting from a another spatially autocorrelated variable such as rainfall determining agricultural productivity) or endogenous in nature (Gaspard *et al.* 2019).

The implication of SAC for matching methods approaches is that it increases the likelihood that treated units will be matched to controls in close spatial proximity to them. This is problematic in the case of 1:1 matching as the estimated ATT cannot be said to be representative of the whole population if, in spatial terms, the matched samples are from a relatively limited portion of the total region of analysis (Negret *et al.* 2020).

Regression based quasi-experimental designs are also by no means immune to the effects of SAC which can violate the assumption of independence (Lee 2013), cause an over-estimation of the effect of predictors (Brun *et al.* 2015), and possibly an inaccurate measure of statistical significance (Mertens *et al.* 2002; Mets *et al.* 2017).

As previously highlighted numerous studies have attempted to implement strategies to mitigate for SAC either through enforcing a minimum distance at the initial sampling stage or indeed doing the same during the matching process (Bowker *et al.* 2017). Fortunately, the extent of SAC can be quantified through several different statistical techniques: Moran's I; Geary's C; and by producing semi-variograms (O'Sullivan and Unwin 2010).

1.3.4.4 Spillover of treatment effects

Spillover is an additional consideration for quasi-experimental assessments that utilize spatial data although whether or not it is a concern is also dependent on the nature of the treatment itself. In the case of PA ecological outcome analyses it, is highly relevant as the treatment i.e. designation of an area as 'protected', has well-documented effects that 'spillover' to the surrounding *de facto* 'unprotected' land (Andam *et al.* 2008). These effects were encompassed within an additional conceptual assumption by Rubin (1980) referred to as the Stable Unit Treatment Value Assumption (SUTVA). The premise of SUTVA is that the outcome for one unit of analysis is not affected by the treatment assignment of any other units. With regards to the outcome of FCL the spillover effects of protection as a treatment can either positively or negatively influence the outcomes observed in control units in close spatial proximity to treated units. Positive spillover equates to reduced FCL in the surrounding area which can often be the result when the establishment of a PA generates economic benefits such as tourism which reduces the need for land conversion for agriculture (Gaveau *et al.* 2009; Wang *et al.* 2013). In contrast, negative spillover represents increased

FCL due to the displacement of clearing activities or even additional clearing by land users as an attempt to pre-empt the establishment of additional protected land (Pfaff and Robalino 2017). Regardless of the nature of the influence the presence of spillover effects violates the SUTVA assumption and risks generating a biased estimate of the ATT/ATE especially if other covariates are spatial autocorrelated.

Of course, it is possible to incorporate processes within quasi-experimental PA effectiveness analyses to quantify the spillover effects of protection. One way to achieve this is to construct buffer areas of a pre-defined width around all PAs within the region of analysis and then calculate the ATT for the treatment of being located inside the buffer as compared to control units outside the buffer (but not inside PAs). A significant ATT in this regard would suggest that there is a spatial spillover of protection occurring.

However, such a strategy raises an interesting question, what is the appropriate size of buffer to capture the spatial extent of spillover effects? So far testing of this has proved inclusive with some studies observing a positive treatment effect but no significant difference with respect to buffer sizes (Andam *et al.* 2008; Ota *et al*. 2020). Further adding to this complexity is that heterogenous spillover effects (both positive and negative) have been found to occur simultaneously depending on the context (land cover) and at different scales (Pfaff and Robalino 2012; Ament and Cumming 2016; Blanco *et al.* 2019).

1.3.4.5 Other considerations

Two final noteworthy considerations for quasi-experimental PA outcome assessments: First is the possibility of time-lagged treatment effects. Take, for example, the protection of a unit of land as a treatment and FCL as the outcome. Is it a valid assertion that the establishment of a protected area will have an impact on reducing FCL after a single year or is it more likely that any effects will only be observable after multiple years? Of course, it is impossible to generalize an answer for this as it is dependent on many aspects such as the capacity of management to implement law enforcement or the extent of the psychological effect of protected status with regards to those who may be engaging in forest clearing activities.

This practical consideration is linked to another under-researched, albeit more methodological, concern which is how best to quantify and propagate measures of variance and error as part of the quasi-experimental process. This is especially problematic with regards to investigations relying on spatial data as whilst some methods of land cover classifications of remote sensing data do specify error in terms of kappa coefficients, there is little consensus on how observational error should be measured for other spatial data or how it should be propagated when data layers are intersected (Devendran and Lakshmanan 2014).

There is also discussion around what measure of variance is appropriate for estimates of treatment effects generated from matching methods analysis (Imbens 2004; Schafer and Kang 2008; Austin 2009a; 2011; Stuart 2010). With some prominent theorists arguing that measures such as standard error (SE) are conceptually inapplicable, especially when matching is conducted with replacement which violates sample independence (Austin 2009a). To this end Abadie and Imbens (2006; 2008; 2011) devised a method to generate bias-adjusted estimates of SE for average treatment effects generated using matching estimators that has since been widely adopted. However, linking this to the observational error present in the data itself remains unexplored territory.

1.4 Reviewing counterfactual assessments of PA ecological effectiveness

The purpose of this section is to provide insight into the breadth to different approaches that counterfactual quasi-experimental assessments of PA ecological effectiveness have taken with regards to aspects of forest change dynamics. Given that a comprehensive review of previous studies in this domain is already offered by Geldmann *et al.* (2013), it does not make sense to re-review the literature they have already collated. Instead section 1.4.1 will provide a brief summary of their conclusions with the focus instead being on reviewing examples that have been published post-2013 or those that were not included by Geldmann *et al.* (2013). In this regard, a selection of the pertinent information (summarized using the terminology introduced in section 1.2.3) related to 37 additional studies has been included as a tabular Appendix (A: Table A1). Subsequent sections will highlight the common covariates employed in these studies (section 1.4.2) as well as critiques of the approach (section 1.4.3).

1.4.1 Summarizing results

Geldmann *et al.'s* (2013) review identified 76 counterfactual assessments of PA effectiveness under some measure of forest or land cover dynamics from 51 published studies. From these they highlighted several trends: there was a strong location bias in research efforts (35 examples were from Latin America); tropical forest was the type analyzed in almost all examples (67); 63 analyses relied on remote sensing data; and whilst the majority of studies made counterfactual comparisons to either buffer areas or similar unprotected land only 10 examples used matching methods to condition on biases. In terms of conclusions the authors noted that in 62 comparisons habitat loss occurred at greater rates outside PAs than inside them.

Although it is not feasible to concisely summarize the results of all the studies in Appendix A individually, reviewing those that were performed after Geldmann *et al.'s* (2013) synopsis, they largely corroborate with the former's conclusions. The majority concurred that PAs are generally more effective in producing a desirable outcome in terms of forest dynamics (be it avoided deforestation, increased carbon sequestration, etc.) vs. unprotected sites (Berefords *et al.* 2013; Carranza *et al.* 2013; Nolte *et al.* 2013; Pfaff *et al.* 2013; Vergara-Asjeno and Potvin 2014; Spracklen *et al.* 2015 Ament and Cumming 2016; Eklund *et al*. 2016; Bowker *et al*. 2017; Yang *et al.* 2019; Ota *et al*. 2020 etc.).

However, it is important to note that there have been a number of studies that have not concurred with this conclusion, either on the basis of observing contradictory results (Curran *et al*. 2004; Rayn and Sutherland 2011) or because the positive effect of PAs was too weak to be significant (Bragina *et al.* 2015; Wendland *et al.* 2015).

An interesting sub-set of these studies are those that have investigated the treatment of protection as not just a dichotomous difference between protected and unprotected but as a polychotomous variable testing the relative effectiveness of different PA management types (e.g. under the IUCN PA classifications), governance types (PAs vs. community forests etc.), and PA size or age (duration since establishment). With regards to PA management types there have been mixed results with some finding that PAs of supposedly stricter management are more effective in reducing undesirable forest changes (Naughton-Treves *et al.* 2005; Carranza *et al.* 2013), whereas others have concluded the opposite (Blackman *et al.* 2015; Brun *et al.* 2015) and interestingly Ferraro *et al.* (2013) found that the effects were heterogenous between different countries.

In terms of governance modalities, Schleicher *et al.'s* (2017) case study of Peru compared: "state PAs, Indigenous Territories (ITs), and civil society and private Conservation Concessions (CCs)" and found no difference between the effectiveness in terms of avoided deforestation as compared to logging concessions.

As for differences in PA size and age, again conflicting results have been observed with Bowker *et al.* (2017) finding that for their sample of African PAs, younger PAs were more effective than older and similarly smaller PAs were effective than larger. However, by contrast Zhao *et al.* (2019) did not observe a significant relationship between PA age and effectiveness.

Another area of interest within this domain is the studies that have tested for the presence of a relationship between PA ecological effectiveness and other measures of PA effectiveness such as PAME scores. Given the management activities captured by PAME assessments, logically we should expect a positive correlation between management effectiveness scores and avoided deforestation although thus far evidence of this has been inconclusive (Nolte & Agrawal, 2013; Carranza *et al.* 2014; Eklund *et al.* 2019).

Finally, even though it did not investigate land change dynamics as a direct outcome, an additional study of note is that of Geldmann *et al*. (2019). This is partly because it claims to be the largest study of PA effectiveness to date by analyzing over 12,000 PAs from around the globe. The authors used a novel metric the Temporal Human Pressure Index (THPI), consisting of combined data on human population density, land transformation and electrical power infrastructure, as the outcome variable. They concluded that overall PAs had not reduced human pressure relative to matched controls over a 15-year period. Although once they separated between forested and non-forested PAs (on the basis of those used by previous studies) they observed that whilst forested PAs displayed increasing human pressure over the outcome period this was significantly less than in matched un-protected areas.

1.4.2 Covariates and confounders of PA effectiveness

Table A1 (Appendix A) shows that there is a core selection of covariates that have been used in a large number of studies and indeed this is further evidenced by the 51 studies reviewed by Geldmann *et al.* (2013) that were not included in this table. These include distance to roads; distance to population centres; population density; slope; elevation; proxies for surrounding habitat cover (distance to forest edge); and indicators of agricultural suitability (soil type and quality; temperature precipitation). Although it is important to note that not all studies have been unanimous with regards to the direction of the relationships between these observed variables and the treatment or outcome. For example, Bowker *et al.* (2017) found the opposite than expected relationship between accessibility and deforestation pressure (outcome) in their matching methods study in Africa, namely that more inaccessible area's (as defined by elevation, slope and distance to cities) had higher deforestation pressure. This they attributed to the fact that greater inaccessibility could correlate with lower levels of law enforcement and that in their case 'distance to cities' was perhaps not adequately reflecting local population density.

By contrast, some lesser used but nonetheless interesting covariates are 'proximity to international borders' (Joppa and Pfaff 2009) and proximity to land of other tenure types (numerous studies test for relative differences in outcomes between these but few include the spatial proximity of them as a covariate; Blackman *et al.* 2015).

1.4.3 Critiques of forest dynamic based assessment of PA effectiveness

The foremost critique of using ecological outcomes as a proxy for PA effectiveness is that while minimizing FCL should be a clear objective for management in PAs that exhibit substantial forest cover, the responsibility of management to maintain land cover in non-forested and partially forested PAs is much less clear (Geldmann *et al.* 2019). Whilst it would be a reasonable assertion that the goals of these PAs should be to prevent the conversion of natural land cover to 'unnatural' (i.e. anthropogenic use) this is complicated by the question of what constitutes 'natural' cover. In fact, in some cases it may be appropriate for management to actively convert forested land to non-forested by, for example, removing non-native tree species to allow for the restoration of natural wetlands.

Another criticism highlighted by Brandt *et al.* (2015) is that if PAs effect on forest dynamics is viewed at too coarse a distinction, i.e. only focusing on total FCL, this can obscure some of the heterogeneity in impacts that may be occurring. For example, the conclusion of their study in China was that there was no significant difference in total forest cover retention by protected vs. control areas. However, when forest types were separated out there was a significant difference with respect to old-growth forest which is arguably one of the most important varieties from a conservation perspective.

Looking beyond the practical aspects of forest dynamic based measures, an important conceptual critique of using them as a proxy for PA effectiveness is that the greatest potential for achieving avoided deforestation is in regions where deforestation rates are high. This is problematic from a cost-benefit perspective considering that the cost of achieving avoided deforestation in areas of high pressure is likely to be high and the overall conservation benefits relatively low in comparison to securing the same amount of avoided deforestation across numerous locations facing lower pressure. Hence focusing on those PAs that appear to be the most effective by virtue of having achieved the most avoided deforestation is perhaps misleading in terms of where limited funding should be allocated (Vincent 2016). Of course, the counter argument to this is that it would be possible to normalize effectiveness estimates across relative deforestation pressures and analysis on the basis of total spending to see which PAs have achieved the most (relative) gains at the lowest expense.

1.5 Chapter conclusions

To draw together the most salient points from this chapter, it is abundantly clear that PAs have a more important role to play than ever in terms of addressing the issues of global environmental change. To maximise the utility of PAs more research is needed to develop measures of assessing their effectiveness. In this regard the ecological outcomes of PAs, quantified through avoided deforestation is an emerging field. In order to produce valid assessments of this outcome, biases in both PA locations and deforestation pressure must be accounted for using quasi-experimental counterfactual study designs. Within this domain the use of statistical matching methods has become increasingly popular and is often favoured over regression-based techniques for intuitiveness and ease of communication. However, many aspects of the methodology of matching have not yet been adequately explored when applied to assessing PA outcomes. For example, further analysis to examine the spatial spillover of treatment effects; techniques to mitigate for SAC; and particularly other factors associated with PAs such as size and age that may influence effectiveness. Additionally, Ament and Cumming (2016) highlight that numerous studies have focused on too large a spatial extent (global or regional) and sometimes miss the dynamics of forest cover change and PA effectiveness that occur at more localized scales. These are clear areas that this study hopes to build upon as part of the continued propagation of quasi-experimental techniques to ultimately address the need for better causal inference in assessments of PA effectiveness in general. This will be achieved through the application of a nearest neighbor matching approach to estimate avoided deforestation of PAs in Cambodia, the rationale for which forms the basis of the following chapter.

2. Cambodia: A case study of PA effectiveness

The purpose of this chapter is to demonstrate why Cambodia represents a suitable case study for a quasi-experimental analysis of PA effectiveness expressed in terms of avoided deforestation. In this regard it is important to first identify the patterns of natural resource use and environmental change that have impacted conservation efforts within the context of the socio-politico-economic circumstances of the country's recent history (section 2.1). Following this section 2.2 will detail how PAs in Cambodia have evolved within this context, their importance in terms of the country's remaining resources, prior assessments of their effectiveness and the current events and emerging trends that have implications for them in the future. These strands are then synthesized to present a clear rationale for how an assessment of PA effectiveness could provide relevant country-specific insights for conservation practitioners and policy makers (section 2.3).

2.1 The context of environmental conservation

In the period from 1990 until the present-day Cambodia has exhibited substantial environmental change. Similar to other surrounding Southeast Asian nations, such as Thailand and Vietnam, this has principally been in the form of changing patterns of land-use particularly exemplified by the clearing, conversion and degradation of primary and secondary forest cover (WWF 2013). Before quantifying the extent of this change (section 2.1.3), it is important elucidate the mechanisms or drivers by which it has come about. In this regard section 2.1.1 will first adopt a socio-ecological system approach to framing these causal factors at the macro-scale. Following this section 2.1.2 will contextualise the drivers within a narrative of historical/events and policies that can be said to characterise natural resource management 'eras' within Cambodia's recent history.

2.1.1 Characterizing drivers of environmental change

Figure 2 below summarises the main drivers of environmental change that have been posited for Cambodia in both a proximate and ultimate capacity as highlighted by a range of sources (De Lopez 2002: Amariei 2004; Broadhead and Izquierdo 2010; EU 2012; Forest Trends 2015; JICA 2017; and Kong *et al.* 2019) and placed into the schema developed by Geist and Lambin (2002). Many of these drivers are those typically displayed by most similarly agrarian countries undergoing development, such as urbanization or and development of a cash crop sector. With others being the result of larger macroscale phenomena such as climatic change (e.g. changes in rainfall). Although in the case of Cambodia there is one particular 'ultimate-proximal' causal relationship that has been disproportionately responsible for the countries pronounced environmental change over the last four decades. Namely the commodification of state land used as a means to perpetuate natural resource exploitation primarily focused upon the country's forests (Beauchamp *et al.* 2018).

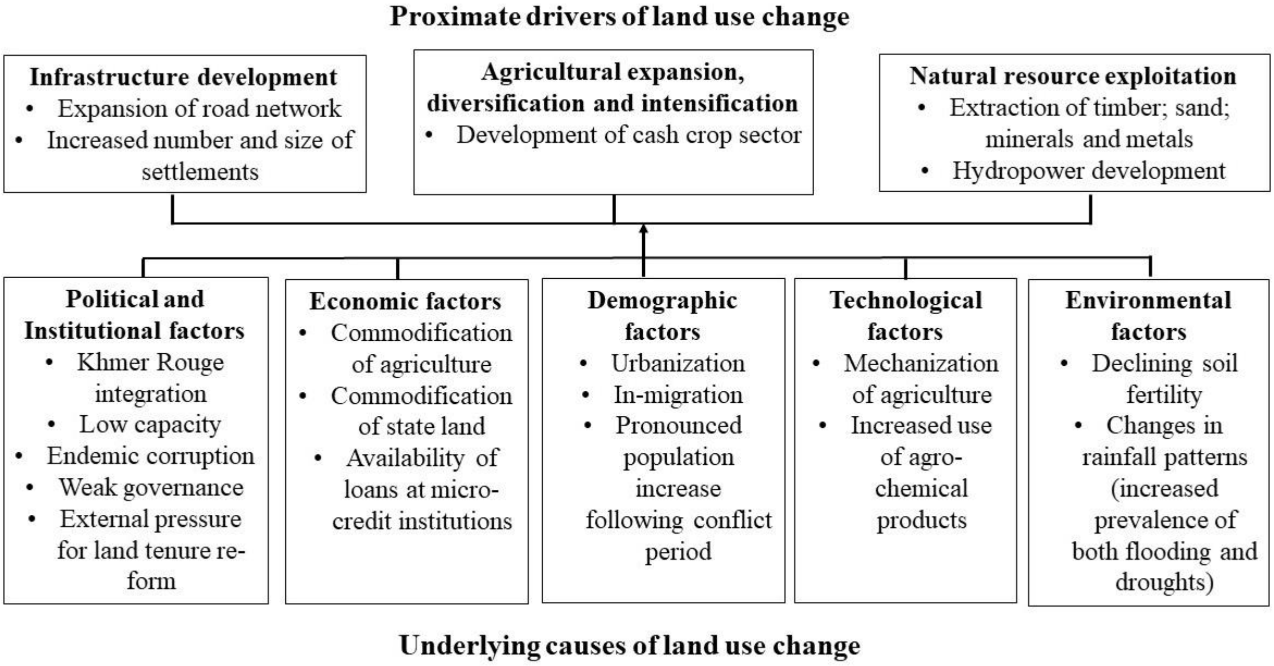


Figure 2: Underlying and proximate causes of land-use change in Cambodia
(Schema adapted from Geist and Lambin 2002)

The exact mechanism by which this has occurred has changed over time and can be loosely characterised into different governmental policy eras of natural resource management that have taken place between the early 1990's and the present day. As these different eras each had particular implications for the country's PAs they are discussed in more detail in the subsequent section (2.1.2). However, a defining feature that transcends them is that whilst the purported aim of natural resource exploitation has always been national development the process has consistently been subverted to benefit an entrenched societal-political elite class (Le Billon 2000; De Lopez 2002; Global Witness 2007; Un and So 2009; Cock 2010; 2016; Baird 2014; Milne and Mahanty 2015). This phenomenon has been facilitated and perpetuated by a combination of weak governance (Un and So 2009), a pervasive culture of institutional corruption (Global Witness 2004; Hill and Menon 2013), and the absence of rule of law (Ghai 2009). Which has resulted in some describing the country's governmental system of the last three decades as being little more than a kleptocracy (Global Witness 2009), that has exploited national resources to support a neo-patrimonial authoritarian regime (Cock 2010; Karbaum 2011; Un and So 2011; Milne 2015). At this point it is worth noting that over this period the Royal Government of Cambodia (RGC) has been comprised from the same political party: the Cambodian People's Party (CPP), led by Samdech Hun Sen as the prime minister (Diepart and Sem 2015).

Aside from environmental degradation, elite capture of natural resources in Cambodia has resulted in myriad negative socio-economic impacts. In 2013 the Cambodian League for the Promotion and Defense of Human Rights (LICADHO) estimated that a minimum of 420,000 rural Cambodians had been directly affected by land grabbing or related conflicts based upon their case studies since 2003 (LICADHO 2014). This is corroborated by the fact that by 2009 an estimated 45% of the country's total land area had been handed out to private investors (Global Witness 2009) whilst amongst the general population there was a growing trend of landlessness (20% in 2009, with an estimated 40% of rural households owning farmland less than 0.5 ha) (Üllenberg 2009).

Additionally, in economic terms, the fact that the majority of resource exploitation has been allowed to occur in an unformalized and unregulated manner, has meant that it has demonstrably failed to generate proportionate revenue for the state (Biddulph 2011). For example, between 1991 and 1998, Cambodia exported at least USD 2.5 billion worth of timber, although from this only 12% ($120 million) of the total public revenue due through taxation was collected (Le Billon 2002).

Whilst the three-decade long exploitation of Cambodia's natural resources has been undeniably undesirable from an environmental perspective, at the same time it cannot be ignored that the country has achieved a remarkable developmental transition. The World Bank's (2019) country overview highlights that in 2015 the country attained lower middle-income status after averaging 8% annual economic growth between 1998 and 2018. Although the poverty rate dropped from 47.8% in 2007 to 13.5% in 2014, they caution that 4.5 million people are still categorised as "near-poor". Simultaneously between 1990 and 2018 Cambodia's score on the United Nations Development Programme's (UNDP) Human Development Index (HDI) rose from 0.384 to 0.581, an increase of 51.4% with average life expectancy at birth over this period increasing by 16 years (UNDP 2019, 2). However, once this 2018 HDI score is adjusted for the inequality of development across the population then it falls by 20% to 0.465, highlighting that strong inequalities still exist across all three dimensions of life expectancy, education and income. These examples highlight the question long asked by scholars and development agencies alike: In the absence of a kleptocratic governmental system could Cambodia have achieved the same or greater development in a more equitable fashion and without the severe depletion of the country's natural resources?

2.1.2 Policy eras of natural resource management

2.1.2.1 The timber concessions of 1990's

The first of the aforementioned natural resource policy eras occurred through the 1990's and into the early 2000's and can be characterised by the privatisation of national forests which were auctioned off to foreign investors, primarily for the purpose of establishing timber concessions. Between 1994 and 1997, this saw an estimated 39% of Cambodia's total land area (~7 million ha) signed over to timber concessionaires on the basis of 5-25-year leases (Bottomley 2000). This comprised the majority of all remaining forested land not included in PAs established in the same year (section 2.2.1) (Forest Trends 2015), with the rest gradually being subsumed into agricultural concessions which by 2001 amounted to over 800,000 ha (McKenney and Prom 2002).

The policy was rationalized as one of the few viable means to generate revenue for post-war reconstruction following nearly two decades of conflict (1970s-80s) (Le Billon 2000). Indeed it was initially endorsed by the donor/development community who were very active in the country at the time, particularly the World Bank, who envisioned it as a transparent system of public-private partnerships that would put an end to the unsustainable forest exploitation that was still taking place to fund the last vestiges of the civil war (Diepart and Sem 2015). However, the way the concession system was actually implemented saw the hopes of donors dashed very quickly as, by 1996, logging practices were described by some observers as "anarchic" (Forest Trends 2015). This was characterised not just by unsustainable extraction inside concessions but also illegal logging outside their boundaries which resulted in rural communities being dispossessed of land and access to natural resources (Barney 2005; Diepart and Sem 2015).

The pressure upon the RGC from international donors to reform the concession system mounted towards the end of the century with successive sectoral reviews in 1996 (World Bank 1996) and 2000 (Barney 2005). The latter resulted in the announcement of the 2002 Forestry Law, and the indefinite suspension of all timber concession licences (ICEM 2003b). Although there is evidence that many concessionaires simply continued to operate despite the suspension (Barney 2005) and their activities did not truly cease until the concession system was finally brought to a halt following the Independent Forest Sector Review (IFSR) the RGC in 2004 (Forest Trends 2015).

The lack of political will to address the glaring issues with the concession system resulted from the fact that many of the concessions licences were held by societal elites and it was the patronage of these individuals that had allowed the CPP to secure its position in charge the newly formed RGC (Diepart and Sem 2015; Diepart and Schoenberger 2016). Thus, the end of this era meant that a new policy actuated means for this patronage network to continue had to be created. In the end the next policy regime did not represent a considerable structural change from the previous era, only that instead of timber concessions, state land was allocated to private companies for the purpose of agri-business development (Forest Trends 2015).

2.1.2.2 The transition to Economic Land Concessions in the 2000s

Whilst a precedent for the allocation of land for large-scale agricultural developments had been set by the granting of such concessions in the 1990s these were few in number and small in scale in comparison to the timber concessions. This changed dramatically with the introduction of the 2001 Land Law (RGC 2001) under which all untitled land in the country (75-80% of the total land area at the time) was re-classified as either state public land or state private land (Neef *et al.* 2013). More importantly it included the legal provision that state private land could be allocated, on a contractual basis, as Economic Land Concessions (ELCs) for the purpose of Agri-industrial development. The Land Law laid out a set of criteria upon which the allocation of ELCs was supposed to be based including that (i) the area of a single concession could not exceed 10,000ha; (ii) no individual nor legal entities (companies) controlled by an individual is allowed to hold more than one concession licence; (iii) the maximum length of ELC lease is 99 years and cannot lead to ownership; and (iv) exploitation of the land for the ELCs stated purpose must begin with 12 months of the granting of the licence (RGC 2001). These were subsequently strengthened by an additional sub-decree (no. 146) on the topic in 2005 (RGC 2005) which introduced the provision that all prospective ELCs had to produce environmental and social impact assessments (ESIA) along with sustainable use management plans prior to agreement of the licence by the Ministry of Agriculture Forests and Fisheries (MAFF).

The passing of sub-decree 146 in 2005 marked the beginning of a substantial increase in the number of ELCs declared which continued to rise until 2012 (Grogan *et al.* 2019) although the total extent of ELC land that was declared in the country is a subject of debate to this day. This is because information related to concession allocations has never been publicly disclosed in a systematic fashion by the RGC leaving it largely up to scholars and civil society organisations to devise their own estimates (Broadhead and Izquierdo 2010: FAO 2012). Whilst exact figures differ most sources agree that the amount of land allocated to ELCs by 2013 was between 2 and 2.5 million ha, representing over half of Cambodia's total arable land (Neef *et al.* 2013; LICADHO 2014; Forest Trends 2015). To put this into perspective Figure 3 shows the extent of ELCs included in one of the most comprehensive datasets produced by the Non-governmental organisation (NGO) Open Development Cambodia (ODC).

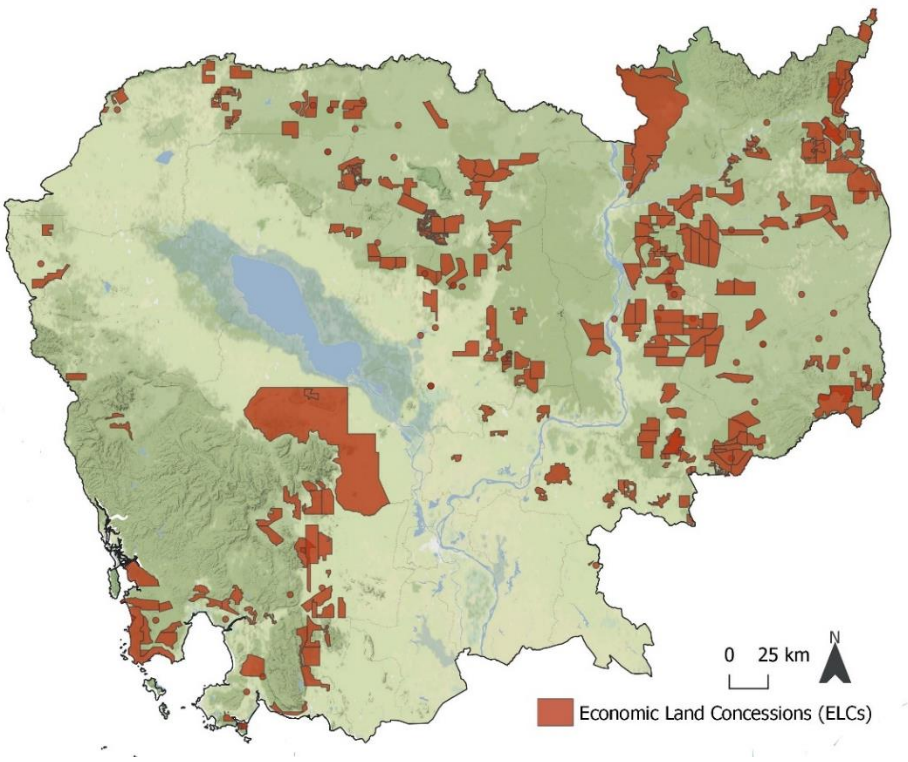


Figure 3: Location of Economic Land Concessions (ELCs) in Cambodia
(data sources: ODC 2017a GADM 2018)

Similar to the timber concessions the purported rationale behind the policy of ELC allocation was that it was in the interest of national development. The expectation was that they would increase production of agricultural goods for national consumption as well as export (particularly rubber) but also diversifying the rural labour market (Beauchamp 2016). However, the reality of the impacts of ELCs was very different and has been subject to considerable analyses from both environmental and social standpoints.

In environmental terms the establishment of ELCs inherently require the alteration of existing land cover which makes them a significant contributor to the trends of FCL observed in Cambodia over the last two decades (Rainey *et al.* 2010; Hor *et al.* 2014; Davis *et al.* 2015: Messerli *et al.* 2015; Beauchamp 2016; Beauchamp *et al.* 2018; Grogan *et al*. 2019; Magliocca *et al.* 2020). This could have been deemed acceptable had the productive capacity of ELCs been realised, however there is evidence to suggest that many concessionaires lacked both the capacity and intention to actually engage in agricultural production (EU 2012; Forest Trends 2015).

To put this into perspective, Un and So (2011) highlight that as of 2007 only 9% of ELCs had been put into productive use and Debonne *et al.* (2019) found that in 2015 only 32% of total ELC land was used productively. This is exacerbated by the fact that many sources attest to ELCs engaging in illegal land clearance and selective logging of luxury timber outside of their boundaries (Global Witness 2015). This was especially problematic considering that many ELCs were in close proximity to PAs and indeed, as of 2008, the RGC made it possible for them to be allocated inside PAs (Banks *et al.* 2014), which will be discussed further in section 2.2.1.

ELCs also resulted in substantial negative social impacts, similar to the timber concessions this was primarily in the form of displacement of rural communities through forced evictions, resulting in violence and other human rights abuses (Subedi 2012; Neef *et al.* 2013; Forest Trends 2015). This expropriation of land led to not only increasing poverty through loss of income from agriculture and access to natural resources (principally non-timber forest products (NTFPs)) but also irreparable damage to cultural heritage through the destruction of spiritual sites which disproportionately affected indigenous communities (CHRAC 2009; LICADHO 2005; 2009; EU 2012; Subedi 2012; ADHOC 2013).

Many of the negative impacts of ELCs stemmed from illegality in the processes of their allocation and management. This is evidenced by the fact that every criterion of ELC allocation established by the RGC was routinely ignored with numerous concessions being granted that were several orders of magnitude larger than the maximum 10,000ha, many owned by a small number of powerful individuals through shell-companies, and the majority submitting no form of ESIA or management plan (Oldenburg and Neef 2014; Global Witness 2015). This has led to the conclusion that the real intent of ELCs was simply a means for societal elites to continue to exploit the country's resources (Un and So 2011; Messerli *et al.* 2015: Diepart and Schoenberger 2016; Beauchamp *et al.* 2018).

However, the collective weight of the problems surrounding ELCs was eventually enough to bring the era of their establishment to a close. In May 2012, prior to the national elections prime minister Hun Sen issued a decree amounting to a moratorium on the granting of new ELCs as well as a pledge for a systematic review of all current ELCs in what became known as 'Directive 01' (also known as Order 01BB) (Diepart and Sem 2015).

2.1.2.3 Directive 01: The move towards genuine land tenure reform?

Directive 01 can be said to characterise the beginning of what can be thought of Cambodia's current policy era of natural resource management which some have suggested represents a genuine step towards tackling the issues of forest exploitation and land tenure insecurity within the country (Dwyer *et al.* 2015). The reason for this is that as well as halting the large scale allocation of land for new ELCs Directive 01 demanded a shift in perspective in the public land titling system towards "the allocation of land to deserving recipients rather than simply the provision of titles based on existing legal rights" (Dwyer *et al.* 2015, par. 35).

This the prime minister dubbed the 'Leopard skin' policy on the basis that it was intended to achieve a mosaiced landscape of smaller formally titled land parcels interspersed between larger existing concessions (Milne 2013; Work and Beban 2016). This was to be achieved through an increase in the granting of three different forms of land titles: private titles (recognition of agricultural land already occupied by individuals or families); social land concessions (SLCs: land allocated to poor or dispossessed communities for re-settlement); and indigenous communal land titles (CLTs: land traditionally occupied and managed communally by indigenous groups). Indeed, progress towards this started strongly with the government issuing 38 SLCs covering over 100,000 ha in 2012 (ADHOC 2013) and a further 485 (626,823 ha) in the following year (ADHOC 2014), primarily on land that had been excised from ELCs.

Unfortunately, this was short-lived as a number of problems came to light. Firstly, with regards to ELCs, despite the moratorium a further 16 concessions had been granted by the end of 2013 amounting to over 80,000 ha of which close to half was inside PAs (LICADHO 2014), with perhaps as many as 33 being issued in total by the end of 2014 (Diepart and Sem 2015). Additionally, the promised review of existing ELCs was disappointingly opaque and whilst many were downsized there was little clear information as to how many concessions were actually cancelled.

Furthermore, despite it's supposed intentions the leopard skin policy also served to perpetuate small scale land conflict in numerous capacities. Many of the SLCs allocated resulted in disputes because of corruption and mismanagement be well as being allocated over land already inhabited by vulnerable groups (ADHOC 2013; 2014). As for CLTs, inadequate provisions were included for certain types of communal land not under active use such as 'spirit forest' (Rabe 2013) and communities were often forced to choose between communal and individual (private) land titles resulting in the break-down of social capital (Milne 2013). The requirements of the private land titling also exacerbated forest clearance (land had to be cultivated to be eligible: Rabe 2013) and resulted in the loss of land for others (maximum title size of 5 ha: Work and Beban 2016).

The totality of these issues has led to the criticism that Directive 01 was merely a façade to win voter support and more importantly did not represent a viable means to halt deforestation in the country which continued illegally (Un and So 2011; Milne 2015). Of course, this cynicism must be taken in light of the fact that this policy era is still 'current' and on-going and thus no definitive conclusions can be drawn from it. In this regard it must also be acknowledged that the intervening period between 2012 and the present has seen a number of other generally positive developments in the domain of natural resource management (NRM). This has included a markedly increased commitment to conservation, evidenced not just through verbal rhetoric surrounding clamping down on illegal logging (EIA 2017) and decentralisation of NRM (Riggs *et al.* 2020), but also in concrete actions such as the expansion of the country's PA system and progression towards other conservation initiatives such as operationalising REDD+ projects, which will be discussed further in section 2.2.4.

2.1.3 Quantifying the loss of Cambodia’s forests

Now that the causative mechanisms of natural resource exploitation in Cambodia have been elucidated, it is important to offer quantification of the impacts over the last four decades. As previously mentioned, this has most strongly been exemplified by a trend of declining forest cover and whilst all sources agree with this general prognosis there has been significant contention with regards to the extent of change that has occurred both between and among national and international sources. The details of these disagreements do not merit discussion here although they are relevant in terms of informing the selection of a data source for forest cover and FCL for the assessment of PA avoided deforestation. For this reason, a discussion of the different sources of forest cover assessment (FCA) available for Cambodia is included as an appendix (Appendix B).

To visually demonstrate the extent of forest cover change that occurred in Cambodia between 1973 and 2014 the FCA produced by ODC (2019a) has been reproduced as Figure 4 below. The figures of total forest cover % in Figure 4 are largely in line with those of other sources which estimate that forest cover in the 1960s-70s was likely in the region of 70-75% of the total land area, decreasing to between 53-65% by the turn of the century and falling to 45-50% by 2014-2016 (approximations aggregated from FAO (2015), GDANCP (2018), and Grogan *et al.* (2019)), representing a loss of over 2.2 million ha of forest cover between 2001 and 2018 alone (Kresek 2019).

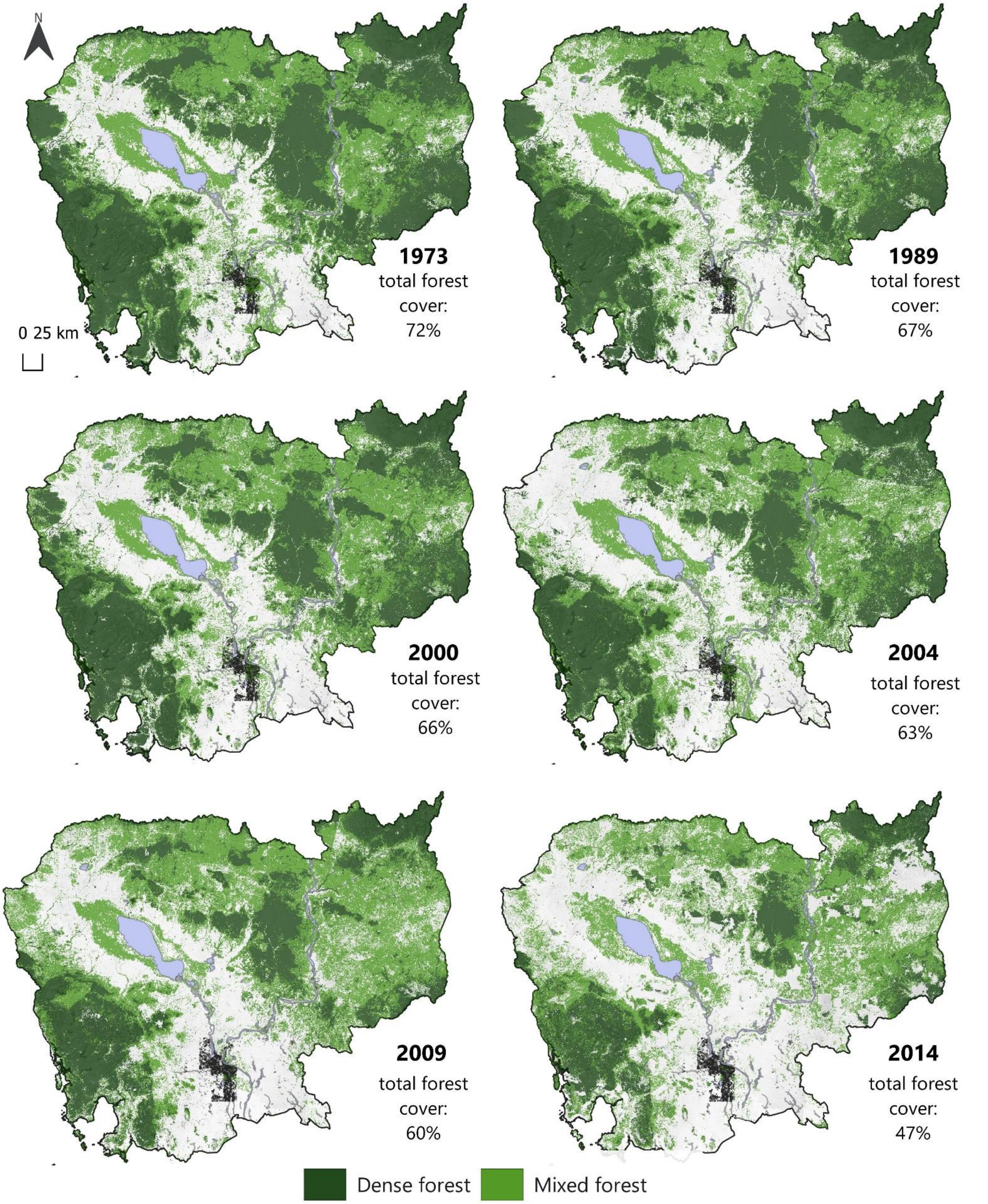


Figure 4: Changes in forest cover in Cambodia between 1973 and 2014 (data sources: GADM 2018; ODC 2019a)

Beyond this picture of total FCL it is also important to examine the trend in the rate of FCL that has been observed since the start of the century. This has been summarized in Figure 5 in terms of the amount of ha of forest lost per year according to Global Forest Watch data (GFW 2020b)

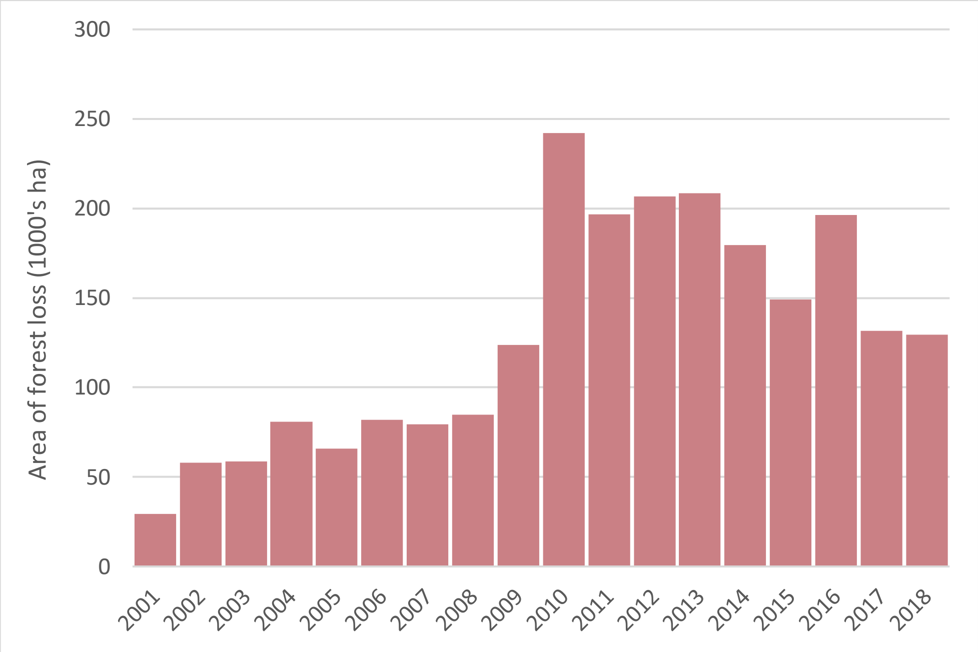


Figure 5: Forest loss per year (>10% canopy cover) in Cambodia between 2001 and 2018 (GFW 2020b)

Figure 5 shows that whilst the annual rate of forest loss was consistent throughout the early to mid-2000s it began to increase substantially after 2008, peaking in 2010, and remaining relatively high through to 2016. This trend is corroborated by the RGC's own figures which found that the annual rate of FCL increased from an average of ~2% per year between 2006 to 2010 to 10.65% between 2010-2016 (GDANCP 2018). These rates of FCL have been widely acknowledged as being substantial even in a global context with Ingalls *et al.* (2018) offering the summary that Cambodia had the "third highest deforestation rate in the world between 2005 and 2010 and the highest rate of tree cover loss in Asia for the period 2000–2012" (*p. 257*). The timing of this trend validates the evidence of deforestation being perpetuated by the allocation of ELCs as described in section 2.1.2.2 and the subsequent section (2.2) will detail that this loss of forest cover was unfortunately prevalent inside Cambodia PAs.

2.2 Cambodia's protected areas

The circumstances and extent of natural resource exploitation that have occurred in Cambodia over the last three decades make it hard to imagine that meaningful conservation strategies could have been implemented and yet today the country has a relatively expansive network of functioning (with active management) PAs (MoE 2017). However, the effectiveness of Cambodia's PAs has long been subject to widespread critique, with Clements *et al.* (2010) referring to early established PAs as 'paper parks' with almost no staff or funding for management. Further, analysis of GFW data in 2015 showed that a mature tree inside Cambodia's PA had an almost equal chance of being deforested as those outside PAs leading conservationists to declare that the PA system had reached a crisis point and was in need of complete restructuring (Peter and Pheap 2015).

Following this, in 2019 subsequent GFW data analysis concluded that: "between 2001 and 2018, Cambodia's protected areas lost 557,000 hectares of tree cover, (totalling approximately) 11.7% of the total protected area in Cambodia." (Kresek 2019, 1). In fact, this loss of forest cover was so pronounced that it led to one PA, Snuol Wildlife Sanctuary being degazetted in 2018 on the grounds that "there was nothing left to protect" (Boyle and Turton 2019, 1). Indeed, FCL is not the only problem, with the European Union's (EU) country profile of Cambodia in 2012 highlighting that 45% of forests inside PAs were degraded (EU 2012). This is further supported by Collins and Mitchard (2017) who highlighted Cambodia as a particularly significant example in their global analysis of CO_2 emissions from PAs on the basis of it having lost a "disproportionate amount of total protected forest carbon… amounting to over 16.5% since 2000" (*p*. 3). Additionally, it has been well acknowledged that biodiversity in the country's PAs faces continuing threat from illegal wildlife poaching (WWF 2012; 2013; Gray *et al.* 2017).

It is important to note however that a large number of critiques of Cambodia's PAs have been primarily anecdotal in nature and where quantitative evaluations have been made, they have often fallen into the trap of weak causal inference by not offering robust counterfactual comparison (section 1.2.3). This is of course one of the main rationales behind the undertaking of this study which will be discussed in section 2.3. Although in order to inform the design of such an assessment it is necessary to first discuss key aspects of Cambodia's PA system, namely: the history of it's establishment, expansion and management

(2.2.1); the analyses of different aspects of its effectiveness (2.2.2); the value of the resources that it conserves (2.2.3); and recent/current events shaping the its future (2.2.4).

2.2.1 History of PA establishment, expansion and management

The origins of Cambodia's post-colonial PA system began with the declaration of the Royal Decree for Protected Areas (RDPA) in 1993 which established 23 PAs amounting to a total area of 3,289,000 ha (18% of the country's total area) and covering 30% of its forests (Diepart and Sem 2015). These PAs were classified under 4 different categories: national parks; wildlife reserves/sanctuaries; protected landscapes; and multiple use management areas. The decree also included 3 RAMSAR sites. Management, planning and development of all these sites was entrusted to the General Department of Administration for Nature Conservation and Protection (GDANCP) of the Ministry of Environment (MoE) under the RGC (RGC 2014).

However, there was also another type of PA established in the country in the same period, in the form of 'protected forests' which constitute part of the country's permanent forest reserve and under the 2002 forestry law and are managed by the Forestry Administration (FA) under MAFF for the purpose of 'conservation and development of the forest resource and biodiversity' (RGC 2010, 45).

The size of the country's PA estate grew throughout the 2000's with the addition of both new PAs under GDANCP and new protected forests under the FA (ICEM 2003b; Broadhead and Izquierdo 2010). The size of these additions was relatively constant until 2016 when 5 new PAs were established covering over 1 million ha of forest and grassland, bringing the supposed total PA coverage up to 6,038,275 ha (Souter *et al.* 2016) (although this figure should not be taken as representative for reasons which will be discussed below). The locations and relative distributions of Cambodia's terrestrial PAs under different management categories as of 2020 are represented visually in Figure 6 below.

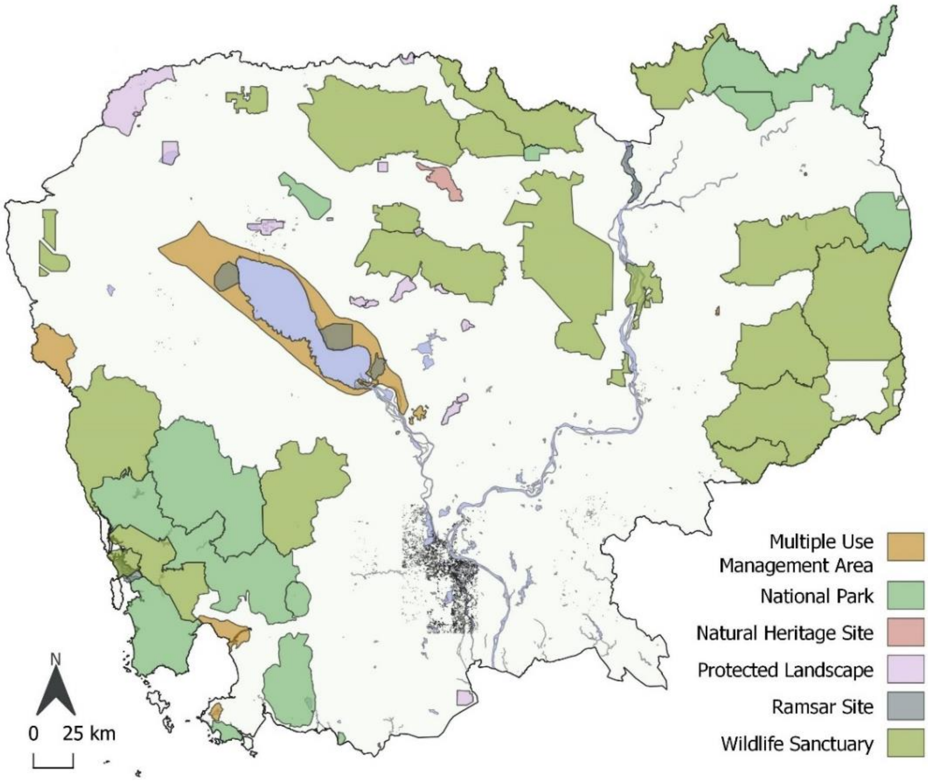


Figure 6: National protected areas in Cambodia established between 1993-2019
(data sources: GADM 2018; ODC 2019b)

The most significant development related to the management of PAs in Cambodia following the RDPA was the 2008 Protected Areas Law (RGC 2008) which included provisions for all PAs to be subdivided into zones based upon management objectives including: core (conservation) zones: sustainable use and community zones. On the face of it this should have been a positive development as it represented progress towards re-dressing issues of natural resource use by communities whose ancestral lands had been contained within PAs (Dunai 2008). In reality its primary significance was quite the opposite as it created the opportunity for the RGC to issue land inside PA sustainable use zones to private companies for the purpose of agricultural (ELCs) and mining concessions as well as irrigation and hydropower developments (Beauchamp 2016). Prior to this, developments such as the timber concessions of the 1990's (section 2.1.2.1) had been allocated adjacent to the borders of PAs but none formally inside their boundaries (ICEM 2003b).

The provision that these activities were supposed to take place in sustainable use zones only was effectively a 'red herring' as almost none of the PAs established at the time had zoning plans implemented (Banks *et al.* 2014). Indeed, the National PA Strategic Management Plan indicates that as of 2017 only 3 PAs had actually been zoned (MoE 2017). This meant that instead the large number of concessions that were issued inside PAs, primarily for ELCs, were justified on the basis that the land they were allocated was degraded and/or unforested (*p. 20*). In reality, the spatial congruence of these concessions with forest cover locations show that this was certainly not the case and concessions were allocated over primary forest (Neef *et al.* 2013; Davis *et al.* 2015*).*

The fact that the explicit purpose of many of these development projects was to extract natural resources or at the very least clear the prevailing land cover (in the case of ELCs and mining concessions), then it is logical that any land given over to this purpose should *de facto* be considered as degazetted or excised from the PAs total area (Clements *et al.* 2014). In the case of Cambodia this is corroborated by WWF and Conservation International (CI)'s database on global PADDD events which lists all the occasions of PA land being allocated to ELCs as either 'downgrading or downsizing events' (WWF 2013; CI and WWF 2019). However, this is ignored in the RGC's own statistics (Banks *et al.* 2014) which report that PA coverage in the country, as of 2017, is 39% of the total land area (MoE 2017). Given the uncertainty surrounding both the size of ELCs and revocations following Directive 01 (section 2.1.2.2) and that there are discrepancies between data sources for PA boundaries (Appendix C) then an accurate figure for genuinely 'protected' PA land in Cambodia is difficult to ascertain. In this regard Figure 7 below provides an insight into the expansion of PA land in Cambodia over time and demonstrates the difference in the size of the total estate when ELC land is indeed excised from purported PA boundaries. Figure 7 shows that the amount of ELC land allocated inside PAs between 1998 and 2014 was somewhere in the region of ~680,000 ha and it is noteworthy that this does not include other forms of concessions granted for which the information is even less reliable (mining concessions) also the locations are highlighted in a subsequent figure in section 2.2.2.1.

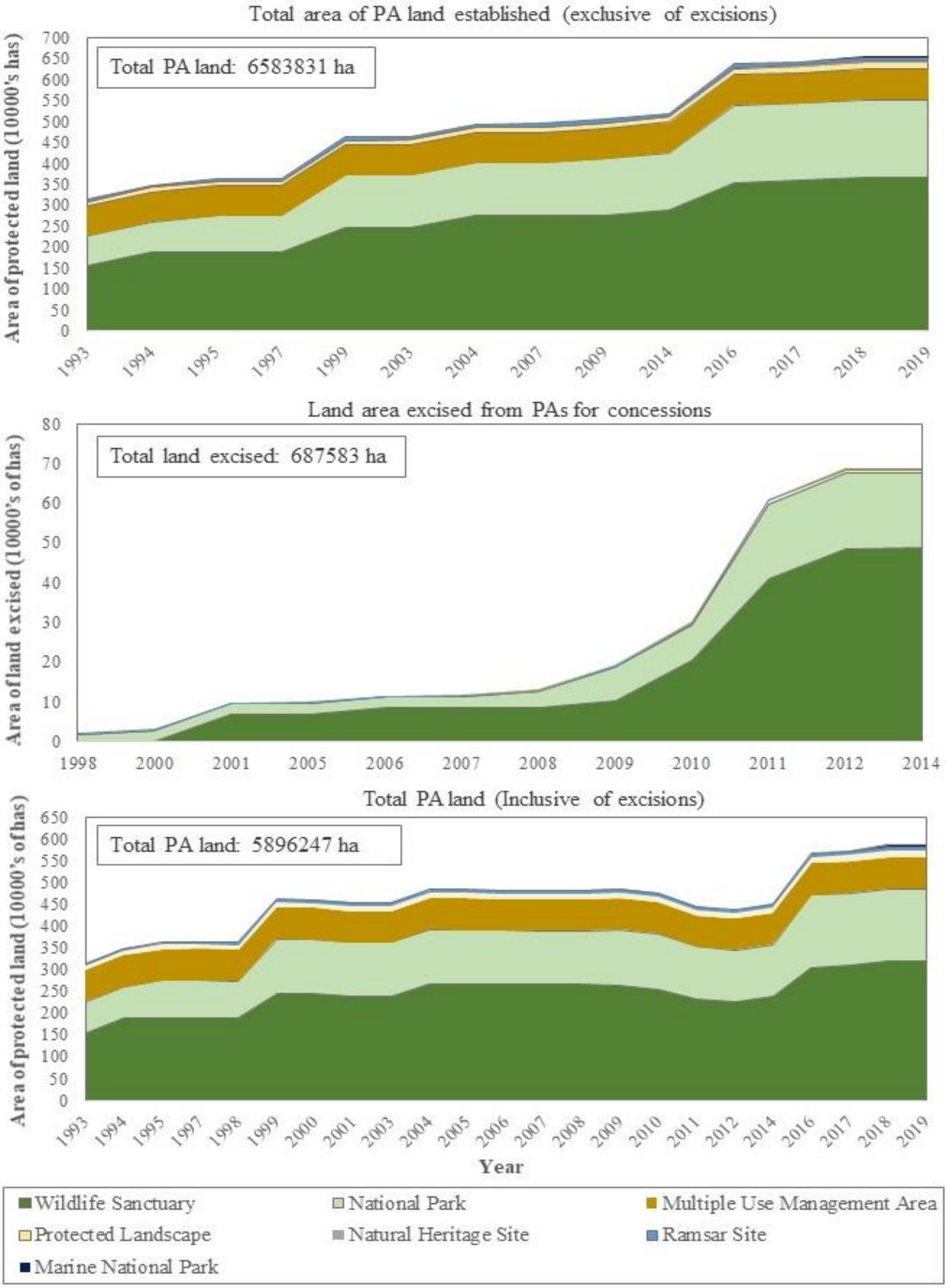


Figure 7: Expansion in the size of Cambodia's protected area estate between 1993-2019 (data sources ODC 2017a: 2019b)

Another important factor to consider with regards to the evolution of Cambodia's PAs is the involvement of international conservation organisations in supporting the MoE and FA in management. The exact timeline of this is hard to pin down but notably the involvement of large-scale organisations such as WWF; Wildlife Conservation Society (WCS); CI; Wildlife Alliance, and Birdlife International began in the early 2000s (WWF 2012; Clements and Milner-Gulland 2014a.; Wildlife Alliance 2017; BirdLife International 2020; CI 2020).

This involvement has undoubtedly resulted in positive outcomes in terms of increasing active management and capacity building through the provision of financial support and technical expertise (ICEM 2003b; WWF 2012). However, there is also evidence that it has downsides as well. For example, it can lead to tension between staff employed by or on secondment with NGOs and those employed by the RGC, as the latter tend to have lower salaries commensurate to greater responsibilities (ICEM 2003b). Additionally, it also presents the risk that the government becomes reliant upon the external funding provided by NGOs and fails to develop sufficient replacement sources when their support is inevitably phased out. Although in the case of Cambodia the RGC's commitment to operationalise REDD+ schemes could address this issue (discussed in section 2.2.4).

Finally, it is important to note that there are two other forms of PAs that exist within the national PA estate with slightly different management modalities, namely Community Protected Areas (CPAs) and Community Conservation Forests (CCFs). CPAs constitute forest areas within PAs managed by the MoE (principally in wildlife sanctuaries and national parks) (Mahanty *et al.* 2006), whereas CCFs are demarcated inside protected forests managed by the FA (WWF 2012). CCFs however, are not be confused with Community Forests (CFs) which are an additional legally recognised form of land title that is granted in forested areas specifically outside national PAs (Broadhead and Izquierdo 2010). The purpose of CPAs, CCFs and CFs is to contribute to conservation by decentralising the management of natural areas to local communities under sustainable use management plans (*p. 56*). Thus far the extent to which this has been achieved and its overall effectiveness as a strategy is still unclear and would benefit from further assessment (Lambrick *et al.* 2014: Lonn *et al.* 2018).

2.2.2 Prior analysis of PA effectiveness

The purpose of this section is twofold: First, section 2.2.2.1 will detail the host of macro-scale factors that have been posited as being responsible for the environmental degradation exhibited by Cambodia's PAs. Second, section 2.2.2.2 discusses the very limited number of studies that have used counterfactual approaches to attempt to quantify whether this environmental performance is indeed representative of poor PA effectiveness or rather if it is simply a microcosm of wider environmental trends occurring in the country.

2.2.2.1 Explanatory factors of environmental degradation

Conceptually the first factor that has been posited for the poor environmental performance of Cambodia's PAs is that those that were declared in the 'first wave' in 1993 (that still make up a substantial proportion of the total PA estate today) were subject to flawed design and planning. Insufficient data on the locations of human settlements meant that many were included inside PA boundaries and were not subsequently re-settled leaving them with unclear tenure rights (Clements *et al.* 2010; 2014). Also, the boundaries and locations of these PAs were devised on the basis of outdated information of habitat coverage and thus they failed to capture many of the areas later deemed critical for biodiversity conservation (ADB 2001; ICEM 2003b; Souter *et al.* 2016). Although Neugarten *et al.* (2020) show that subsequent additions to the PA network have gone some way towards rectifying this.

The next widely cited explanation for the poor environmental outcomes of Cambodia's PAs is weak management effectiveness. Whilst the GD-PAME lists that 49 formalized PAME assessments have been completed in Cambodia (UNEP-WCMC 2020c), only one, Lacerda *et al.* (2004), has been published in the public domain. Furthermore, this was hardly a detailed assessment as it was conducted using the WWF's RAPPAM (Rapid Assessment and Prioritization of Protected Area Management) methodology (intended to provide a quick overview), and covered only the PAs that existed under MoE management at the time. Regardless, one of its primary conclusions was that the PA system was "chronically lacking resources in practically all levels of management" (*p. 19*).

Outside of formal assessments numerous other sources have expanded upon specific aspects of how an overall lack of resources has compromised PAME in Cambodia. A principal example of this is a lack of financial resources which has clear implications for the level and scope of management activities that can be achieved. Historical evidence of this in Cambodia's PAs has been well documented, with some reports finding that funding for some national parks in the 1990's was equivalent to less than $10,000 per year (ICEM 2003a) and Clements *et al.* (2014) noting that funding for management zones in PAs in 2004 was approximately $2/ha. This level of funding is barely sufficient to cover staff costs leaving little to invest in infrastructure (ICEM 2003b). Worryingly, Souter *et al.* (2016) (of whom the authors represent a cross section of the leading conservation practitioners in the country) indicate that severe underfunding still remains a contemporary problem, requiring greater commitment on the part of the RGC.

Another dimension in which Cambodia's PAs are lacking in resources is in terms of the capacity of their staff (i.e. human resources) (ICEM 2003b; WWF 2012). This has been particularly problematic with regards to formalizing PA management plans, with the RGC acknowledging in 2014 that 84% of PAs at the time were ineffective for this reason as well as an overall shortage of competent staff (RGC 2014). Although a National Protected Area Strategic Management Plan (NPASMP) for 2017-2031 has now been produced (MoE 2017), this highlights two further issues resulting from the combined lack of capacity as well as underfunding. First many PAs still do not have adequate physical demarcation of their boundaries (*p. 9*) and that the zonation of PAs had only been achieved for 3 of the total 49 PAs in 2017 (*p. 24).*

Exacerbating the above, a separate well documented phenomena contributing towards poor PAME in Cambodia is the institutional structure of management. Specifically, the historical overlap in jurisdiction between the MoE and FA has resulted in inefficiency and confusion stemming from poorly delineated responsibilities, especially in cases where PAs managed by each authority are spatially contiguous with each other (ICEM 2003b; Souter *et al*. 2016). This has been aggravated by poor coordination, communications and a lack of information sharing between the two organisations (ADB 2001), stemming from the fact that decision-making processes remain highly centralised and there is a climate of inter-ministerial competition (Amariei 2004).

One of the clearly observed implications of poor management effectiveness in Cambodia's PAs has been the inability to address illegal activities inside their boundaries. Illegal logging of luxury timber and land encroachment are perhaps the prime examples of this (ICEM 2003b; EIA 2017) although poaching of endangered species is also prevalent (WWF 2012; 2013; Gray *et al.* 2017). These activities persist due to weak PA law enforcement, which is partly due to a lack of capacity and funding, but has also been constrained by the limited judicial authority granted to PA rangers (UNODC 2015).

Overall, though illegal activity in PAs has arguably not been as damaging to their environmental effectiveness as the legalization of formal resource extractive industries inside their boundaries with insufficient oversight (Broadhead and Izquierdo 2010). As mentioned in section 2.2.1 this has primarily taken the form of ELCs, with 113 separate concessions being granted inside PAs between 2008 and 2012 alone, amounting to over 272,000 ha and covering over 14% of total PA land (Oldenburg and Neef; Forest Trends 2015; Souter *et al.* 2016). The activities of these concessions have been explicitly linked to illegal logging outside their boundaries, with Global Witness (2015) documenting that even officials as high up as PA managers were employed by ELCs to identify the location of high value timbers within the PAs surrounding the ELCs for them to extract.

Substantial amounts of PA land have also been allocated for mining concessions (WWF 2020) and hydropower development (Matthews and Geheb 2014) with the latter being particularly prominent. These developments obviously involve clearance of land as part of their operation although both have also been linked to illegal clearance of forest beyond their permitted areas (Käkönen and Thuon 2019). Similar to ELCs there is a pronounced lack of transparency with regards to the status of these developments, however Figure 8 below demonstrates the extent to which all three types have been allocated inside PAs according to government records.

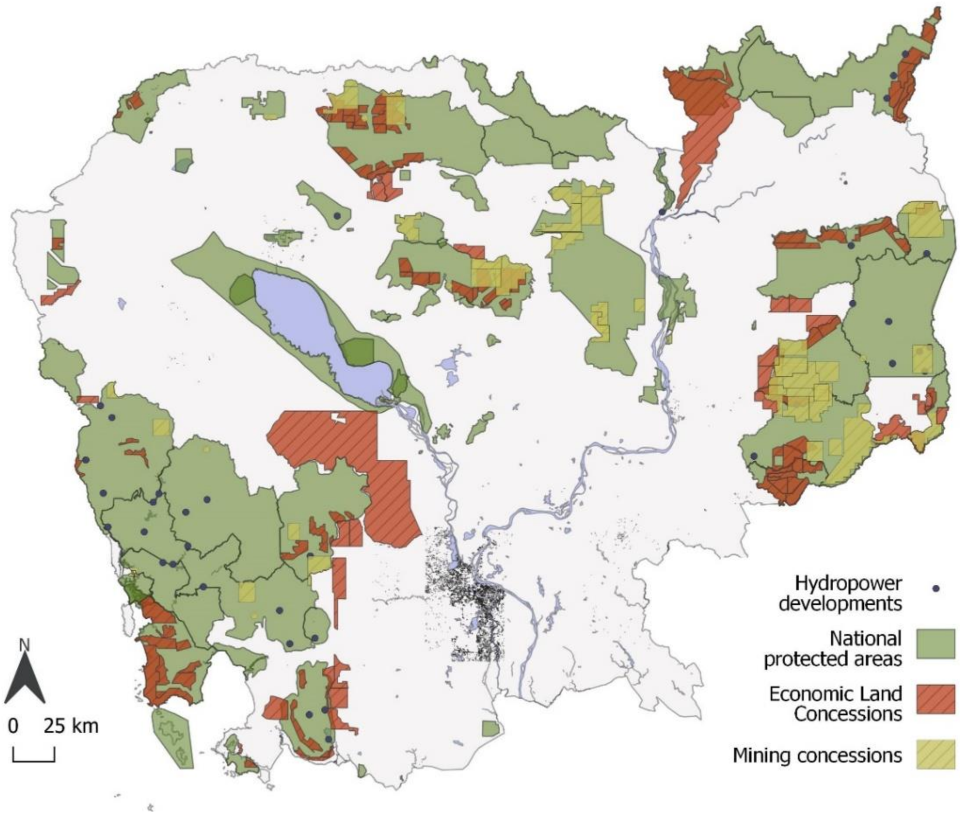


Figure 8: Locations of resource extractive developments inside protected areas in Cambodia (data sources: GADM 2018; ODC 2017a: 2019b: 2020b: 2020c)

2.2.2.2 Counterfactual investigations of PA effectiveness

As previously alluded to the contribution of counterfactual studies to the collective literature surrounding PA environmental effectiveness is Cambodia is slim, consisting of just two studies: Clements and Milner-Gulland (2014a) and Ota *et al.* (2020) (although Clements *et al.* (2014) did adopt a counterfactual approach to assessing PA impacts on local livelihoods). Both these studies concluded that PAs in Cambodia were effective in reducing the deforestation rate inside their boundaries compared to counterfactual control areas, however they also highlight the existence of clear knowledge gaps that warrant further investigation.

Clements and Milner-Gulland's (2014a) study utilised a quasi-experimental BACI design with nearest neighbour covariate matching performed using the Mahalanobis distance albeit with a fairly restricted scale, focusing on just two PAs. For the portion of their study analysing the impact of PAs on deforestation the authors used two outcome periods, the first between 2001 to 2006 and the second between 2006 to 2010 (to reflect the transition toward more active management of PAs) (*p. 80*). The biophysical and socio-economic covariates employed by Clements and Milner-Gulland (2014a) are detailed in Table A1 in Appendix A and are typical of studies of this type although this is a positive indication that such variables are applicable in the context of Cambodia.

As alluded to above the primary conclusion from this study was that deforestation rates inside PAs were significantly less (by as much as 60%) than in matched control areas following the transition to active management. Perhaps equally important was the observation that the deforestation in PA 'border areas' (4-12 km buffer zone) was greater than inside PAs and increased significantly in the second outcome period (active management), leading the authors to suggest that there 'may' have been negative spillover effects of protection (section 1.3.4.4) (*p. 81*).

A final noteworthy point from Clements and Milner-Gulland (2014a) is their suggestion that one of the key impacts of the PAs they analysed was their influence in stopping the establishment of ELCs on the land they occupied (*p. 84*). Thus, their analysis obviously cannot be said to be inclusive of the effect of ELCs on all PAs as the wider literature has shown that many PAs were not able to mitigate ELC establishment even inside their boundaries (section 2.2.2.1). This is especially true as their outcome periods precede the time when ELC allocation was at its highest between 2010-2012 (Figure 7: section 2.2.1).

As for Ota *et al.* (2020) they evaluated 'forest conservation effectiveness' between PAs, CFs, and Protected Forests (PFs: see section 2.2.1) established in Cambodia between 1994 and 2005. They quantified this in terms of avoided deforestation observed between 2005-2016 using an analysis based upon a combined RBC and matching methods approach (see section 1.3.3). This involved propensity score matching and generalized boosted models (*p. 3*). One of the aims of their study was to highlight how the difference in management authority (MoE vs. FA) might influence effectiveness between PAs and PFs which is why they considered them as separate treatments. Again, the covariates they used are detailed in Table A1 (Appendix A), and whilst they were relatively standard, they did explicitly include 'distance from ELCs' (*p. 3*). Unfortunately, they offer no discussion as to how this particular variable influenced PA effectiveness and indeed it is not clear if they treated this variable as time variant by only including ELCs that were established in the first year of their outcome period (see section 1.3.4.2). This is confusing as they state that they did remove the areas of ELCs that overlapped with PA boundaries which implies that they must have only used ELCs that were in existence prior to the establishment of the PAs. Indeed, even if they did do this, their study cannot be considered to constitute a thorough investigation of the effects of ELCs as many were established later than 2005, especially those inside PA boundaries (see section 2.2.1).

The results of their primary analysis were that CFs, PAs and PFs all produced significant negative ATE's with regards to their respective matched controls meaning that all generated avoided deforestation. Amongst the three there was a 'hierarchy' of effectiveness with PFs exhibiting significantly less deforestation than PAs and both significantly reducing deforestation as compared to CFs (*p. 4*). This is interesting as it contradicts an earlier assessment by Broadhead and Izquierdo (2010) who found that conservation areas managed by the FA (PFs) were subject to greater FCL than the MoE PAs because of the existence of more former logging roads within their boundaries. Whereas Ota *et al*. (2020) suggest that their result of greater effectiveness of PFs could be the result of more active management by the FA (by comparison to the MoE (GDANCP)). This could have implications for the effectiveness of PAs in the future given institutional changes to PA management that occurred in 2016 which will be detailed in section 2.2.4.

Similar to Clements and Milner-Gulland (2014a) the secondary results of Ota *et al.*'s (2020) analysis also offers important insight, namely their testing for spatial spillover effects. They found non-significant differences between different widths of buffers less than 6 km (0–

2; 2–4; 4–6). However, they did find some significant differences between the prevalence of deforestation within these buffers vs. the wider control region (> 6 km from PA boundaries) although these were not consistent across the treatments. Firstly, they found no significant spillover effect for PAs but they did for PFs and CFs (*p. 4*). Secondly the nature of the spillover effects for PFs vs. CFs were different, with PFs having a positive spillover i.e. reduced deforestation in the buffer zone, whereas CFs showed a negative spillover effect with significantly greater deforestation occurring within their buffer as compared to the wider control region (*p. 4)* The absence of a significant spillover effect for PAs specifically is surprising given that a phenomenon to that effect was observed by Clements and Milner-Gulland (2014a).

In conclusion, the summary of these two studies, representing the entirety of the literature surrounding counterfactual PA environmental effectiveness in Cambodia, make it clear that further investigation is needed on a number of fronts. These will be explored in more detailed in section 2.3 which will draw together the rationale and proposed foci of an assessment of PA effectiveness in Cambodia.

2.2.3 What's left to conserve? The value of remaining resources in PAs

Previous sections have painted a bleak picture of Cambodia's PAs by focusing on the extent of FCL that has occurred. However, it is important to recognise that they do still contain natural resources that are of substantial importance not just for the country's population but also in a global context.

As expected, the primary resource of interest in this regard is still Cambodia's forested habitat of which the majority is now contained inside PAs (Renard *et al.* 2020). Despite the long-term trend of FCL Cambodia still exhibits greater forest cover than the larger surrounding countries of Thailand and Vietnam (Forest Trends 2015). The importance of this is acknowledged by the RGC whose national forest policy has set the ambitious target of increasing national forest cover to 60% and retaining it into the future (RGC 2010). The benefits of adhering to this target and protecting Cambodia's remaining forests through its PAs are myriad, and from an anthropocentric perspective they can be characterised as either direct or indirect benefits.

The direct benefits of conserving forests in Cambodia is primarily exemplified by their socio-economic importance to rural communities. Numerous studies have sought to quantify this, with Hansen and Top (2006) and Jiao *et al.* (2015) estimating that rural households still derive between 30-42% of their total household incomes from forest resources and the RGC (2010) acknowledging that as many as 75% of subsistence farmers depend on these resources. This reliance on forest resources takes many forms, through the use of timber for the construction of homes and firewood to the collection NTFPs for general consumption, traditional medicinal uses and also to supplement livelihoods as a 'safety net' in times of economic shocks (ICEM 2003b; RGC 2010; Chou 2018b).

These direct benefits of access to forest resources have been explicitly linked to the functioning of PAs with Clements *et al.* (2014) finding that improved (more active) PA management resulted in positive economic benefits for surrounding communities. Furthermore, Chou (2018a) highlighted that the ability to engage in NTFP extraction was one of the most influential factors in local people's decision to participate in conservation activities and as an incentive this generated benefits of around $0.95/ha for one specific PA.

As for the indirect benefits of forest conservation, perhaps the most significant from the perspective of the population of Cambodia is the continued provision of ecosystem services of different descriptions, particularly watershed maintenance and water purification which will be crucial if the growing number of hydropower developments in the country are to be successful (see section 2.2.2.1) (ICEM 2003a; 2003b; Netzer *et al.* 2019). Additionally, maintaining forest cover has clear benefits in terms of reducing the impact of flooding and droughts, nutrient and sediment retention, carbon storage (EU 2012: Watkins *et al.* 2016), and ultimately playing a key role in the mitigation and minimization of the many adverse effects of global climatic change (ICEM 2014). To put this in monetary terms the ADB (2015) estimate that if FCL in Cambodia continues at its projected rate between 2010-2030 the resultant loss in ecosystem services will equate to over $6 billion.

The second key indirect benefit of Cambodia's forests is their important contribution to regional and global biodiversity as part of the Indo-Burma biodiversity hotspot (Souter *et al.* 2016). More specifically PAs within the country contain a number of globally threatened habitat types such as tropical deciduous dipterocarp forest (DDF), riverine forest, seasonally inundated wetlands as well as coastal mangroves (ICEM 2014). Many of these habitats are recognised as key biodiversity areas because of the presence of endemic and flagship species across the taxonomic spectrum (Daltry 2008; Clements *et al*. 2013; Gray *et al.* 2012; Kibria *et al.* 2017).

Finally, in a more abstract sense the preservation of remaining forest resources can also be considered as resulting in indirect benefits through the option (the benefit of being to utilise the resource in the future offset against the opportunity cost of not using it in the present) and existence values (the socio-cultural and emotional benefits of simply knowing that something exists) it represents (Pearce 2001). These benefits are of unique importance to Cambodia's indigenous minorities for whom the forest is an integral part of their culture and belief systems (Kibria *et al.* 2017).

2.2.4 Socio-political developments shaping the future of conservation

Whereas sections 2.1.2 and 2.2.1 discussed the historical context of PAs in Cambodia, in order to understand their current situation and how this might change in the near future, it is important to highlight several recent events and trends within conservation in the country.

The first and most influential of these for PAs is that in 2016 the management of all protected forests was transferred from the FA to the GDANCP under the MoE which saw them re-classified as wildlife sanctuaries (in keeping with the schema of MoE PA categories) (ODC 2017b). This transition was particularly significant given that it occurred simultaneously with the establishment of five new PAs. This meant that in total the GDANCP was responsible for the management of a further 2.6 million ha of protected land, representing an increase of 80% as compared to its previous portfolio (Souter *et al.* 2016).

This development was criticized at the outset by a cross section of conservation practitioners in Cambodia especially as there was no indication that MoE would receive additional funding and the ministry is already acknowledged to be under-resourced (*p.3*). However, in the period since this institutional change there has been no formal analysis of the impacts that it has had.

Another concerning development has been the apparent increase in the number of serious violent confrontations between PA staff and individuals engaged in illegal activities. Since 2015, five PA patrolling staff have been killed in several different incidents with a sixth being shot and wounded in 2019 (Kasztelan 2018; WWF 2019). This makes it clear that not only is illegal activity still occurring in Cambodia's PAs but that there is also a growing culture of fear with respect to PA law enforcement. This is made more problematic by the fact that those responsible for the murder of three PA staff on patrol in 2018 were government border police attempting to cover up their involvement in illegal logging (Lipes 2018). This trend has arguably contributed to what can be seen as somewhat of a growing dichotomy between the approaches to PA management pursued by different international conservation agencies who support the GDANCP. The perspective offered by WWF and WCS, who are perhaps the most visible of the NGOs supporting the greatest number of PAs, can still very much be characterised as a 'win-win' approach combining conservation with a strong community development focus.

In contrast, Wildlife Alliance (who have operated for a substantial amount of time in Cambodia but are focused only on the PAs in the region of the Cardamom mountains) appear to be pursuing an approach that, outwardly at least, is much more reminiscent of a fortress conservation mentality. Whilst they do operate community and education programs the primary focus of their promotional material and donor outreach belies a strong emphasis on strict PA law enforcement replete with many images and videos of armed rangers on patrol arresting suspects (Wildlife Alliance 2020).

Another alternative is being offered by Rising Phoenix Ltd. who are a private company seeking to be the first in Cambodia to secure a private-public partnership (based on the African parks model) with the RGC to manage a PA (Western Siem Pang Wildlife Sanctuary). Although this has yet to be finalized, part of their proposed plan would be to fence substantial portions of the PAs boundary under the guise of 'rewilding' efforts (Gray *et al.* 2019).

This diversification into a new funding model is itself symptomatic of a wider trend across all the conservation NGO's in Cambodia who are seeing funding for PA management become increasingly difficult to obtain as larger multilateral aid and development organisations seek to devolve further responsibility to the RGC (Souter *et al.* 2016).

The result of this has seen NGOs and the RGC invest considerable effort into operationalising REDD+ projects within PAs as a means of securing long-term funding (Forest Trends 2015: JICA 2017). Although, the success of project implementation thus far has been mixed with one pilot project in Oddar Meanchey being particularly contentious with regards to achieving deforestation reductions and being inclusive of community participation (Frewer 2015; Lang 2016). However, by contrast a project in Keo Seima Wildlife Sanctuary generated $2.6 million from the sale of carbon credits in 2016 (Ken *et al.* 2020). In total 17 REDD+ projects are at different stages of operation and development at present (p. *3*) and if these can be implemented effectively, they have great potential as a means of improving PA effectiveness by providing a clear monetary incentive against continued state sponsored resource extraction.

In addition to making progress with REDD+ in recent years the RGC has also begun negotiations to participate in the joint FAO-EU Forest Law Enforcement, Governance and Trade (FLEGT) initiative (Forest Trends 2015). This would see them enter into a voluntary partnership agreement that would aim to ensure that timber exported from the country is legally sourced (*p.60*). Obviously to achieve this would mean addressing the issues of illegal timber extraction which would undoubtedly have benefits for the PAs in which this still occurs.

Further to this the RGC have also made a very public commitment to reintroducing the extirpated wild tiger (*Panthera tigris*) to Cambodia as part of WWF's global 'Tx2' initiative to double the number of wild tigers by 2022. Several PAs in the country have been identified as suitable locations and a re-introduction plan drafted (Gray *et al.* 2017; Debonne *et al.* 2019). Although Miquelle *et al.* (2018) caution that the necessary steps to facilitate a successful re-introduction, primarily the elimination of the threat posed by poaching and recovery of prey populations, will require 10-20 years of concerted effort.

In conclusion, when viewed collectively these developments do suggest an increasing desire on the part of the RGC to achieve genuine conservation progress. It is also clear that an in-depth assessment of PA effectiveness could provide numerous insights that could contribute towards this, such as which locations should be prioritized for the establishment of REDD+ sites. In this regard the following section will expand upon the rationale behind such an assessment and detail other possible synergies in knowledge it could produce.

2.3 Rationale for a national-scale assessment of PA effectiveness

In summary, the successive sections of this chapter have presented a logical progression through the following notions:

i. Historically environmental resource management in Cambodia has been a contentious issue (section 2.1.1), characterized by several different policy regimes (2.1.2), all of which have been co-opted by an elite class to perpetuate large scale exploitation of the country's forests (2.1.3).

ii. Despite this hostile setting the country has established a network of PAs although the landmark decision to legalize extractive industries within them raised questions as to the commitment of the country's government to using PAs to achieve conservation goals (2.2.1).

iii. The impact of this policy and other factors that have resulted in sub-optimal environmental outcomes for Cambodia's PAs have been the subject of some investigation (2.2.2.1). Although only two studies have analyzed PA effectiveness in a counterfactual capacity and these have clearly highlighted knowledge gaps that could be addressed by further assessment (2.2.2.2).

iv. The resources that Cambodia's PAs still protect are of significance not just at a national scale but also in terms of global biodiversity, and ecosystem services (2.2.3). This is increasingly recognized by those responsible for them who have exhibited a renewed focus towards creating a genuinely effective system of PAs within the country (2.2.4).

It is the totality of these notions that make it clear that a comprehensive and robust counterfactual assessment of the effectiveness of Cambodia's PAs is highly relevant in the current context. First and foremost, such an assessment should seek to build upon the narrow literature of country-specific counterfactual assessments. In this regard, the existing studies by Clements and Milner-Gulland (2014a) and Ota *et al.* (2020) provide a strong incentive to continue with assessment based upon the use of avoided deforestation as a metric for effectiveness. This is further supported by the lack of data available to assess other aspects of effectiveness such as PAME (section 2.2.2.1).

Now that a rationale has been established the next steps are to conceptually delineate what the focus of such an assessment should be and how it could offer relevant insights to the authority now responsible for PA management, namely the GDANCP of the MoE under the RGC. For clarity these have been presented as separate subsections below.

2.3.1 Planning an assessment of PA avoided deforestation

Summaries of the previous counterfactual studies of Cambodia's PA effectiveness in section 2.2.2.2 highlighted several knowledge gaps that could form the basis of the forthcoming assessment. Firstly, neither study analyzed the effectiveness of the PA network in its entirety, with Ota *et al.* (2020) being the more comprehensive in terms of its coverage but still only investigating PAs and PFs established between 1994 and 2005. Hence analysis should be performed for a broader selection of PAs that were established following this period (see section 2.2.1).

This inherently creates the need to analyze PA effectiveness in other time periods, which simultaneously should be viewed as an opportunity to investigate those that can be characterized by prominent events or themes. Chief amongst these should be the influence of ELCs as whilst it is clear that they had a profound impact on PAs (sections 2.1.2.2 and 2.2.2.1), this has yet to be adequately quantified. As such the location of ELCs should be included as a covariate and investigated in a time variant manner. For the purpose of comparison this should include periods where their influence on PAs can reasonably be assumed to differ. For example, it should be expected that the period surrounding the time when the majority of ELCs were allocated will be that in which their effect was the most significant and hence it should be expected that avoided deforestation in PAs will be the lowest. In this regard Figure 9 below shows the amount of ELC land declared in Cambodia between 1995 and 2018 both inside and outside PAs.

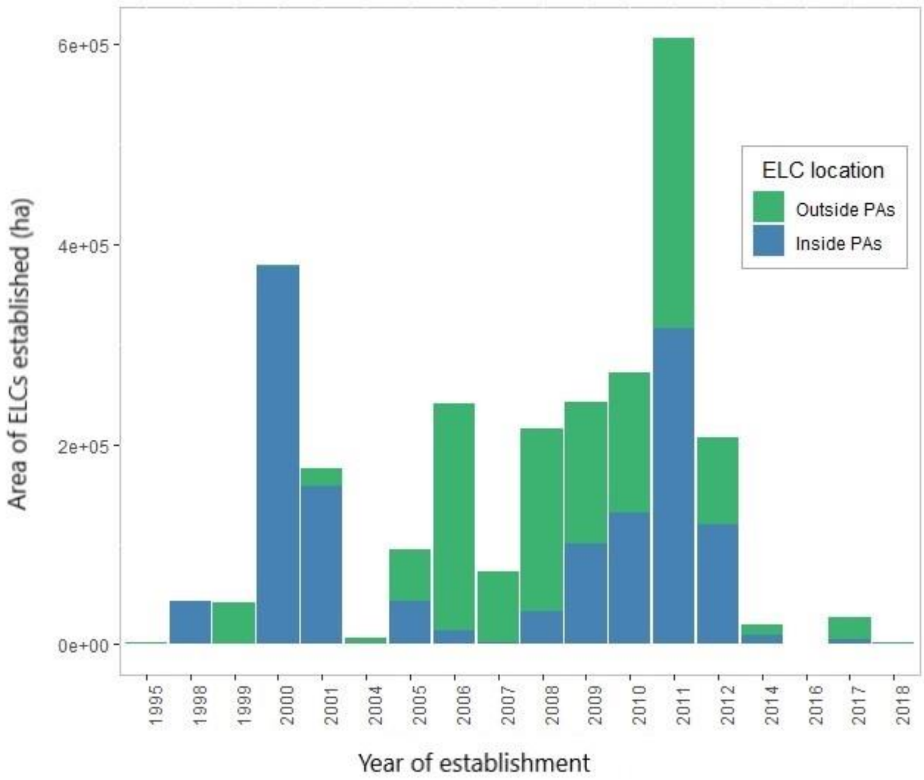


Figure 9: Area of ELC land established in Cambodia per year between 1995-2018 (data sources ODC 2017a; ODC 2019b)

On this basis the proposed analysis should take place in several outcome periods within a total period of analysis between 2010 and 2018. This would not only capture the period of greatest ELC establishment but also encompasses the corresponding trend of increasing FCL loss (Figure 5: section 2.1.3) and allows for investigation of the effect of PFs being re-designated as PAs under the authority of the GDANCP in 2016 (section 2.2.4), something which Ota *et al.* (2020) stressed as having potential implications for overall PA effectiveness.

The use of multiple outcome periods also creates the opportunity to test whether there is a difference in effectiveness associated with PAs established in different periods. There is a clear rationale to investigate this given that the design and efficacy of Cambodia's early PAs has been highlighted as flawed (2.2.2.1) and Clements and Milner-Gulland (2014a) indicate that management activities in many did not begin until NGO's began to provide funding and logistical support around 2005. Whilst previous counterfactual analyses of PA effectiveness in Cambodia have investigated the difference between different types of PA (Ota *et al.* 2020) this has not been done with regards to explicit attributes of PAs themselves such as their age (i.e. duration since establishment). This is an interesting factor to investigate given that other studies in the wider literature have exhibited conflicting results with regards to it being a significant predictor of effectiveness (Section 1.4.1).

This can conceivably be investigated in two ways: firstly 'intra-outcome period effectiveness' i.e. does the period in which PAs were established result in different estimates of effectiveness under similar deforestation pressure? Of course, this comparison is only valid if indeed pressure can be said to be similar across the different groups of PAs. This is further confounded by the fact that the pressure experienced is relative to the size of the PA and it is clear that the earlier PAs (established pre-2000) make up the majority of the currently existing estate (section 2.2.1). On this basis, a better comparison would be how PAs established in different time periods respond to different levels of pressure, i.e. a comparison of inter-outcome period effectiveness. This would mean that the same PAs would be compared across different periods which eliminates the issue of different sample sizes.

Another area warranting further investigation is that of the spillover effects of protection (i.e. PA establishment) for which the two previous studies generated conflicting results (section 2.2.2.2). Here again, the analysis of multiple outcome periods could lead to novel insights as to how spillover effects change over time and relative to the deforestation pressure inside PAs and in the wider unprotected landscape.

Beyond just quantifying PA effectiveness this assessment should also seek to use the biophysical and socio-economic covariates required for the matching analysis to identify whether there are biases that exist in the siting (location) of PAs in Cambodia. This is a feature common to other quasi-experimental investigations of PA effectiveness but something that has yet to be explicitly investigated in the case of Cambodia.

Similarly, given the outcome variable to be investigated is deforestation occurrence this analysis can also be used to shed light on the predictors of this with respect to the covariates employed. Whereas the drivers of deforestation in Cambodia have been highlighted in different contexts by Michinaka *et al.* (2013), Davis *et al.* (2015), Beauchamp *et al.* (2018), and Lonn *et al.* (2018), this study could provide insights into how the predictors of deforestation differ between forests inside PAs and those outside of them and how these have changed over time.

2.3.2 Potential applications of results

Insights related to the effectiveness of the whole PA network over time, as well as whether PAs established in different time periods exhibit different responses to changing pressure, are of clear benefit to conservation practitioners and government PA management authorities in Cambodia. First and foremost, evidence of PAs generating the positive conservation outcome of avoided deforestation could be used to support requests for continued or increased funding from both central government and international donors. This is especially important in light of the previous critiques of PA effectiveness (section 2.2) and waning donor support (section 2.2.4).

This same information can indicate which PAs in particular are facing the highest pressure and thus how management could better allocate their limited resources and budget for management activities such as law enforcement. This is of particular relevance given the changes to PA management structure that occurred in 2016 with the GDANCP now being responsible for all PAs in the country (section 2.2.4). In addition, information concerning the impacts that developments inside PAs have upon their effectiveness (as captured by the changes in coverage of ELCs between the different outcome periods of the analysis) could be useful in shaping future policy regarding further allocations of this kind.

Finally, the results of this analysis also have the potential to contribute to several of the projects being pursued by the RGC with respect to PAs but also in terms of wider natural resource management. Knowing which PAs are more effective as well as the predictors of deforestation both inside and outside of PAs could be used to inform the selection of locations for additional REDD+ projects (section 2.2.4). This is also relevant with regards to further planning for the potential re-introduction of tigers as PAs that are effective in retaining forest cover are likely those that represent more suitable re-introduction sites.

3. Aims and Objectives

3.1 Aims

The two introductory chapters of this thesis have offered distinct and yet harmonious conclusions. The first (section 1.5) highlighting that there still exists a knowledge and implementation gap at the macro-scale in terms of quasi-experimental counterfactual assessments of PA ecological effectiveness. In particular the requirement for further testing and refinement of aspects of the methodology through country-scale studies. The second chapter showed that there is a clear rationale for such an assessment to be performed for the country of Cambodia, with the potential to generate real insights for practitioners and policy makers (section 2.3).

Thus, the overarching aim of this study will be to contribute to both of these directives by performing a quasi-experimental matching methods analysis of PA avoided deforestation in different periods in Cambodia between 2010 and 2018. More specifically this study will investigate:

i. The biases that exist in both the siting of PAs and the occurrence of deforestation in Cambodia expressed by a range of bio-physical and socio-economic variables.
ii. The effectiveness of Cambodia's PAs in terms of avoided deforestation in several outcome periods wherein forest resources were subject to differing levels of extractive pressure.
iii. Whether PAs established in different time periods show trends in effectiveness in response to changing deforestation pressure.
iv. The presence of a possible spillover effect of PA establishment in terms of reduced deforestation in the areas immediately surrounding them.

Of course, it is important to re-iterate at the outset that such an analysis using matching methods requires a substantial number of considerations to be addressed in order to ensure that the results generated are robust and meaningful. Section 1.3.4 described the conceptual basis of these however the link between these and the investigations aims has been codified within the practical objectives in the following section.

3.2 Objectives

In the interest of cogency, the objectives of this study have been divided into three stages: preliminary analysis, primary analysis and secondary analysis. These do not include the processes of data selection and preparation necessary prior to analysis although these are detailed at the outset of the methodology.

3.2.1 Preliminary analysis

The preliminary analysis encompasses the tasks necessary to confirm that matching is an appropriate approach for the main analysis of PA effectiveness, with the specific objectives being to:

i. Determine the optimal length (duration) of outcome period to be used with respect to the inclusion of PAs and other land use types.
ii. Address the considerations highlighted in section 1.3.4 that confirm the appropriateness of matching analysis by testing the validity of a selection of covariates informed by the literature. Covariates should be refined on the basis of their explanatory power with respect to the outcome period, the presence of any multicollinearity between them, and the extent of spatial autocorrelation in the data.
iii. Utilise the results from the process of covariate refinement to identify the biases that exist in both the location of PAs in Cambodia as well as the determinants of deforestation in terms of the suite of bio-physical and socio-economic variables.
iv. Test different techniques of statistical matching to determine which is most appropriate and select a method for calculating treatment effects.
v. Trial the matching approach by analysing whether there is a significant effect of spatial spillover of protection in a 5 km buffer zone surrounding PAs in the different outcome periods.

3.2.2 Primary analysis

The objective of the primary analysis is to use the selection of covariates along with the matching technique refined within the preliminary analysis to perform a nearest-neighbour matching analysis between protected (treatment) and unprotected (control) areas within different outcome periods. The effectiveness of PAs will be quantified as the ATT in terms of the binary outcome of forest cover loss or retention at the end of the outcome period. Any differences in ATT between the different outcome periods will then be discussed in light of changing deforestation pressure, natural resource management policies and other socio-political developments, particularly the impact of the allocation of ELCs within PAs and the change in management structure from FA to MoE (section 2.2.4).

3.2.3 Secondary analysis

The objective of the secondary analysis is to build upon the primary investigation of PA effectiveness by performing additional matching analysis to elucidate whether there are observable differences in effectiveness based upon the periods in which PAs were established. This will utilize the same data as the primary analysis but ATT will be estimated for different periods of PA establishment separately within each outcome period.

4. Methodology

For clarity the methodological description of this study has been sub-divided into the following sections: 4.1 and 4.2 detail the processes of data acquisition and wrangling that were required to create the datasets for the subsequent analysis; 4.3 pertains to the objectives of the preliminary analyses as described in section 3.2; whereas 4.4 and 4.5 describe the primary and secondary analyses respectively. For conciseness each of these sections in the main text will highlight only summary information with references to appendixes containing further details as well as any preliminary results required in order to inform subsequent processes. Finally, section 4.6 acknowledges the methodological considerations made to ensure that the results of this study constitute reproducible and open science. In this regard it is important to highlight that all statistical procedures and analysis were performed in RStudio version 3.6.1.

4.1 Source selection and data acquisition

It is important to state at the outset that the unit of analysis or resolution that was chosen for this study was 30x30m spatial pixels, with the rationale being that this is the highest resolution data that is freely (non-commercial) available for the outcome variable of forest cover dynamics. Additionally, as noted by Blackman (2013) the utilization of a 'plot' level unit of analysis such as this is preferable to the coarser alternative of analysing at the level of whole PAs as it negates the requirement for extrapolating covariate values which may introduce aggregation bias.

The following subsections will precede stepwise through each variable in the analysis, highlighting the rationale for their inclusion, the data sources that were chosen and what re-structuring was required. It is worth noting that all data management processes were performed in either ESRI's ArcMap 10.2.2 or Quantum GIS (QGIS) 3.12.0 and to ensure consistency all spatial data was projected using the World Geodetic System 1984, Universal Trans Mercator zone 48 north (the correct zone for Cambodia; WGS 1984 UTM 48N) coordinate reference system.

4.1.1 Treatment/control assignment (independent variable)

4.1.1.1 Assignment to treatment

In the case of this study, assignment of a unit of analysis to the treated group (as part of the primary analysis) was dependent on two conditions: i) the unit displayed forest cover in the first year of the outcome period, and ii) the unit was contained within the borders of a PA established prior to the first year of the outcome period excluding any land within PAs assigned to other tenure types (ELCs). The first condition required the same data as the outcome variable (forest cover dynamics) and hence to avoid repetition will be discussed only in section 4.1.3. The second condition required data of the boundaries and extents of all PAs in Cambodia established prior to and within the total analysis time period of 2010-2018. In this regard, other studies have opted to utilise the UNEP-WCMC's WDPA (Bowker *et al.* 2017; Abman 2018). However, in the case of Cambodia this source is substantially outdated following the institutional re-arrangements in 2016 (section 2.2.4). By comparison the most up to date dataset of PA boundaries in Cambodia is ODC's 'natural protected areas (1993-2019) dataset' (ODC 2019b) although this too has its limitations. Hence for this study a new dataset was synthesised by selectively combining information from the two sources and filtering the records to suit the purpose of the analysis. The details of this process are described in Appendix C with the result being a dataset of 51 PAs containing their best-known boundaries and extents (area) along with their date of establishment. This dataset was dubbed 'filtered PAs' and Table 1 below shows the number of PAs of respective designations included with a full list of PAs by name in Appendix C.

Table 1: Numbers of protected areas contained in the dataset produced for this study

Category	Total
Multiple Use Management Area	5
National Park	11
Natural Heritage Site	2
Protected Landscape	12
Ramsar Site	2
Wildlife Sanctuary	19
Grand Total	51

From this additional spatial dataset was created of the filtered PAs extents minus that of any ELCs that were either contained within them or overlapped their borders. The rationale for this is described in section 2.2.1 although in summary ELC land *de facto* cannot be considered as protected as its purpose is for agricultural development. This was done by 'clipping' the spatial polygons of the PAs using the ELC spatial data that will be highlighted in section 4.1.3, with the resulting dataset being dubbed 'PAs minus ELCs'.

4.1.1.2 Assignment to control

In the case of this analysis the control region can essentially be thought of as land not managed by the relevant authorities of the RGC for the purpose of nature conservation, or what could be more loosely referred to as 'unprotected land'. However, conceptually identifying this is problematic as Blackman (2013) notes that no land is explicitly demarcated as non-protected by governments and hence the decision as to how to define unprotected land is largely subjective upon the investigator.

Ultimately the decision was made that the unprotected control region should represent all land managed by any means apart from the types of nationally recognised protected areas identified in Table 1 above (also Appendix C), with the rationale for this discussed in detail in Appendix D. In practical terms this meant that the boundaries and extent of the control region were identified by taking a polygon of Cambodia's border and clipping it using the polygon layer of 'PAs minus ELCs' (Section 4.1.1.1). Hence for units to be assigned to control they had to be present inside the control region and display forest cover in the first year of a given outcome period.

4.1.2 Source for outcome (dependent) variable

The outcome or dependent variable for this analysis was binary in nature, namely whether or not a given unit remained forested at the end of the outcome period or if it was deforested at any point within the outcome period. Obviously, this requires a source of data for both forest cover extent and forest cover loss at an annual level. As mentioned previously, the forest cover extent data was also necessary to identify units suitable for either assignment to treatment or control groups (sections 4.1.1.1 and 4.1.1.2).

Appendix B detailed that there are a number of FCAs that have been completed for Cambodia by both national and international organizations that could possibly serve as data sources. Of these the nationally produced estimates were ruled out on the basis of both data availability and insufficient temporal coverage. This left a decision between the global scale data from GLAD or the regionally produced data from the SERVIR-Mekong project, with the latter being chosen as the preferred source on the basis of it's supposed greater accuracy (Appendix B).

SERVIR-Mekong's forest monitor system (SERVIR-Mekong 2020b) allows users to specify a definition for forest cover with respect to tree height and % canopy crown cover. In this regard it was decided to adopt that of the UNFCCC's REDD+ scheme (which is also used by the RGC (GDANCP 2018)) which defines forest as "a unit of an ecosystem in the form of wetland and dry land covered by natural or planted vegetation with a height from 5 metres on an area of at least 0.5 hectares, and canopy crown cover of more than 10%." (JICA 2017*, p.8*). Once this had been specified the data for forest cover extent and loss for all of the years required under the different outcome periods was downloaded directly.

At this stage it is important to highlight the issue of validation with respect to this data, which is of course a crucial consideration for all land cover assessment and particularly forest cover. There are several methods of validating remotely sensed forest cover data: first it can be 'ground-truthed' by comparing it against data collected *in situ* from a number of field sites although given the nature of this study this was obviously not an option. Alternatively, it can be validated against other data produced for the same region but as Appendix B detailed there are clear discrepancies between the major sources available for Cambodia and so this approach would itself be contentious. Finally, it can be compared against other remotely sensed data relying on different technologies such as synthetic aperture radar (Singh *et al.* 2018), although this was deemed too time-consuming for the scope of this project.

Instead, the decision was made to perform cursory validation of the SERVIR-Mekong FCL data by cross checking it against forest cover data for the final year of the outcome period. The rationale for this was that if a given unit has supposedly experienced a FCL event within the outcome period, given that the data resolution is 30x30m patches, then it is reasonable to expect that the unit will show an absence of forest cover in the final year of the outcome period. Vice versa if a unit did register a FCL event and still displays forest cover in the final year of the outcome period it is likely to be a classification error. As the nature of the misclassification is unknown the unit should then be discounted from the sample. In reality however this approach to validation was not successful as there were a substantial number of apparent contradictions between the forest cover and FCL dataset. The most likely reason for this was not that the data itself is inaccurate rather the segregation into annual time periods does not take into account that datasets may have different start/end points of classification.

4.1.3 Selection of covariates/confounders

Reviewing the wider literature of quasi-experimental assessment of PA avoided deforestation highlighted a core set of covariates found to be significant by large numbers of studies (section 1.4.2). This selection was then bolstered by several additions prompted by the Cambodia specific literature regarding deforestation and PA effectiveness (sections 2.1 and 2.2) to give a preliminary set of covariates for which there is sufficient evidence to suggest play a causal role in either the siting of PAs and/or the occurrence of deforestation in the country.

The next stage was to source representative data for this selection of covariates in light of the constraints of the matching methods technique. Namely that, similar to the variable controlling assignment to treatment, the data should ideally originate from, prior to, or within the first year of a given outcome period. This factor along with general data availability and evaluation of data quality led to some narrowing of the initial covariate selection, details pertaining to which are included in Appendix E.

The final selection of preliminary covariates along with the rationale for their inclusion (causal relationship), and the data processing completed in order to make them relevant for the subsequent analysis, have been detailed in Table 2 below (note that the distance to ELC covariate required more extensive data wrangling which has been detailed in Appendix F). The use of each of these covariates is extensively supported by the wider literature but for the sake of repetition the citations are not included in Table 2; instead the readers should refer back to Table A1 Appendix A. However, references for the applicability of covariates in the context of Cambodia specifically have been included where relevant. Finally, it is worth highlighting that this represents the selection of covariates prior to further refinement within the preliminary analysis (section 4.3.1).

Table 2: Details of the preliminary selection of covariates

Variable designation	Covariate or confounder?	Contextual causal relationship (with Cambodia specific reference where applicable)	Data source/s	Data wrangling/processing
Distance to surrounding FCL	Confounder	Units have a higher probability of being deforested if deforestation has occurred recently in close proximity to them (Beauchamp *et al.* 2018). PAs are more likely to be sited away from areas of deforestation	Forest cover loss extent (SERVIR-Mekong 2020b)	Data for FCL occurring in the two years prior to each outcome period (2008-2010; 2011-2013; 2014-2016) downloaded, converted to point data and processed using distance matrix tool to establish mean Euclidean distance between unit of analysis and the nearest 10 FCL events from the preceding two years.
Distance to international land borders	Covariate of FCL	Extensive reporting that forested regions have been subjected to cross-border (Thailand and Vietnam) illegal logging and timber transit (ICEM 2003b; WWF 2012; Singh 2014; EIA 2017)	Cambodia administrative boundaries (GADM 2018)	Polygon of Cambodia's land border transformed into line format, border to ocean removed and Euclidean distance (30x30m) to borders layer produced.
Distance to ELCs	covariate of FCL	Substantial evidence of ELCs conducting illegal logging outside of their borders (Global Witness 2009; Clements *et al.* 2014). Hence deforestation is more likely to occur in close proximity to them (demonstrated by Davis *et al.* 2015; Beauchamp *et al.* 2018; Magliocca *et al.* 2020)	ELC boundaries (combination of ODC (2017a) and LICADHO (2020)).	Dataset of filtered ELC boundaries (see Appendix F) temporally partitioned and Euclidean distance layer at 30x30m resolution produced for each.
Soil type	Confounder	PAs often sited on land of poor productivity which is typified by certain soil types. Deforestation is more probable on land of high agricultural suitability (Michinaka *et al.* 2013) again exemplified by particular soil types	Map of distribution of soil types ODC (2020a)	Data projected using standard project coordinate reference system (CRS), converted to raster format for ease of intersection with sample/control point data

Average annual temperature	Covariate of FCL	Both of these variables are correlated with the agricultural suitability of land which Michinaka *et al.* (2013) found to be a significant predictor of FCL in Cambodia. However, the nature of the relationship is expected to differ depending on the type of cropping that is occurring.	Average annual temperature between 1970 and 2013 (Karger *et al.* 2017)	Data reprojected to project CRS maintaining native resolution of $1km^2$
Average annual precipitation	Covariate of FCL		Average annual precipitation between 1970 and 2013 (Karger *et al.* 2017)	Data reprojected to project CRS maintaining native resolution of $1km^2$, unit is mm/year
Distance to major roads	Confounder	PAs are typically located further away from major roads. Deforestation is more likely to occur closer to major roads (ICEM 2003a; Broadhead and Izquierdo 2010; Ota *et al.* 2020) as these provide access and are required in order to transport the product to market. Although Lonn *et al.* (2018) observed the opposite i.e. that probability of deforestation was reduced with proximity to roads in Cambodia. However, there is the problem of endogeneity where areas are deforested specifically to allow for the construction of roads.	Locations of major roads in Cambodia from commune database in 2011 (ODC 2019c)	Distance to major and minor roads was trialled however the extent made the proximity calculation ineffective even at a high resolution. Time variant road data was also trialled although they were inconsistencies between the sources and hence it was elected to use Euclidean distance to primary/major roads only (coded as highway, national road or arterial) from 2011 data.
Distance to provincial capital	Confounder	PAs typically sited in areas of low human population ergo further from capitals. Provincial capitals are the best available proxy for population centres in Cambodia given scarcity of census data. The products of deforestation i.e. timber is likely to pass to market in provincial capitals and hence a reduced travel distance to these is preferable for those engaged in forest clearing (Ota *et al.* 2020).	Location of provincial capitals in Cambodia (MLMUPC RGC 2008)	Location of provincial capitals is essentially time invariant and hence the single data layer was used for all 3 outcome periods. Point data of locations was convert to raster with a resolution of $5000m^2$ for the capitals then a Euclidean distance layer was produced at 30x30m resolution.

Surrounding human population	Confounder	PAs typically sited in areas of low human population. Heterogenous effects expected for deforestation occurrence: conversion for agriculture more likely to occur in proximity to populations, clearance for timber in locations of lower population density. Tested by Ota *et al.* (2020)	LandScan (2020)	Landscan estimated population count layers downloaded for years 2010, 2013 and 2016 at 30x30m (900m^2) resolution. Clipped to extent of Cambodia, Layers re-sampled using sum values to give estimated population (no. of people) per 5000m^2.
Elevation	Confounder	PAs typically sited in areas of higher elevation, as evidenced by the fact that many PAs in Cambodia are situated in the North-Eastern provinces or Mondulkiri and Ratanakiri and the Cardamom mountains (Figure 7) Unclear relationship with deforestation occurrence: Low elevation regions typically converted to agriculture earlier meaning they have a higher probability of deforestation (Lonn *et al. (*2018) observed this relationship in Cambodia). But this leaves behind primary forest desirable for deforestation at higher elevations.	Shuttle Radar Topography Mission (SRTM) Digital Elevation Map (DEM) dataset for Cambodia (Aruna Technology Ltd. 2020)	No processing required data projected using standard project coordinate reference system at 30x30m resolution
Slope	Confounder	PAs typically sited in areas of higher slope, largely because this is correlated with elevation changes Areas of higher slope are harder to deforest and less suitable for agricultural conversion. Confirmed by Lonn *et al.* (2018) who found higher deforestation probability at lower slope % in Cambodia.	Calculated from Aruna Technology Ltd. (2020)	Slope calculated from elevation data using the GDAL slope algorithm with a 1:1 ratio of vertical units to horizontal projected at 30x30m resolution

4.2 Data exploration and preparation

4.2.1 Defining outcome periods for analysis

Although section 2.3.1 conceptually delineated the outcome periods to be analyzed based on when specific policies and events occurred, these needed to be refined given the specific restrictions of the matching methods approach. Namely, that treatment and control groups are identified on the basis of data for variables up to and including the first year of the outcome period only. For example, in selecting an outcome period to best represent the time of maximum ELC establishment (likely to be a strong confounder of PA effectiveness) the start date had to follow the years of highest establishment to ensure maximum inclusion of records. As for determining the duration of the outcome periods, whilst it would have been ideal to perform the analysis at an annual level this was not achievable given the scope of this study. Hence there was a trade-off between selecting outcome periods that begin at an appropriate time and are of sufficient cumulative duration to cover the whole temporal period of 2000-2018. After testing the amount of both PA and ELC land that would be excluded (omitted) from the analysis under different outcome periods (described in Appendix G) the decision was made to utilize 3-year outcome periods divided as follows: 2010-2012; 2013-2015; and 2016-2018.

4.2.2 Creating relational datasets

In the case of this analysis the term ‘relational dataset’ is used to refer to a dataset containing the spatial location of all units of analysis along with their status as either a treated or control unit, their outcome (forested; deforested) in the final year of the outcome period and the data for all of the covariates. Separate datasets were required for each of the outcome periods under the primary/secondary analysis along with an additional three for the spillover analysis as part of the preliminary investigation. The process for creating all of the datasets was similar as all required the identification of treatment and control populations, achieved through the temporal partitioning of the variables for both treatment assignment and the outcome. These processes are described in Appendix H along with the subsequent steps of sampling the treated and control populations in order to make them feasible for analysis.

4.3 Preliminary analysis

4.3.1 Refinement of covariates

As highlighted in section 1.3.4.2 there are two primary concerns in confirming the validity of the covariates chosen in matching methods analysis. Firstly, that the hypothesised covariates are significantly correlated with the outcome (dependent variable) and secondly that there is a region of common support (sufficient overlap) between their variance with respect to the treated and control groups (the second assumption under SITA: section 1.3.4).

To address the first of the concerns, it was decided to follow the technique of Schleicher *et al.* (2017) by creating generalized linear models (GLM) initially including all covariates and using stepwise model selection to iteratively test which combination of predictors provided the most explanatory power or best 'model fit' based upon Akaike Information Criterion (AIC) scores.

However, this method of covariate refinement alone is not sufficient as it doesn't explicitly assess whether any multicollinearity exists between the variables. The presence of multicollinearity is important to test as it relates back to the first assumption of unconfoundedness under SITA (section 1.3.4). This was tested through calculation of variance inflation factor (VIF) which informed whether the removal of any additional covariates from the final selection was necessary prior to matching.

With regards to the second concern of confirming overlap between covariate distribution between the control and treatment groups, the 'bal.tab' function in Greifer's (2020a) Cobalt package for R was used to calculate a range of summary statistics for the pre-matching covariate distribution and hence show that there was an appropriate overlap. Detailed information on the methods and results for all these processes are included in Appendix I.

4.3.2 Identifying biases in PA location and predictors of deforestation

The process of quantifying the covariate distributions between treated and control groups also served the dual purpose of contributing towards addressing an additional objective of the preliminary analysis (section 3.2.1), namely identifying the biases that exist in PA siting. Appendix I.3 details the summary statistics produced for each of the covariates, such as the mean and SD, which were useful in helping elucidate the nature (direction) of the biophysical and socio-economic biases that exist. Whereas the standardised mean difference values between treated and control groups (Appendix I.3) in addition to visualisations of the smoothed density distributions of each covariate (also produced by the Cobalt package (Greifer 2020a)) were used in order to make inferences about the 'extent' of these biases.

The same process was used to address the objective of characterising the predictors of deforestation occurrence in Cambodia, by instead investigating the difference in covariate distributions in term of the dichotomous outcome (dependent variable) of forest cover retention or FCL at the end of each outcome period.

However, making inferences with respect to either of these objectives on the basis of the covariate distributions alone is somewhat limited as it does not indicate which factors correspond most strongly to the variance exhibited by the different groups. For this reason, principal component analysis (PCA) was used to capture this concept in an intuitive form. In simple terms PCA is a dimensionality reduction technique that involves transforming predictors into hypothetical linearly un-correlated variables known as 'principal components' (PCs) that explain a defined percentage of the variance in the sample (Dytham 2011; Zuur *et al.* 2007). The extent to which each of the original predictors 'load' upon the PCs gives an indication of how they contribute towards variance and in this respect, it is also possible to map the contribution of individuals units in the sample. Both of these interactions can be visually represented in biplots which map the variables and individuals in 2 dimensions with the 1st and 2nd PCs representing the X and Y axes.

In the case of this study PCAs for both the predictors of PA location and deforestation occurrence were conducted using the 'prcomp' function in the R stats package (R core team 2020) with the data centred and scaled. Following this PCA biplots were produced using the R Factoextra package (Kassambara and Mundt 2020). As these results represent a key finding for this study they are presented as part of the results section of the main text (sections 5.1.3 and 5.1.4).

4.3.3 Testing for spatial autocorrelation

Section 1.3.4.4 highlighted that the phenomenon of SAC has the potential to adversely affect the results of the matching methods analysis. Thus, it is pertinent to assess the extent of SAC in the data prior to matching especially as it can also influence the results of covariate selection through the use of regression models such as the GLMs used in this study. In this regard the presence of SAC was investigated by first visualizing the residuals of the GLMs in a spatial context and then following up with statistical confirmation using the Moran's I test. The outcome of this precipitated the need to produce new GLMs including the spatial location of the data units to account for SAC and re-affirm the final selection of covariates to be used in matching. Again, because the results of these processes were critical in informing the methodology of the primary analysis, they are presented in more detail in Appendix J.

4.3.4 Selecting matching method

Prior to conducting the analysis for the spillover effects of protection (section 3.2.1) as well as the primary and secondary analyses (section 3.2.2), it was necessary to test different matching approaches to determine which was most appropriate. The rationale behind this was two-fold: first, to test what size of samples could feasibly be analyzed computationally and second which matching technique resulted in the best 'quality' of matches.

In this regard two different methods were tested using the R package: Matching (Sekhon 2020): Propensity score matching (PSM) and covariate matching using the Mahalanobis distance (MDM). For both methods matching was conducted using the selection of 10 covariates refined in sections 4.3.1 and 4.3.3; on a 1:1 (treated: control) nearest-neighbour basis; with replacement of units, no deterministic handling of ties; and a caliper of 0.5 SDs to exclude inferior matches (section 1.3.4.3).

Initially these approaches were tested using the full samples of control and treated (treatment: located within PAs) units from the 2010-2012 outcome period, however this was computationally unfeasible. This led to the testing of different sizes of sub-samples including 10%; 20% and 30% random samples, with the Cobalt package (Greifer 2020a) again used to produce statistical summaries of the covariate distributions to assess the quality of matching (section 1.3.4.3) under each technique. As the results of this process were decision relevant to the subsequent analysis they are presented in detail in Appendix K however the primary conclusion was that the most appropriate matching technique for this study context was PSM using samples not to exceed ~100000 units with a ratio of approximately 1:3 treated to control units.

With the matching approach finalised the next decision was how the matched sample produced would be used to quantify the effect of the treatment. More specifically, given the nature of the treatments being investigated, this meant producing an estimate of ATT (section

1.2.3). As alluded to in section 1.3.3 there are two widely used approaches to achieving this: The first option is to estimate ATT directly from the matched sample by "averaging the within-match differences in the outcome variable between the treated and the untreated units" (Abadie and Imbens 2006). Whereas the alternative is to estimate ATT by fitting an appropriate regression model to the matched data with treatment assignment specified as an independent variable (Pan and Bai 2015; Leite 2017).

In the case of this study the decision was made to follow the former strategy largely because this is the means of ATT estimation that can be specified to be produced directly by the 'Match' function in the Matching package (Sekhon 2020). However, Abadie and Imbens (2011) note that this form of ATT estimation is prone to bias when matching is not able to produce perfect covariate balance. To account for this, they developed a technique to produce ATT estimates conditioned for this bias by linearly regressing the outcome (dependent variable) onto the covariates of the matched units (Leite 2017). Given that the testing of the chosen matching approach (PSM) for this study did not result in perfect covariate balance (Appendix K) it was deemed pertinent to specify the 'Match' function to calculate Abadie-Imbens bias-adjusted ATT estimates for the all subsequent matching analysis. Similarly, the function was specified to report bias-adjusted estimates of SE under the Abadie-Imbens variance formula (Abadie and Imbens 2011).

4.3.5 Testing for unobserved covariates

Section 1.3.4.2 highlighted the need to test whether the results of matching analysis are susceptible to the influence of unobserved 'missing' covariates the inclusion of which would explain more of the variance between the treated and control groups. If this effect proves to be substantial it is a clear indication that the conceptual model of what determines assignment to treatment is flawed and should be reassessed to ensure that treatment effect estimates are robust.

On this basis the decision was made to test for unobserved variance prior to conducting the actual matching analysis by using the results generated as part of the process of trialling different matching approaches (Section 4.3.4). The most widely used test for unobserved variance in this context is the Rosembaum bounds sensitivity analysis which was implemented using R package: rbounds (Keele 2014). A table of the results of this analysis for the 10% random sample of the 2010-2012 outcome period under the finalized PSM approach has been included in Appendix L. Although the conclusion was that the matching model was robust (not susceptible) with respect to unobserved covariates and did not require adjustment.

4.3.6 Investigating spatial spillover effect of protection

To re-iterate, the purpose of this analysis is to identify the presence, extent and nature of spillover effects from the establishment of PAs in Cambodia (sections 1.3.4.4 and 3.2.1). To this end section 4.2.2 and Appendix H have already detailed how samples of treated units from 5km buffer zones surrounding PAs and control units from the wider unprotected landscape were created for each outcome period. The sizes of these samples are detailed in Table 3 (section 5.1.1) however section 4.3.4 made it clear that the size of these full samples was too large to be feasibly matched. Thus, the decision was made to analyse four sub-samples for each outcome period, containing 100,000 units, made up of random samples of 25,000 treated units and 75,000 control units (1:3 ratio). The rationale behind this being that the combined total of these sub sample would equate to ~12% of the total population in each outcome period (See Table 3). These sub-samples were subjected to PSM under the specifications highlighted in section 4.3.4 in order to compare deforestation occurrence within the treated and control groups. The bias-adjusted ATT estimates and their SE's generated by each sub-sample were then averaged to produce an estimate of avoided deforestation in each outcome period. Following this the quality of matching in terms of the improvements in covariate balance was investigated using summary statistics produced by the Cobalt package (Greifer 2020a) as described in Appendix I.3.

4.4 Primary analysis

As specified in section 3.2.2 the purpose of the primary analysis was to estimate the effectiveness of PAs in Cambodia in terms of the outcome of forest cover loss vs. retention observed for forested (treated) units within PA boundaries as compared to those in the wider unprotected (control) region. Similar to the spillover analysis, this was achieved by calculating the average bias adjusted ATT and SE of a number of matched sub-samples of the data using the matching approach defined under section 4.3.4. In this case 12 sub-samples were used for each outcome period to effectively represent the 10% samples of the populations (See Table 3 in section 5.1.1). Again, these sub-samples were randomly generated, following the previously used standard of 100,000 units total consisting of 25,000 treated units and 75,000 control units.

The requirement to conduct sub-sampling in order to perform matching was possibly beneficial in the sense that random sampling has the potential to reduce the presence of SAC in the data. In order to determine whether this was actually realised the residuals of a GLM produced for the post-matching data of 1 randomly selected sample from each outcome period were tested using the Moran's I test. As per the spillover analysis the quality of matching was assessed by calculating summary statistics related to the covariate balance in the matched and unmatched samples.

Finally, in order to provide context for the discussion of the estimated treatment effects, the spatial data of forest cover and FCL events (see section 4.1.2) was used to calculate relative deforestation rates for the treated and control regions in each outcome period to give a general indication of deforestation pressure.

4.5 Secondary analysis

As per the specific objective in section 3.2.3 the purpose of the secondary analysis was to compare the effectiveness of PAs that were established in different periods between the 3 outcome periods. In practical terms the first step was to split up the datasets of treated units used for the primary analysis into separate datasets of units from PAs established in different time periods (categories). For the 2010-2012 outcome period these categories were: PAs established prior to the year 2000 (i.e. between 1993-2000); and those established between 2001-2010. These same 2 categories were carried over to the 2013-2015 outcome period with the intention of using a 3rd category of PAs established between 2011-2013 however this was not possible given that were none established in this time period. Finally, for the 2016-2018 outcome period the first 2 categories were used (1993-2000; 2001-2010) with the addition of a 3rd category of PAs established between 2011-2016.

A table of the relative numbers of treated units in each of these categories within each outcome period is included in the results (Table 7: section 5.3). Again because of the total number of units, it was necessary to perform the matching analysis on sub-samples taken from each category. This was done using the same sampling and matching protocol as the spillover and primary analyses although the number of sub-samples matched for each category were not uniform (detailed in Table 7).

Following matching, treatment effect estimates were produced along with covariate balance statistics for each category of PA establishment date. Finally, to offer validation for the inter-outcome period comparison the relative deforestation pressure faced by each category of PAs was again calculated using the spatial data of forest cover and FCL events (see section 4.1.2).

4.6 Ensuring Reproducibility

Given that this investigation relies on a substantial number of data sources and processing procedures as well as statistical techniques reproducibility is a key concern. The purpose of making this analysis reproducible is not just to demonstrate robustness of results by allowing them to be tested by others but also to help in propagating the techniques used to a growing audience.

To this end all of the spatial data used in this investigation (including the datasets created by filtering open access sources) has been formatted as a 'Geopackage' database including the accompanying QGIS project file and is available through the author's GoogleDrive upon request (Black 2020a). Similarly, all of the R scripts used for the statistical analysis have been made publicly available through the author's GitHub repository (Black 2020b).

5. Results

The following section will be subdivided in accordance with the objectives and methodology with sequential sections pertaining to the results of the preliminary analysis; primary analysis; and secondary analysis.

5.1 Preliminary analysis

5.1.1 Sampling of control and treated populations

Table 3 below shows the population and sample sizes of treated and control units, for both the primary investigation of PA establishment as treatment as well as the spillover analysis. There are few relevant observations that can be drawn from this other than the fact that the ratio of treated to control units makes it evident that there were sufficient numbers to allow for comparison using matching. It is notable that the number of treated units in the 2016-2018 outcome period did exceed that of the controls, which of course is to be expected given the expansion of the PA estate (section 2.2.1) and the trend of ongoing forest cover loss (section 2.1.3). However, given that matching was specified with replacement of units this was not an issue.

Table 3: Population and sample sizes for the primary and spillover analyses

<table>
<tr><th colspan="6">Primary analysis</th></tr>
<tr><th rowspan="2">Outcome period</th><th colspan="2">Population (no. units)</th><th colspan="3">10% random sample (minus NA records) (no. units)</th></tr>
<tr><th>Treated</th><th>Control</th><th>Treated</th><th>Control</th><th>Total</th></tr>
<tr><td>2010-2012</td><td>3579236</td><td>5767875</td><td>353822</td><td>573781</td><td>927603</td></tr>
<tr><td>2013-2015</td><td>3111479</td><td>5380047</td><td>307486</td><td>534417</td><td>841903</td></tr>
<tr><td>2016-2018</td><td>4158821</td><td>3769179</td><td>413223</td><td>374099</td><td>787322</td></tr>
<tr><th colspan="6">Spillover analysis</th></tr>
<tr><th rowspan="2">Outcome period</th><th colspan="2">Population (no. units)</th><th colspan="3">10% random sample (minus NA records) (no. units)</th></tr>
<tr><th>Treated</th><th>Control</th><th>Treated</th><th>Control</th><th>Total</th></tr>
<tr><td>2010-2012</td><td>906479</td><td>4861396</td><td>90004</td><td>483777</td><td>573781</td></tr>
<tr><td>2013-2015</td><td>790271</td><td>4594883</td><td>78443</td><td>455974</td><td>534417</td></tr>
<tr><td>2016-2018</td><td>723189</td><td>3045990</td><td>72235</td><td>301866</td><td>374101</td></tr>
</table>

5.1.2 Conclusions of covariate refinement and testing of spatial autocorrelation

For clarity it is pertinent to recap the important conclusions from the processes of covariate refinement and SAC testing the results of which were presented in Appendixes H and I. First, stepwise model selection and multicollinearity testing demonstrated that all of the covariates identified in the preliminary selection (Table 2 in section 4.1.3) with the exception of average annual temperature are appropriate as predictors of the dependent variable (forest cover change at the end of the outcome period). Second, there is sufficient overlap between the distributions of these covariates with regards to the treated and control groups across the different outcome periods to meet the assumption of SITA (section 1.3.4). Thus, this selection of covariates was appropriate to be carried forward to the subsequent stages of preliminary analysis.

The presence of SAC was confirmed in the data for the 2010-2012 outcome period through visual inspection of the model residuals in a spatial context as well as through a significant result in the Moran's I test. Under this conclusion the GLM of the 10 refined covariates as predictors was re-tested in two forms, first with the inclusion of the unit's X/Y coordinates and second with an autocovariate representation of SAC. These models did alter the significance of one covariate in each case however given that a subsequent Moran's I test of the residuals of the autocovariate model showed that it still showed significant SAC the decision was made to retain all of the covariates for the matching analysis.

5.1.3 Identification of biases in PA siting

At the outset is important to acknowledge that the inferences that can be made regarding biases in PA location in Cambodia from the results of this study are limited by its conceptual delineations. The most significant of these being that all of the units analysed were explicitly chosen on the basis of being forested at the start of each outcome period. Thus, these inferences should be viewed more specifically as the differences between forested land located inside PAs and in the wider unprotected landscape. Although of course given the concentration of forest cover in PAs this is still somewhat generalisable to wider conclusions.

The first result to be discussed is the summary statistics of the covariate distributions for the treated (protected) and control (unprotected) samples prior to matching. These have already been included as Table I4 in Appendix I.3 and so the reader should refer to this. The mean values of the covariates in Table I4 show that in general forested units in PAs were found at greater distances from surrounding forest cover loss, provincial capitals, and major roads as compared to unprotected forest. They were also situated in areas of higher average elevations and slope with greater average annual precipitation. Conversely PA forest was on average located closer to ELCs and land borders in areas of lower human population than unprotected forest. The values of standardised mean difference between the treated and control samples for the majority of covariates were fairly consistent (between ~0.3-0.5), with some of the lowest values being soil type (average of -0.279 across the three outcomes periods) indicating that it did not vary strongly between the treated and control groups. Vice versa elevation shows markedly higher standardised mean difference values (0.77-0.81) which suggests that it varied the most strongly.

These trends are also reflected in the values of the variance ratio, KS statistic and COC, in particular the latter two for which the majority of results are <0.3 (with 0 indicating perfect balance between groups). However, in order to get a better picture of the how the variance of the covariates differ away from the mean it is necessary to refer to plots of the smoothed density distributions, which are shown for each covariate for the 2010-2012 outcome period in Figure 10 below.

The density plots in Figure 10 largely corroborate the inferences drawn from the summary statistics above, especially the notion that most covariates do not differ strongly between PA and unprotected samples (i.e. there is substantial overlap). Although the plot for distance to ELCs gives a better insight into the fact that whilst PAs had a lower mean distance to ELCs there was a substantially greater proportion of control units at intermediate distances from ELCs (~50,000-75,000). This already suggests that perhaps the influence of this covariate on the outcome is not straightforward to elucidate.

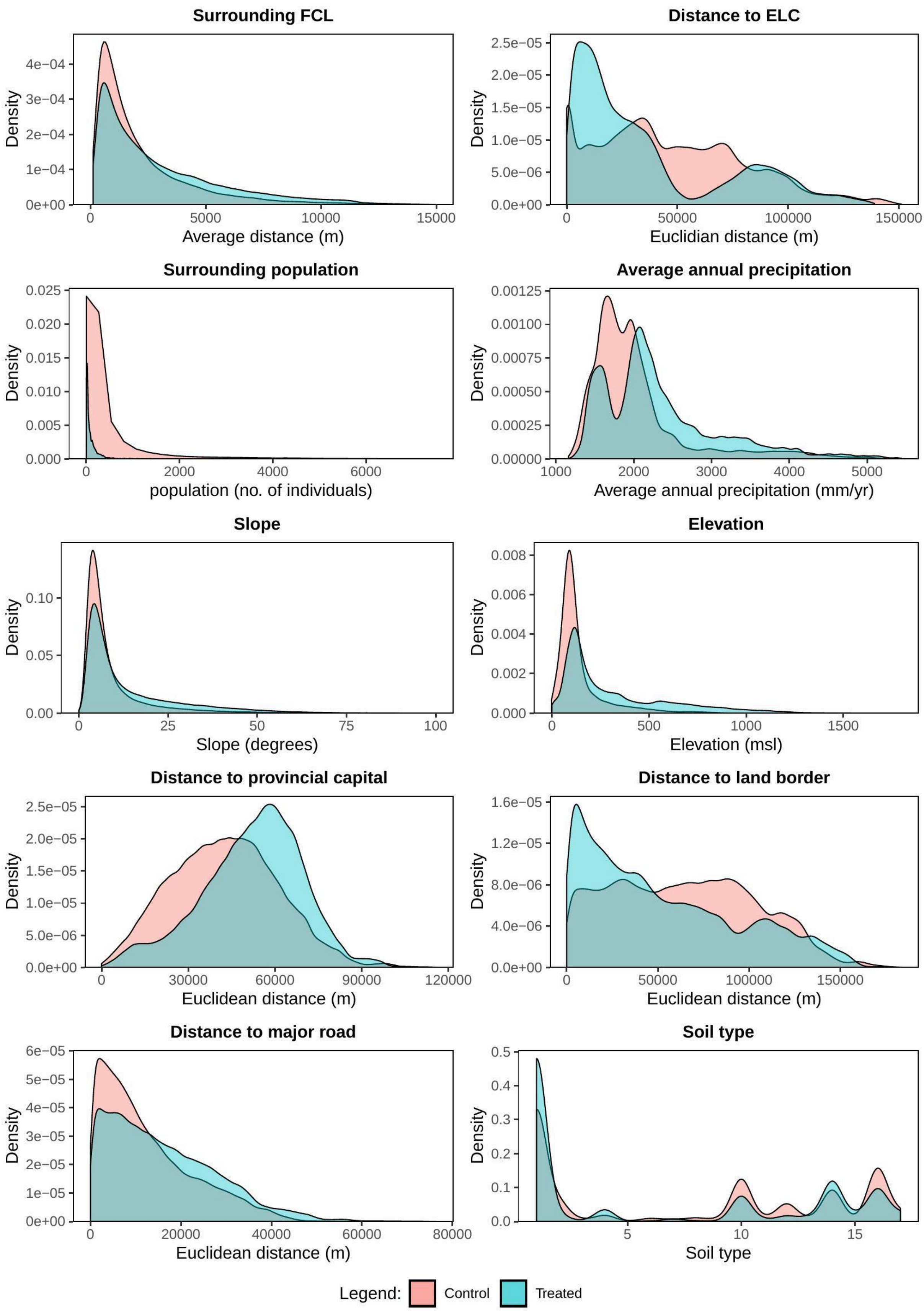


Figure 10: Smoothed density distributions of the covariates for treated and control samples in the 2010-2012 outcome period prior to matching

Given the observation thus far that very few of the covariates vary particularly strongly with respect to the treated and control groups the use of PCA becomes particularly pertinent in determining which factors are responsible for the majority of the variance. Figure 11 below contains the PCA biplot for the 2010-2012 outcome period with the treated and control units distinguished by colour and marker shape. From the X and Y axes labels we can see that the first 2 principal components (PCs) explain a total of 42% of the variance. Whilst it would be preferable for them to explain the majority of variance (>50%) this is still sufficient to allow for some inferences to be made about which covariates are the most influential in characterising the two groups.

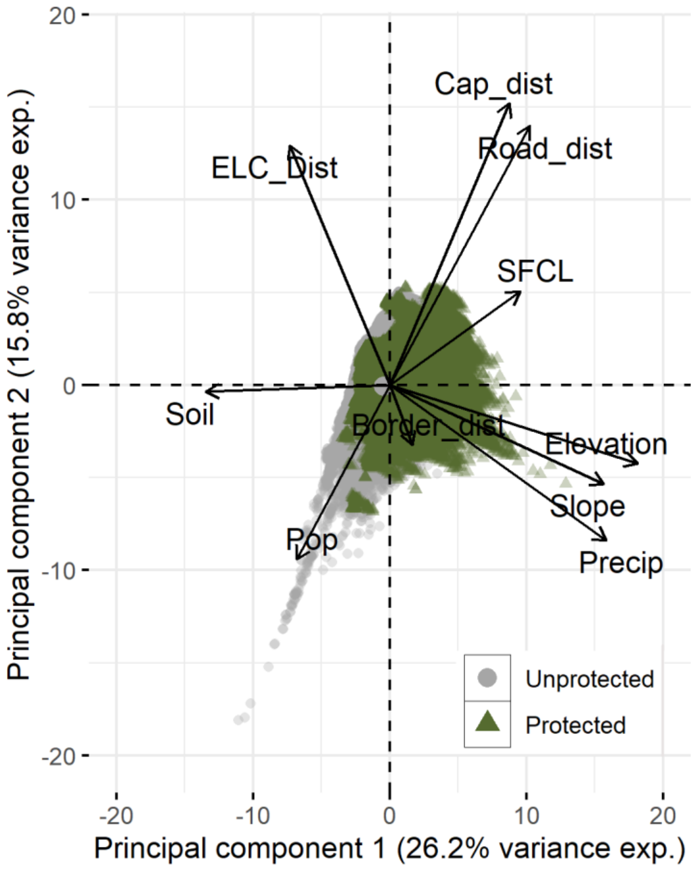


Figure 11: Biplot of the PCA analysis for units in the 2010-2012 outcome period: Note units are formatted with transparency to avoid over plotting.

The direction and lengths (measured from the centre point of the axes) of the arrows corresponding to each covariate is an indicator of how strongly each 'loads' upon the 2 PCs (X and Y axis respectively) and the nature of the relationship (Kohler and Luniak 2005). In addition to this the cosine of the angle between the arrows tells us the degree of correlation between the covariates (*p. 209*). In this regard Figure 11 shows that the covariates that exhibit the greatest variance within the data distance to provincial capital (Cap_dist) and distance to major road (Road_dist). However, these two covariates load more strongly on PC2 which explains less total variance than PC1. Hence the covariates that explain the most variance within the data are elevation, slope, soil type and average annual precipitation (Precip) which all load strongly on PC1. The small angle of the cosine between the former 2 covariates (Cap_dist and Road_dist) and elevation, slope and average annual precipitation suggest that these groups show moderate correlation amongst each other. This seems logical given that road density is higher around provincial capitals and elevation and slope are typically related.

The fact that the arrows for distance to ELCs (ELC_dist), and surrounding population (Pop) are all still relatively long as compared to the covariates discussed above suggests that these covariates also display considerable variance within the data however they do not load strongly on PC1 and PC2, suggesting it is likely that they are better represented by other PCs that would explain the remaining cumulative variance (~58%). Finally, the arrows for surrounding FCL (SFCL) and distance to land border (Border_dist) show that these covariates explain the least variance.

By contrast, the relative distributions of the observations as represented by the coloured points, provides an insight into how the different groups vary as well as the presence of outliers within them (Kohler and Luniak 2005). Unfortunately, in the case of Figure 11 this is not that useful as the distributions of the protected (treated) and unprotected (control) units are largely overlapping which confirms the earlier conclusion drawn from the statistical summaries and density plots that, in terms of the covariates utilised here, there are not strong differences between forest areas in PAs and non-protected forest areas in Cambodia. Perhaps the covariate for which there is the greatest difference is surrounding population density, which does show some deviation in distribution for the un-protected units. Although this could be an artefact of the outliers that exist for this covariate which are likely to represent small areas of forest remaining in high population areas such as public parks etc.

5.1.4 Predictors of deforestation in Cambodia

Similar to the preceding section it is important to identify the limitations of the inferences that can be made from the data used in this study. The results do not show all deforestation that occurred across the total outcome period of the study (2010-2018) rather only the deforestation of the proportion of the populations of treated and control units that were selected on the basis of being forested in the first year of the outcome periods. Similarly, the deforestation events are being compared only to the units that remained forested at the end of each outcome period. In effect this means that it is only possible to identify the predictors that contribute to whether a given unit loses or retains its forest cover within an outcome period. However, given that the number of deforestation events captured by the datasets is still substantial these inferences are likely to be relatively robust.

Table 4 below shows the differences in means, SD and standardized mean difference (SDM) in the values of the covariates between units that remained forested at the end of each outcome period and those that were deforested within them. The largest SDM values across all three outcome periods are for the surrounding FCL covariate, indicating that it is a strong predictor of deforestation likelihood. This shows that units that were deforested were on average much closer to recent FCL events than those that retained forest cover. In addition to this elevation, distance to major roads and average annual precipitation were also moderate predictors with SDM values >0.375 (with the exception of the 2013-2015 outcome period for distance to major roads). As expected, units that were deforested were in closer proximity to major roads and at lower elevations. Distance to provincial capital also displayed relatively high SDM values in the 2010-2012 and 2016-2018 outcome periods although markedly lower in the 2013-2015 period, which hints that perhaps the nature of the factors driving deforestation changed in this period. By contrast the weakest predictors of deforestation occurrence with respect to SDM are distance to land border (<0.1 for all outcomes periods) and surprisingly distance to ELCs (all values <0.13).

Table 4: Summary statistics for covariates of units separated by forest cover outcome

Covariate	Outcome period	Forested		Deforested		SDM
		Mean	SD	Mean	SD	
Surrounding FCL	2010-2012	2708.445	2611.225	854.859	854.621	-0.954
	2013-2015	2603.022	2604.029	692.957	803.261	-0.991
	2016-2018	2309.489	2473.284	993.688	1648.270	-0.626
Distance to ELC	2010-2012	43141.760	34138.373	46299.925	33860.852	0.093
	2013-2015	28194.142	25224.167	28714.903	26789.006	0.020
	2016-2018	22079.626	18841.640	24745.563	23277.566	0.126
Surrounding population	2010-2012	313.471	945.130	412.329	886.417	0.108
	2013-2015	282.857	824.419	308.285	731.569	0.033
	2016-2018	269.736	765.368	523.165	1043.685	0.277
Average annual precipitation	2010-2012	2167.795	734.888	1928.523	471.331	-0.388
	2013-2015	2204.940	746.154	1946.137	556.611	-0.393
	2016-2018	2234.379	758.114	1985.170	554.927	-0.375
Slope	2010-2012	11.048	11.840	7.850	7.193	-0.326

	2013-2015	11.515	12.185	8.623	8.442	-0.276
	2016-2018	11.960	12.417	9.094	9.313	-0.261
Elevation	2010-2012	214.479	222.921	131.061	111.931	-0.473
	2013-2015	224.806	230.623	137.998	115.606	-0.476
	2016-2018	234.716	237.107	156.674	145.510	-0.397
Distance to provincial capital	2010-2012	46774.904	18557.934	40970.980	17897.997	-0.318
	2013-2015	46894.691	18821.762	44536.245	17295.180	-0.130
	2016-2018	47770.696	18885.390	39330.688	17827.204	-0.460
Distance to land border	2010-2012	60357.117	40474.591	58753.869	39642.574	-0.040
	2013-2015	59393.850	40204.980	63377.342	40811.249	0.098
	2016-2018	59727.194	40265.766	57029.983	40740.639	-0.067
Distance to major roads	2010-2012	14332.204	11109.156	9827.803	8520.585	-0.455
	2013-2015	14695.461	11265.702	11533.973	9594.964	-0.302
	2016-2018	15094.118	11385.211	10483.544	8984.866	-0.450
Soil type	2010-2012	7.021	6.303	9.772	6.088	0.444
	2013-2015	6.754	6.251	8.804	6.274	0.327
	2016-2018	6.559	6.238	8.524	6.164	0.317

As per the analysis of biases in PA location it is also useful to examine the smoothed density distributions of the covariates to see how they vary beyond just measures of central tendency. These are visualized in Figure 12 below for the 2010-2012 outcome period. Again, these density plots largely confirm the inferences made from the statistical summaries above i.e. that units that were deforested were in closer proximity to surrounding FCL events.

However, there are several noteworthy features, particularly that a large proportion of the units that remained forested were in areas of higher human population than those that were deforested, which is obscured in the mean values in Table 4. Also, for distance to ELCs, the density distribution shows that a greater proportion of units in close proximity (0-10000m) were deforested which was not evident from the comparisons of means. Although surprisingly at intermediate distances (10,000-50000m) a larger proportion of units remained forested.

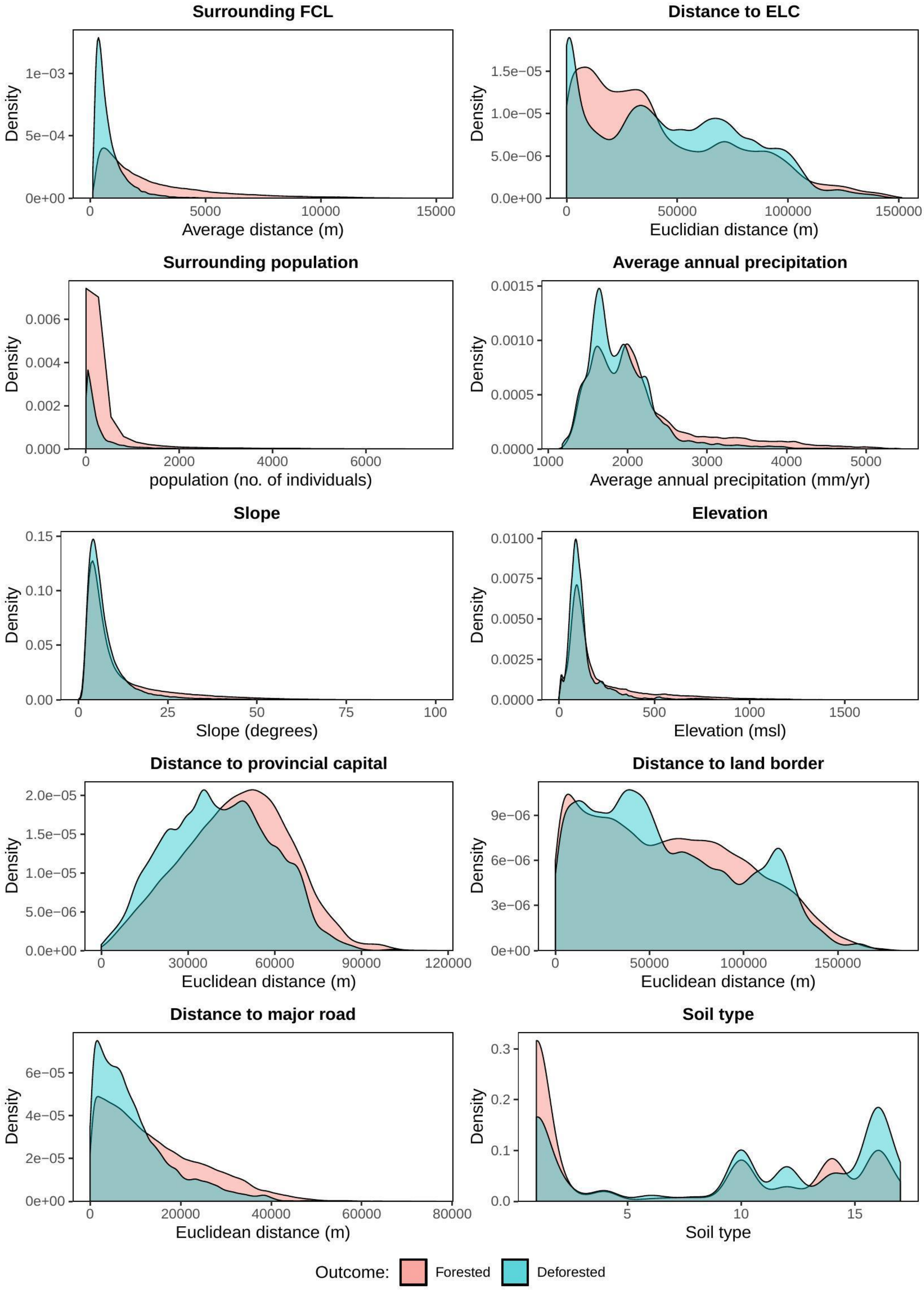


Figure 12: Smoothed density distributions of each of the covariates for units exhibiting different forest cover outcomes in the 2010-2012 period

The statistical summaries and density distributions have already provided insights into the differences in covariates between units that remained forested and those that were deforested. Rather than using PCA to further validate this it was instead deemed pertinent to use it to investigate the differences in units that were deforested inside PAs vs. those that were deforested in the unprotected landscape. This of course allows for inferences to be made as to whether there are differences in the predictors of deforestation as a result of protection. In addition, by performing PCA for the deforested units in each of the three outcome periods separately as well as again for all units in all outcome periods, it is possible to ascertain whether these predictors have changed over time. Figure 13 below contains the biplots for all of these PCAs.

The axes of the biplots for all outcome periods show that the first 2 PCs in each explain relatively low proportions of the total variance with the highest being 38.2% for the 2016-2018 period and the lowest being 35.9% for 2013-2015. This indicates that the variance in the data is not easily explained by a small number of factors, rather it is the result of numerous factors, the implication of this being that any inferences drawn the distributions of the data points and the variables are fairly limited.

Irrespective of this, there are some differences displayed in the biplots from different time periods. The variable arrows in the 2010-2012 biplot show that for this period elevation, slope and precipitation explained the most variance in the data. This was followed closely by distance to provincial capitals, land borders and major roads and whilst the distribution of the two groups of observations (deforestation events in PAs and unprotected areas) in the region of these covariates do overlap there is a clear area in the upper right hand quadrant of the plot where the distribution of unprotected units is not overlapped. This indicates that deforestation events occurred at more extreme values of these covariates than for the protected pixels. However, this is not the most pronounced region where the distributions of the two groups diverge, which is instead in the lower right quadrant of the plot and shows that deforestation events in the unprotected landscape occurred with greater variability with respect to the size of the surrounding human population.

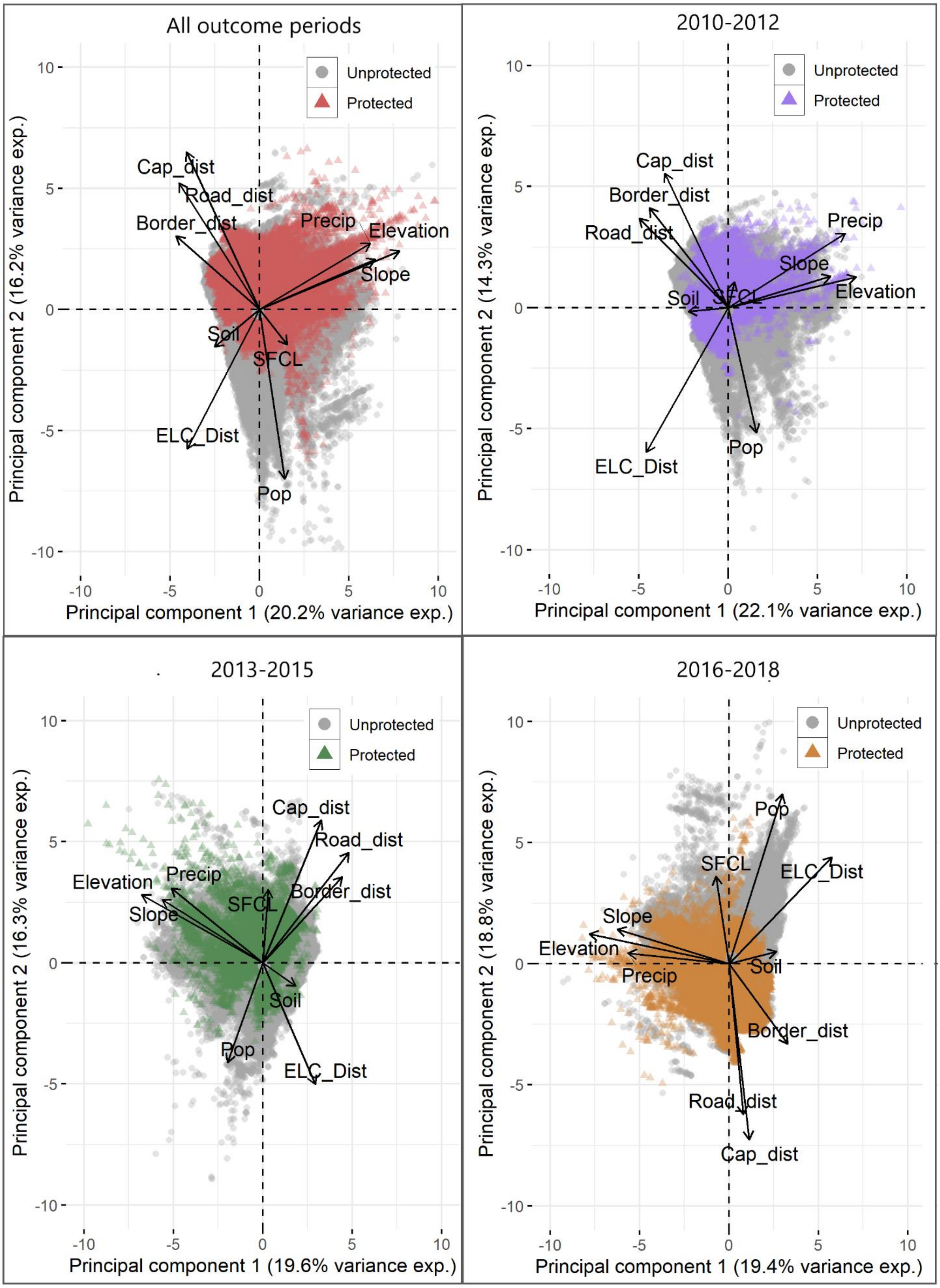


Figure 13: PCA biplots for deforested units in each outcome period and across all periods: Note units are formatted with transparency to avoid over plotting.

As for the 2013-2015 biplot the covariates of elevation, slope and precipitation still explain the most variance with respect to the first 2 PCs. Although the nature of the relationship between them and PC1 changed from the previous outcome period with these variables now exhibiting negative variance values. This suggest that the variance in these covariates for this outcome period is due to smaller values rather than larger values. The fact that the distribution of the unprotected observations 'breaks away' from that of the protected observations towards the upper left corner of the plot, indicates that more deforestation took place at lower values of elevation, slope and average annual precipitation in unprotected areas in this outcome period. In addition to this it appears that the distributional difference between the groups with respect to surrounding population size is less pronounced in this outcome period than in 2010-2012.

In the 2016-2018 biplot the 2 PCs are the closest in terms of the % of variance they explain (PC1 = 19.4%, PC2=18.8%). This is noteworthy as it clear that there is less difference between the covariates in terms of which explains the most variance with respect to the PCs. Similar to the two previous outcome periods the most prominent of these are distance to provincial capital and roads, elevation, slope and average annual precipitation. For this outcome period the difference in distributions of the two groups with respect to the surrounding population covariate (upper right quadrant) shows a similar pattern to that of the 2010-2012 outcome period.

To summarize, whilst the PCAs have highlighted some minor differences between the three outcome periods, the patterns of predictors of deforestation occurrence with respect to protected and unprotected areas are largely consistent. This is exemplified by the biplot in Figure 13 for the data of all outcome periods combined which shows that the greatest difference between the two groups is that a greater proportion of deforestation events in the unprotected landscape occurred at higher values of surrounding population size.

In conclusion, the main points to take away from this portion of the analysis are that, first the strongest predictors of whether a 30x30m forested area in Cambodia will be deforested or not within the subsequent 1-3 years are its proximity to surrounding FCL events, elevation and distance to major roads, with lower values of all three variables increasing the likelihood of deforestation. Second, the factor that varies most with regards to deforestation occurrence inside PAs vs. in unprotected areas is the size of the surrounding human population. This is unsurprising given that section 5.13 shows that this covariate showed the most variation between protected and unprotected areas.

5.1.5 Results of spillover analysis

Table 5 presents the results of the ATT estimation for the treatment of a spillover effect of PA establishment into 5km buffer zones exogenous to PA boundaries. Values of ATT are included for each of the 4 matched samples along with an average for each outcome period. Given that the outcome variable is dichotomous the values of ATT produced are effectively odds ratio representing the difference in probability of a unit under treatment displaying a given outcome (Benedetto *et al.* 2018). Note that the averages of the bias-adjusted (Abadie-Imbens) SEs are simply the mean variance across the samples and not calculated as a function of the average ATT.

Table 5: Estimates of ATT and bias-adjusted SE produced by the spillover analysis

	2010-2012		2013-2015		2016-2018	
Sample	ATT^{signif}	Bias-adjusted SE	ATT^{signif}	Bias-adjusted SE	ATT^{signif}	Bias-adjusted SE
1	-0.03988***	0.00274	-0.01092***	0.00270	-0.0332***	0.00328
2	-0.03864***	0.00272	-0.01448***	0.00271	-0.02956***	0.00326
3	-0.04536***	0.00273	-0.00928***	0.00268	-0.03004***	0.00327
4	-0.03456***	0.00269	-0.00928***	0.00268	-0.02628***	0.00322
Average	-0.03961	0.00272	-0.01099	0.00269	-0.02977	0.00325

Note: $^{**}p<0.05^{***}p<0.01$

Table 5 shows that for all samples in all outcome periods there was a significant effect of ATT at the 99% confidence level. The fact that the values of ATT are negative is an artefact of how the outcome variable was coded in the data however they do represent a positive treatment effect. In practical terms this means that units within PA buffer zones were significantly less likely to be deforested over the course of the outcome periods than those in the unprotected (control) landscape. However whilst the effect may be significant the size of the effect is not particularly large, if the decimal ATT odds-ratios are expressed as percentages this means that in the 2010-2012 period a 30x30m area of forest (the unit of analysis) inside a radius of 5km from a PA boundary was on average ~4% less likely to be deforested than a similar forested area >5km away from the PA boundary. The 2016-2018 period displayed a similar result with an average ~3% reduction in the probability of deforestation for treated units. However, the ATT for the 2013-2015 period was noticeably lower with a sample average of ~1%. At this stage it is not pertinent to offer explanation as to why this reduction in ATT may have occurred without first observing the results of the primary analysis and as such this will be addressed as part of an overall summary of results (section 5.4).

Of course, the treatment effects observed need to be viewed in consideration of the quality of matching that was achieved. In this regard Figure 14 below contains a love plot of the absolute standardized mean difference (ASMD) and the KS statistics for all the covariates, averaged across the four samples of the 2010-2012 outcome period.

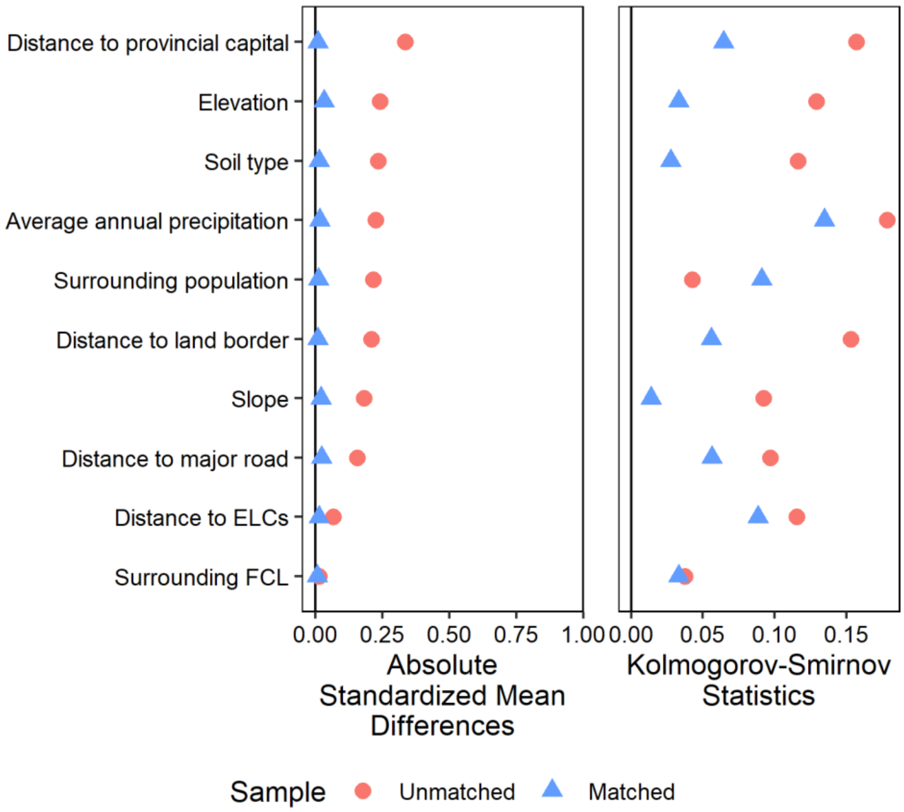


Figure 14: Average values of covariate summary statistics pre- and post-matching in the 2010-2012 outcome period of the spillover analysis

Figure 14 shows that PSM was able to reduce the values of ASMD and the KS statistic for all covariates, with many of the ASMD values being close to 0 in the matched data. This is a better result than was observed for the post-matching covariate balance when trialing the PSM technique for the treatment of PA establishment (Appendix K: Figure K1). Although this is not unusual given that the spatial extent of the treated units for the spillover analysis (within 5km PA buffer zones) is substantially smaller than that of the treated units used in testing (within PA boundaries). This considerable improvement in covariate balance is an indicator that the ATT estimates produced from the post matching samples are robust.

5.2 Primary analysis

5.2.1 Assessing quality of matching

Before presenting the main results of the analysis in terms of treatment effect estimates it is important to inspect the quality of matching that was achieved in terms of improvements to the covariate balance. Figure 15 below contains a love plot of the ASMD and KS statistic values for each covariate averaged over the 12 sub-samples for the 2010-2012 outcome period that were subjected to PSM.

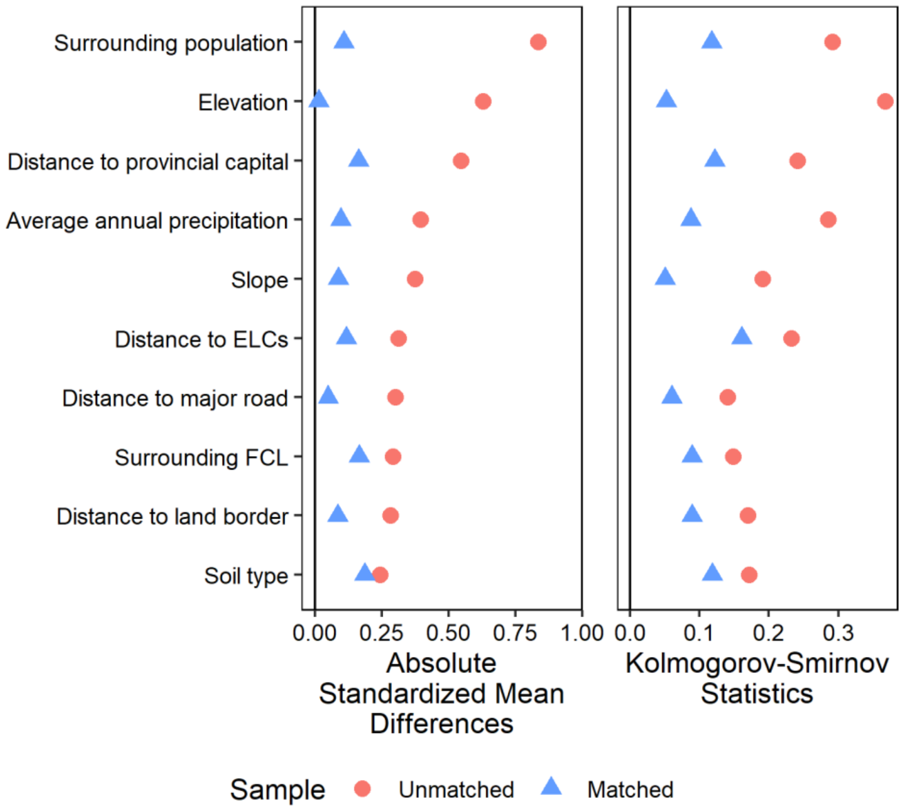


Figure 15: Average values of covariate summary statistics pre- and post-matching in the 2010-2012 outcome period of the primary analysis

Figure 15 shows that for all covariates PSM improved covariate balance between treated and control units as exemplified by reductions in average ASMD and KS statistic values. For some covariates the improvement in balance in more pronounced than others however these tend to be the covariates that showed greater imbalance to begin with, such as surrounding population. Overall, this result is largely equivalent to that which was achieved when the PSM method was trialed as part of the preliminary analysis (Figure K1: Appendix K) and whilst it also did not achieve perfect covariate balance (ASMD and KS statistic values = 0), it does affirms that the approach of sub-sampling was appropriate.

A very similar result in terms of covariate balance improvements was achieved for the 2013-2015 outcome period, and thus for the sake of brevity the love plot visualizing this is not presented here although it has been included as an Appendix (M). On the other hand, the covariate balance for the 2016-2018 period, did show some discrepancies and hence is presented as Figure 16 below.

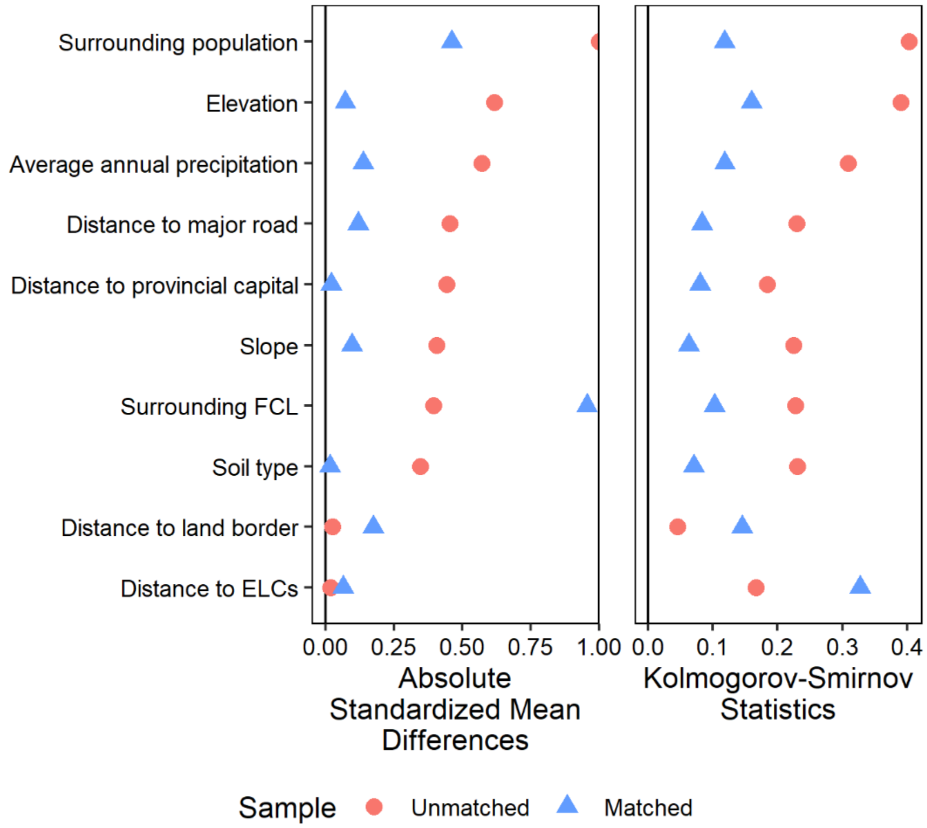


Figure 16: Average values of covariate summary statistics pre- and post-matching in the 2016-2018 outcome period of the primary analysis

Figure 16 shows that in the 2016-2018 period matching did not result in an average reduction in ASMD for three covariates: surrounding FCL; distance to land border and distance to ELCs. Although for surrounding FCL it did still reduce the value of KS statistic. This is problematic as it reduces the robustness of the ATT estimate produced for this period and unfortunately there is little that can be done to rectify this short of removing these covariates and repeating the analysis. Logically, this would have to be performed for all outcome periods which itself would be contentious given that balance was improved upon these covariates within the other periods.

5.2.2 Results of post-matching SAC testing

To re-iterate the purpose of this testing was to highlight whether the sub-sampling procedure reduced the presence of SAC in the data (as detailed in Appendix J). The residuals of GLMs produced from the matched data of one randomly selected sample from each outcome period were tested using the Moran's I test. The results of these tests found significant SAC at the 99% confidence level ($p<0.01$) in each outcome period. The implication of this is that the results of the treatment effect estimates are likely being influenced by SAC to some extent. Unfortunately given earlier that attempts to mitigate the effect of SAC through the use of spatial GLMs were not successful (Appendix J), there is no realistic way to estimate the magnitude of this impact or to mitigate it. It is clear that SAC remains a pervasive issue for studies of this nature which will be reviewed further in the discussion.

5.2.3 Treatment effect results

Table 6 below presents the results of the ATT estimation for the treatment of units being included inside established PAs at the outset of each outcome period. In this case values of ATT are included for each of the 12 matched samples, along with an average, for each outcome period. As in the spillover analysis (section 5.1.5) the ATT values are expressed as odds ratios and the averages of the bias-adjusted SEs are calculated as the mean variance across the samples (not from the average ATT value). All samples in all outcome periods showed a significant positive treatment effect at the 99% confidence level ($p<0.01$), with the ATT values and bias-adjusted SE values being relatively consistent across the samples within each period. There was however a pronounced difference in the average ATTs values between the outcome periods corresponding to an apparent trend of decreasing PA effectiveness over time. In practical terms, the results show that in the 2010-2012 outcome period a 30x30m area of forest inside a PA was on average ~8.6% less likely to be deforested than a similar forested area not under protection. Whereas in the 2013-2015 outcome period this reduced likelihood of deforestation fell to ~6.1% and in the 2016-2018 period it decreased further to just ~2.9%.

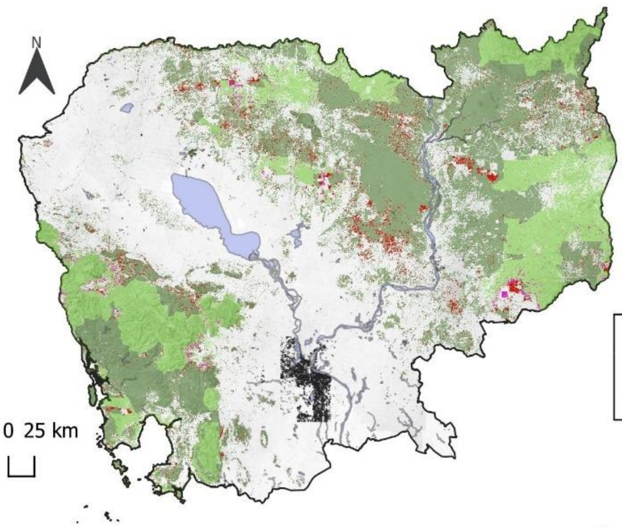
Table 6: Estimates of ATT and bias-adjusted SE produced in the primary analysis

	2010-2012		2013-2015		2016-2018	
Sample	ATT^{signif}	Bias-adjusted SE	ATT^{signif}	Bias-adjusted SE	ATT^{signif}	Bias-adjusted SE
1	-0.0831***	0.0022	-0.0606***	0.0022	-0.0293***	0.0024
2	-0.0812***	0.0022	-0.0604***	0.0022	-0.0247***	0.0024
3	-0.0950***	0.0023	-0.0602***	0.0021	-0.0306***	0.0024
4	-0.0822***	0.0022	-0.0624***	0.0021	-0.0271***	0.0024
5	-0.0856***	0.0023	-0.0595***	0.0021	-0.0294***	0.0024
6	-0.0904***	0.0023	-0.0619***	0.0022	-0.0303***	0.0024
7	-0.0868***	0.0023	-0.0597***	0.0021	-0.0297***	0.0024
8	-0.0862***	0.0022	-0.0583***	0.0021	-0.0296***	0.0024
9	-0.0880***	0.0023	-0.0648***	0.0022	-0.0279***	0.0024
10	-0.0863***	0.0023	-0.0599***	0.0021	-0.0315***	0.0024
11	-0.0837***	0.0022	-0.0622***	0.0022	-0.0326***	0.0024
12	-0.0850***	0.0023	-0.0616***	0.0022	-0.0234***	0.0024
Average	-0.0861	0.0023	-0.0610	0.0021	-0.028853	0.00242

Note: $^{**}p<0.05^{***}p<0.01$

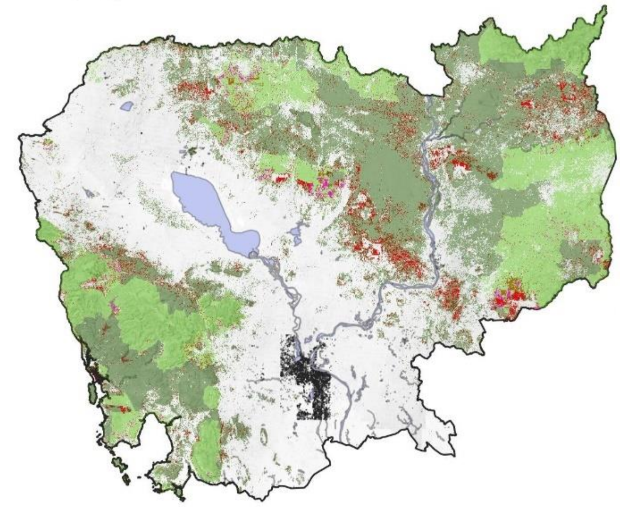
However, the validity of this inference of decreased PA effectiveness between outcome periods must be viewed in light of the relative deforestation pressure faced by PAs and unprotected forests over time. I.e. If the extent of FCL exhibited by PAs increases relative to the previous outcome period whilst simultaneously the extent in unprotected regions becomes substantially less then there are likely exogenous factors driving this that were not present in the previous period. For example, deforestation in the unprotected forest has become less commonplace as more land has been formally titled whilst it has increased in PAs due to a reduction in law enforcement activities. In this case it would be valid to infer that PA effectiveness has decreased but comparing the difference in ATT for this period to another where the control region exhibited greater FCL allows for only weak causal inference as the influence of the change on the control units is not accounted for. Alternatively, if deforestation pressure across different outcome periods is either consistent or changes uni-directionally with respect to PAs and unprotected forests then this is not problematic.

To facilitate comparison between the outcome periods, Figure 17 below includes visualizations of the spatial distributions of all FCL events occurring in either PAs or control (unprotected) regions in each outcome period, as well as summary statistics of the proportions of forest cover lost.



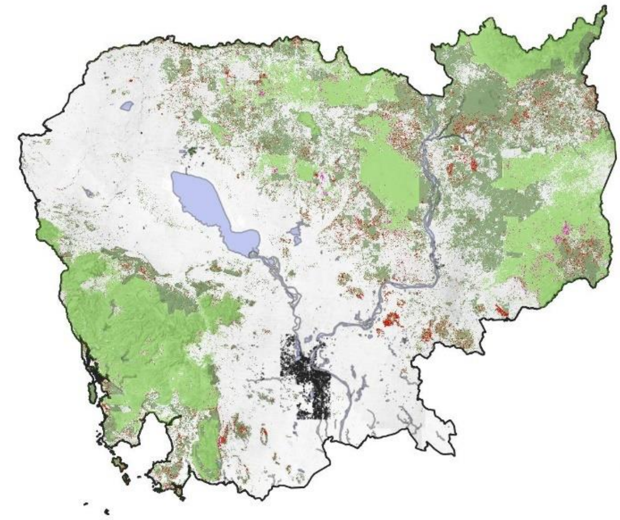
2010-2012

	PAs (treatment)	Non-PA (control)
FC in 2010 (km^2)	34406	55820
FCL (km^2)	959.16	3515.71
% of FCL	2.79	6.30



2013-2015

	2013-2015	
	PAs (treatment)	Non-PA (control)
FC in 2013 (km^2)	29845	52136
FCL (km^2)	958.53	2936.24
% of FCL	3.21	5.63
Relative change in FCL compared to 2010-2012 (%)	-0.07	-16.48



2016-2018

	PAs (treatment)	Non-PA (control)
FC in 2016 (km^2)	40304	36478
FCL (km^2)	1152.68	2770.66
% of FCL	2.86	7.60
Relative change in FCL compared to 2013-2015 (%)	20.26	-5.64

Figure 17: Forest dynamics occurring in protected and unprotected areas in each outcome period of analysis (data sources: GADM 2018; ODC 2019b; SERVIR-Mekong 2020b)

First, on a positive note, the summary data for all outcome periods in Figure 17 does corroborate the treatment effects observed in the primary analysis as it shows that PAs saw a smaller proportion of FCL relative to the area of forest cover they exhibited at the start of the each outcome period as compared to unprotected areas (PAs: 2.79% vs. 6.30% in unprotected areas).

By contrast deforestation pressure can be compared between each outcome period by considering both the total area of FCL incurred by each region and the relative change in the area of forest cover lost as compared to the previous outcome period. In this regard Figure 17 shows that PAs exhibited almost the same area of FCL in both the 2010-2012 and 2013-2015 outcome periods (~960 km^2) with a relative decrease of only 0.07%. This implies that the deforestation pressure they faced was consistent. However considering that the total area of forest cover in PAs in the second outcome period was substantially less than in the first (29845 km^2 vs. 34406km^2) this does confirm the notion of a reduction in PA effectiveness (despite there being less forest to protect, PAs failed to reduce the total amount of FCL incurred). In this case the fact that the deforestation pressure on the unprotected region appears to drop between the first and second outcome periods does not impact this inference given that the pressure on PAs remained consistent.

On the other hand, the inferred reduction in PA effectiveness between the latter two periods of 2013-2015 and 2016-2018, which was greater than that observed between the former two (decrease in absolute ATT of 3.2% vs. 2.5%: Table 6) is not so simple to validate. In this case PAs in the 2016-2018 period exhibited a substantially greater area of FCL area (1152km^2) equating to a relative increase of ~20.3% as compared to the 2013-2015 period. However, this must be viewed relative to the fact that the area of forest cover inside PAs in 2016 increased to 40304km^2 which is greater than it was at the outset of the preceding period (29845km^2) let alone before the deforestation that occurred up until the end of 2015. This obviously results from the increase in the size of the total PA estate due to the new areas declared in 2016 (section 2.2.1), with the implication being that it is not possible to state whether deforestation pressure has increased relative to the previous period from this aggregated figure for all PAs. Instead confirmation of whether the decrease in ATT value observed for this period can actually be ascribed to decreasing PA effectiveness should come from a comparison of treatment effect estimates for the PAs in this period excluding those established in 2016 as compared to the relative FCL they experienced. These are the estimates that will be presented as part of the secondary analysis and thus the answer to this will be addressed in the summary of results (section 5.4).

5.3 Secondary analysis

Table 7 below shows the number of treated units contained in the original samples for category of PA establishment date under all outcome periods. These were used to determine the number of sub-samples to be analyzed using PSM matching. Table 7 highlights that the category of PAs established between 2001-2010 had considerably lower number of treated units in the sample implying that these PAs amounted to a lower total area than the other categories something which is shown clearly in Figure 18 which follows later in this section.

Table 7: Treated sample sizes and number of sub-samples used in the secondary analysis

	Outcome periods					
PA establishment categories	2010-2012		2013-2015		2016-2018	
	No. of treated units	No. of sub-samples matched	No. of treated units	No. of sub-samples matched	No. of treated units	No. of sub-samples matched
1993-2000	319090	13	275396	11	264415	11
2001-2010	34732	2	32090	2	28593	2
2011-2016	-	-	-	-	120215	5

The average treatment effect estimates (mean ATT) resulting from the secondary analysis are presented in Table 8 below. Given that these estimates of means are taken from the multiple samples analysed for each category they do not have significance values associated with them. However, all samples did produce positive significant ATT estimates at the 99% confidence level ($p<0.01$) and the results of treatment effect estimation for all individual matched samples has been included as a tabular Appendix (N).

Table 8: Mean ATT estimates for PAs in categories of establishment date

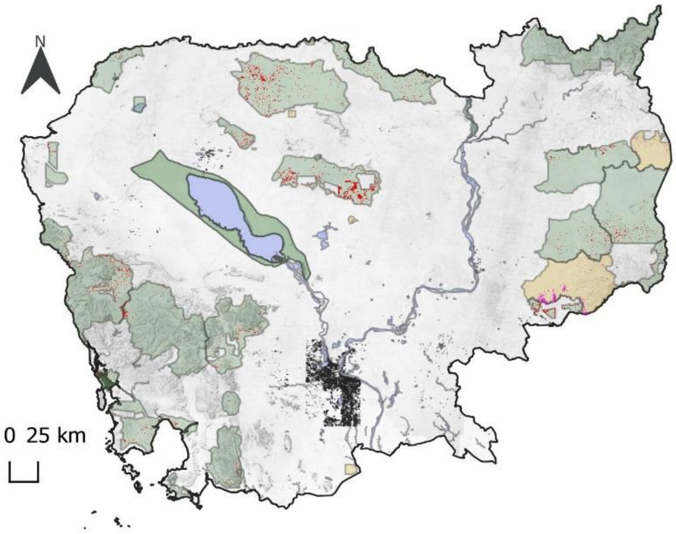
	Outcome period					
	2010-2012		2013-2015		2016-2018	
PA establishment date categories	Mean ATT	Mean bias-adjusted SE	Mean ATT	Mean bias-adjusted SE	Mean ATT	Mean bias-adjusted SE
1993-2000	-0.072	0.00215	-0.061	0.00210	-0.026	0.00246
2001-2010	-0.21544	0.00291	-0.118	0.00278	-0.015	0.00305
2011-2016	-	-	-	-	-0.063	0.00240

In a similar fashion to the primary analysis the results in Table 8 should be viewed in combination with the summary statistics related to the deforestation pressure faced by each category of PA across the different outcome periods which are presented in Figure 18.

Starting with the 2010-2012 outcome period, Table 8 shows that PAs established between 2001-2010 produced a substantially greater ATT value than those established between 1993-2000 (~-0.22 vs. ~0.07). This implies that the former groups of PAs, which are effectively younger, were more effective in terms of generating avoided deforestation than the latter group. Although whether or not this would still be true if they covered the same total area as the older group cannot be verified i.e. it cannot be said with certainty that the overall effectiveness of these PAs intrinsically stems from how they were managed or simply due to differential pressure upon the forest resources they protected.

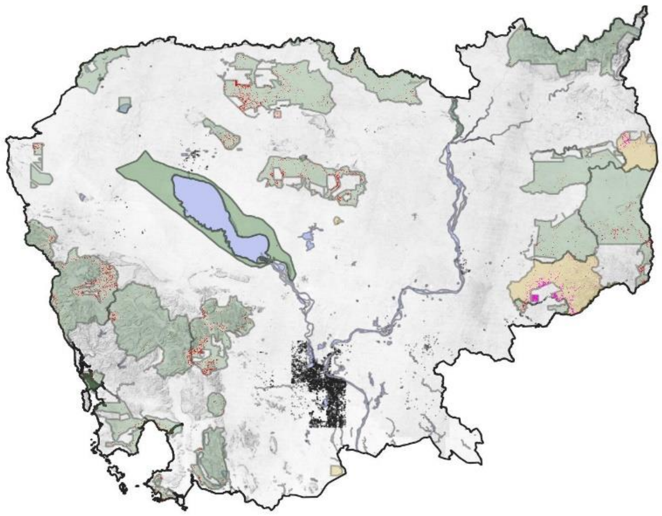
As for the 2013-2015 period, the two categories of PAs exhibited the same general feature with the 2001-2010 PAs showing a greater mean ATT value compared to the 1993-2000 PAs (~0.12 vs. ~0.06), but the relative difference between these values is quite a bit smaller compared to the previous outcome period. This is validated by the values of relative % change in FCL area (compared to 2010-2012 period) in Figure 18, which show that the amount of FCL occurring in the 1993-2000 PAs fell by 9.2% whereas the 2001-2010 PAs saw a dramatic increase in FCL loss area of 85.5%. Overall, this is a strong basis to conclude that whilst both groups of PAs were less effective in 2013-2015 than in the previous period, the decrease in effectiveness was proportionally greater for younger PAs.

This trends also holds true for these groups of PAs in the 2016-2018 outcome period, with the 1993-2000 PAs exhibiting a relative % change in FCL area (as compared to 2013-2015 period) of only ~-2.8% whereas for the 2001-2010 PAs it was ~-16.6%. In fact, this led to the former group displaying a higher mean ATT value as compared to the latter (Table 8: -0.026 vs. -0.015), which is a notable change from the previous two outcome periods.



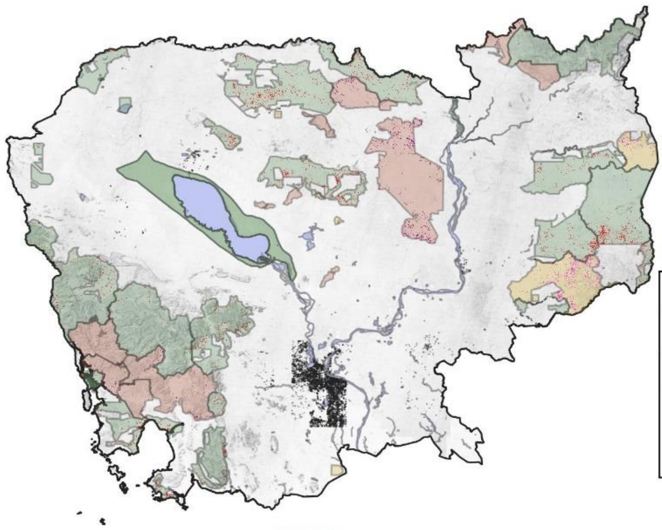
2010-2012

	PA establishment period	
	1993-2000	2001-2010
FC in 2010 (km^2)	31003	3395
FCL (km^2)	866	93
% of FC lost	2.79	2.73



2013-2015

	PA establishment period	
	1993-2000	2001-2010
FC in 2013 (km^2)	26669	3125
FCL (km^2)	786	172
% of FC lost	2.95	5.51
Relative change in FCL area compared to 2010-2012 (%)	-9.23	85.54



2016-2018

	PA establishment period		
	1993-2000	2001-2010	2011-2016
FC in 2016 (km^2)	25834	2752	11719
FCL (km^2)	764	144	245
% of FCL	2.96	5.22	2.09
Relative change in FCL area compared to 2013-2015 (%)	-2.79	-16.58	-

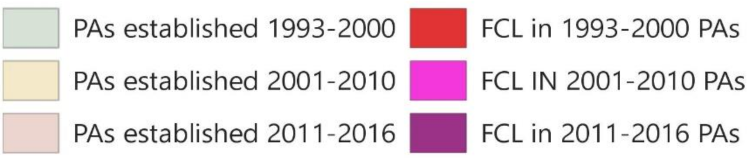


Figure 18: Forest dynamics occurring between 2010-2018 inside PAs established in different time periods (data sources: GADM 2018; ODC 2019b; SERVIR-Mekong 2020b)

Finally, it is interesting to note that the new category included in the 2016-2018 outcome period of PAs established between 2011-2016 had a greater mean ATT estimate compared to the two other groups (Table 8: -0.63 vs. -0.026 (1993-2000) and -0.015 (2001-2010)). This supports the inference made with respect to the 2010-2012 outcome period that newly established PAs are more effective in their early years than older ones. Although again this is difficult to prove given the difference in areal coverage of the respective categories.

As per the primary analysis it is important to inspect the improvement in covariate balance resulting from the matching process as this can highlight whether any category of PA suffered from poorer matching quality than others which in turn implies that the forest resources contained in those PAs were more different with respect to the control region than that contained in others.

Figure 19 contains a joint love plot showing the changes in ASMD and the KS statistic values for covariates in each category of PA establishment date in the 2010-2012 period. Comparing between the two categories there does appear to be some general trends, first PAs established between 2001-2010 showed higher values of ASMD and the KS statistic prior to matching than the 1993-2000 category and despite a greater magnitude of reduction in these values through matching the post matching values also remained higher. This would imply that the treated units (forest) contained in these PAs was more different (and thus harder to match) than that of the 1993-2000 PAs. However, it must be said that this is not the case for all covariates with notable exceptions being elevation and distance to provincial capital for which the relationship was essentially the opposite to that described above. Also, this trend is hardly surprising given that the 2001-2010 PAs covered a much smaller area than those established prior to them (see Figure 18), logically making them more likely to exhibit a smaller variance with regards to the covariate values associated with their treated units.

The same general relationship was observed for the two categories in the 2013-2015 outcome period for which a love plot has been included in Appendix O (Figure O1). As for the novel category of PAs established between 2011-2016 in the 2016-2018 outcome period, Figure O2 (Appendix O) shows that matching was able to achieve balance improvements on the majority of covariates. Although for surrounding population matching did not reduce the value of ASMD and for surrounding FCL it actually increased it. This was also true for this latter covariate in the other two categories of PA establishment in this outcome period. At the same time however the KS statistic value for both of these covariates was reduced by matching indicating that overall the process did make the distributions of the treated and control units closer even if this was not reflected in the measure of central tendency (ASMD).

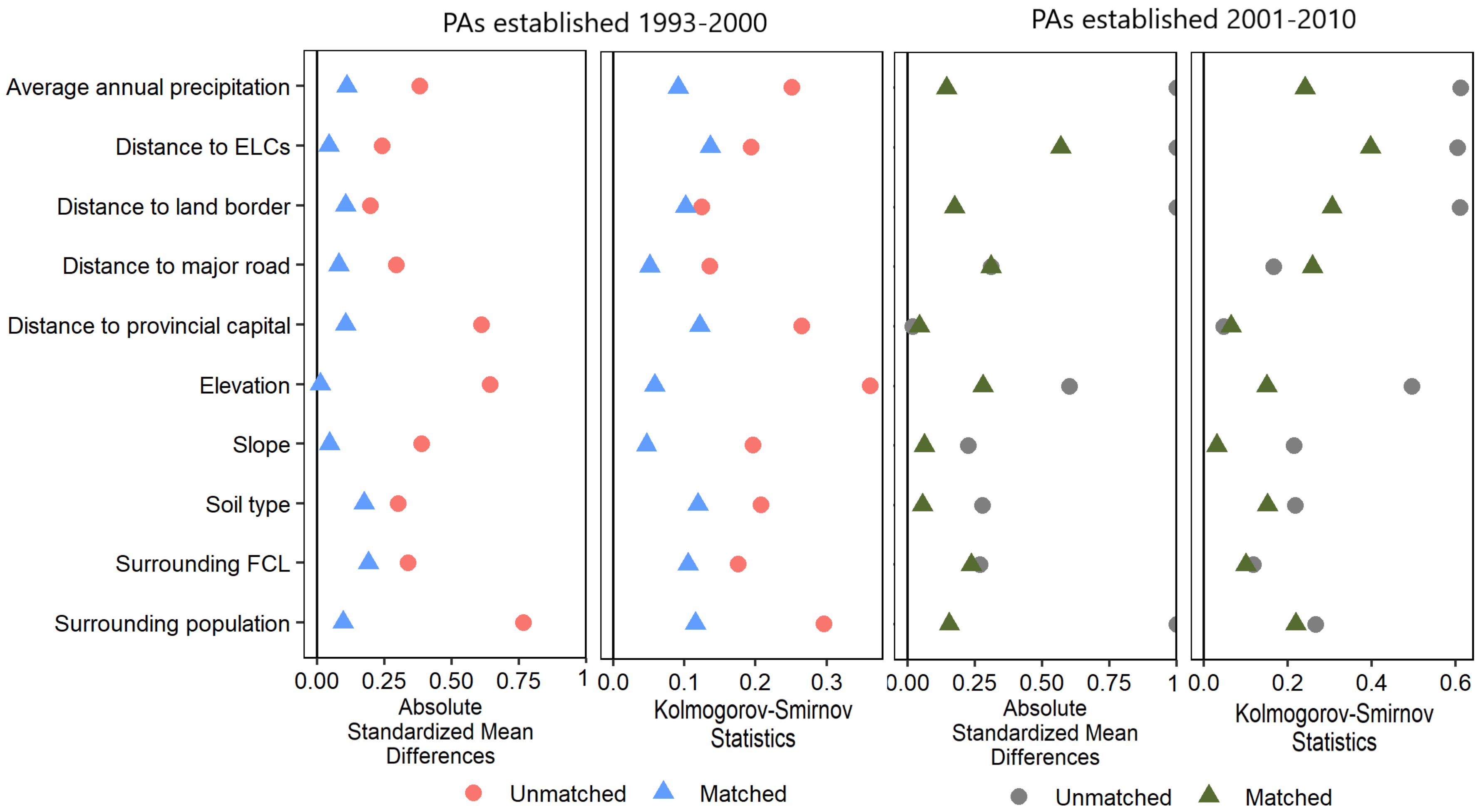


Figure 19: Average values for covariate summary statistics for PAs grouped by establishment date in the 2010-2012 outcome period

5.4 Summarizing results

The purpose of this section is to tie together the results of each section of the analyses (spillover, primary and secondary) and highlight complementarities or discrepancies that exist between them.

First, the spillover analysis found a significant positive spillover effect of reduced probability of deforestation in 5km buffer zones exogenous to PA boundaries in all outcome periods, albeit with a decreased magnitude of treatment effect in 2013-2015 (Table 5). Overall, this result is validated by those of the primary and secondary analyses which also observed significant positive effects in all outcome periods. More specifically, the reduced magnitude of the spillover effect in the 2013-2015 period concurs with the reduction in mean ATT for the same period in the primary analysis (Table 6). However, the fact that spillover effect in 2016-2018 increased with respect to the former period does not match the trend of continued decreased PA effectiveness in that period for the primary analysis. This is a topic for further explanation which is addressed in the discussion (section 6.1).

Second, the results of the treatment effect estimate of the primary analysis implied a trend of decreasing PA effectiveness over time. This assertion was validated for the transition between the 2010-2012 period and 2013-2015 on the basis of the population level trends in FCL observed, although the same leap was not possible for the 2016-2018 period given that so much new PA land was added to the national estate. In this regard the results of the secondary analysis create the opportunity for a 'pseudo-counterfactual' comparison of what trend would have occurred had the PAs in 2016 not been established by only considering the treatment effects of the PAs established in the 1993-2000 and 2001-2010 categories. Table 8 shows that the mean ATT estimates for both these categories of PAs in 2016-2018 also decreased relative to the 2013-2015 period which in effect confirms what the primary analysis alone could not, namely that PA effectiveness did successively decline in the periods between 2010 and 2018. The first question that this result should prompt is why did this occur? Which of course will be a topic of the discussion which comprises section 6.

6. Discussion

Given the breadth of analysis undertaken the discussion has been sub-divided into several sections. First section 6.1 will interpret the results in greater detail with regards to the circumstances that characterized each outcome period as well as other studies of PA avoided deforestation both in Cambodia and in a wider context. Second, section 6.2 will highlight the limitations of the analysis in both a practical and conceptual capacity. Finally, section 6.3 will summarize the implications of the results for stakeholders at a national scale as well as the wider context of quasi-experimental PA assessment literature and make recommendations for further study.

6.1 Interpretation of results

6.1.1 Findings of the preliminary analysis

Beginning with the findings of the preliminary analysis that addressed objective iii (section 3.2.1). The first component of which was the identification of biases in PA location in Cambodia as expressed by differences in the biophysical and socio-economic covariates between the units inside PAs and in the unprotected region. The three different techniques (summary statistics. smoothed density distributions and PCA) used gave rise to the same conclusion that overall, the groups show similar distributions across the majority of covariates with minor differences in only a few (section 5.1.3). The implication being that PAs in Cambodia do not show strong biases in their siting.

The moderate differences that were observed were that PA units were found at greater average distance from provincial capitals, major roads and surrounding FCL events. With the most pronounced differences in distribution being that PA units were mostly in areas of lower human population at higher average elevations (Figure 10;11, Table I4).

Given that no previous studies have analyzed PA location in Cambodia in this manner there is no direct source of comparison for these results. However, they do match the results of similar studies in the wider literature which are collectively responsible for the broadly supported prognosis that PAs are located in remote and less accessible areas, (Mas 2005: Andam *et al.* 2008; Joppa *et al*. (2008); Joppa and Pfaff 2009: Pfaff *et al.* 2009: Kere *et al.* 2017: Sarathchandra *et al.* 2018).

In this sense the nature of the differences observed are of less interest than the question of why the magnitude of the differences (biases) was not so pronounced. This likely stems from the fact that, Cambodia exhibits a relatively high coverage of PAs as a percentage of its total land area (26% in 2014 (RGC 2014) increasing to 34% in 2016 (Souter *et al.* 2016) not inclusive of development projects inside PA boundaries). Given that this high coverage is spread throughout the country (Figure 6, section 2.2.1) (i.e. encompassing the range of geophysical conditions that exist) the lack of a pronounced difference as compared to unprotected areas is not so unusual. If anything, this is further exaggerated in this study considering that only forested units of analysis were compared and it is clear from the size of the populations of both treated units and control units that PAs contain a large proportion of the countries remaining forests (Table 3: section 5.1.1).

However, there is another factor that suggests that the explanation is not quite as simple as this. Namely that, given the size of the PA estate increased in the final outcome period then we would have expected this to have reduced the variance of covariates between treated and control groups as compared to previous periods. Whilst the values of the KS statistic and COC in Table I4 (Appendix I.3) show that this is the case for some covariates (decreasing value in the 2016-2018 period) for others the inverse is true i.e. the distributions became less similar (increasing values).

There are two possible explanations for this difference: Firstly, it could result from whether covariates were time-invariant or changed over time only due to endogenous processes (i.e. related to themselves). For example, the location of provincial capitals did not change in the data however average annual precipitation could have changed because of macro-scale climatic trends. Secondly the variance of both of these types of covariates is determined in part by the exogenous processes that change the distribution of all units over time, i.e. patterns of deforestation. For the time variant covariates, the influence of exogenous processes maybe occurring concurrently with the endogenous changes over time or either one may be occurring in isolation. Elucidating which of these circumstances is the case is difficult and as such this confounds explanation of why the variance between protected and unprotected areas did not increase as the former were expanded over time.

On this basis a more comprehensive method of testing for biases in PA location in Cambodia in the future would be to use the same covariates but instead to analyze the distributions of all units inside PAs not just those that were forested which would of course exclude the impact of the exogenous factors affecting the distribution of units.

Moving on to the second component of objective iii the identification of predictors of deforestation. The results showed that the strongest predictors of whether a given unit was deforested were its proximity to other surrounding FCL events, followed by elevation, distance to major roads and average annual precipitation (section 5.1.4). These factors and the nature of the relationships with deforestation are broadly in keeping with the results of other studies in Cambodia. For example, ICEM (2003a), Broadhead and Izquierdo (2010), Beauchamp *et al.* (2018); Ota *et al.* (2020) all also concluded that deforestation is more likely to occur closer to major roads. Similarly, this study's result of a greater probability of deforestation occurrence at lower elevations is corroborated by Beauchamp *et al.* (2018) and Lonn *et al.* (2018). As for lower average distances to surrounding FCL events being a strong predictor of deforestation this is a similar result to Beauchamp *et al.* (2018) who found that "deforestation was more likely to occur when areas are surrounded by a high proportion of non-forest" (*p. 439*). Overall, the fact that these results concur with the country-specific literature is unsurprising given that these relationships, similar to the biases in PA location, have been extensively confirmed by studies in many different contexts (see Geist and Lambin 2002 and Green *et al.* 2013 for an overview).

By contrast an unexpected result was that distance to ELCs which was expected to be strong predictor of deforestation based upon previous quantitative analysis (Michinaka *et al.* 2013; Davis *et al.* 2015; Beauchamp *et al.* 2018; Magliocca *et al.* 2020) was in fact one of the weakest (low values of SDM: Table 4: Section 5.1.4). The most likely explanation for this is that the means by which this covariate was implemented (by producing a Euclidean distance matrix from all ELC boundaries) was too crude a measure given the number and size of ELCs present in each outcome period. However, a minor effect was more visible for this covariate in the smoothed density plots (Figure 12: section 5.1.4).

As for the investigation into deforestation predictors between protected and unprotected areas. Whilst the PCA biplots showed that differences between the groups were minimal (Figure 13: section 5.1.4), the fact that the first two PCs explained a relatively proportion of the total variance (maximum of 38.2% in the 2016-2018 period) is an indication that overall deforestation is a complex process not easily captured by a small number of predictors. This is a similar conclusion to that of Eklund *et al.* (2019) who highlight that the occurrence of deforestation is dynamic in both a spatial and temporal sense and can shift between accessible and inaccessible regions. In this sense, one means to improve this aspect of this study would be to make the analysis more comprehensive by including all deforestation events not just those that occurred for the units identified as either treated or controls.

6.1.2 Main findings with regards to PA effectiveness

In fulfillment of the stated objective (section 3.2.2) the primary analysis of this study found a significant positive treatment effect (ATT) resulting from a unit of analysis being located inside a national PA for all three outcome periods (section 5.2.3). This corresponds to PAs exhibiting significantly less deforestation than their matched counterfactual unprotected areas which concurs with the generalized findings of the two previous quasi-experimental studies that performed similar analysis in Cambodia: Clements and Milner-Gulland (2014a) and Ota *et al*. (2020). Although the magnitude of the treatment effects observed are not comparable given differences in scale and temporal coverage. This finding is also supported by other quantitative analysis such as that of Beauchamp *et al.* (2018) who, whilst they did not utilize a quasi-experimental approach, did observe lower rates of deforestation inside PAs than outside. In a broader sense this overall result of a positive treatment effect of PAs with regards to an ecological outcome is also in keeping with the majority of studies in the wider context beyond Cambodia (Andam *et al.* 2008; Berefords *et al.* 2013; Carranza *et al.* 2013; Geldmann *et al.* 2013; Nolte *et al.* 2013; Pfaff *et al.* 2013; Vergara-Asjeno and Potvin 2014; Spracklen *et al.* 2015 Ament and Cumming 2016; Eklund *et al.* 2016; Bowker *et al.* 2017; Butsic *et al.* 2017; Yang *et al.* 2019).

However, within this general finding there was a trend of decreasing treatment effect between the outcome periods (Table 6, section 5.2.3), which when contextualised against the relative rates of total FCL events and the results of the secondary analysis, can reasonably be ascribed to a trend of decreasing PA effectiveness over time. This trend is hardly surprising given the history of poor natural resource management and accompanying widespread exploitation that has occurred both inside and outside of Cambodia's PAs as described in successive sections in chapter 2. Section 2.1.2 specifically highlighted the broader trends and events that the particular temporal period that was investigated was intended to characterise and as such it is pertinent to review these now in combination with the results.

Firstly, in terms of FCL at the national scale the first outcome period of 2010-2012 showed a comparatively greater area of FCL having occurred than in the 2013-2015 period (sum of FCL in all three years of each period: Figure 5, section 2.1.3). On the basis of this alone it would seem counterintuitive that PA effectiveness decreased over this time however when the area of FCL is divided into that occurring inside PAs vs. outside of them a different picture emerges. Figure 17 (section 5.2.3) showed that whilst the area of FCL occurring inside PAs was almost the same in the two outcome periods (~960 km^2) the amount of forest there was inside PAs to protect in the latter period was considerably lower. Hence the relative % of FCL in PAs between 2013-2015 was higher and meaning that protection was less effective.

Given that there is no evidence that suggests that this reduction in PA effectiveness over this period was due to endogenous factors such as a decline in funding or decrease in extent of management activities it is logical to assume that instead it is due to exogenous processes namely an increase upon the pressure of forest resources inside PAs. This increase in pressure upon PA resources is most likely due to the activities of the ELCs that were established inside PA boundaries from 2008 onwards (section 2.2.1). In this regard Figure 9 (section 2.3.1) shows that the area of ELCs established inside PAs on an annual basis increased from 2008 and more than doubled in 2011. Whilst this would appear to slightly pre-date the decline in PA effectiveness it must be remembered that due to the restrictions of the counterfactual study design employed these ELCs only began to be included in the 2013-2015 period (Appendix G). This is unlikely to produce a dissimilar effect from what occurred in reality given that many ELCs did not begin operations (land clearance) immediately after their licenses were issued (Beauchamp *et al* 2018).

This assertion of ELCs impacting the environmental outcomes of PAs and thus their effectiveness is supported by other sources (Broadhead and Izquierdo 2010; Global Witness 2015; Peter and Pheap 2015; Boyle and Turton 2019). However, in order to further validate it, additional analysis could be performed estimating treatment effects on an annual basis between the years of 2011-2015 to determine exactly what year the biggest reduction in PA effectiveness occurred. This could then be compared with RS imagery of land cover change from ELCs sites inside PA boundaries to give a more accurate indication of when they actually began operations.

By contrast the decrease in PA effectiveness between the latter two outcome periods of 2013-2015 and 2016-2018 is more likely to be the result of factors endogenous to PA management/operations. The reason for this is that, the area of FCL that occurred in each year at the national scale was generally lower than in 2013-2015 (although 2016 did see an increase vs. 2015: Figure 5, section 2.1.3), and whilst the area of FCL inside PAs did increase (Figure 17, section 5.2.3) this is overshadowed by the concurrent increase in the amount of forest to be protected given the expansion of the PA network (section 2.2.1). However, we do know that 2016 was a significant year for PA management in Cambodia with the MoE being transferred the jurisdiction over areas previously overseen by the FA and thus now being responsible for the whole national PA estate (section 2.2.4). At the time Souter *et al.* (2016)

warned of the potential negative implications that this change in conjunction with the expansion of the PA network could have on the management and effectiveness of PAs given the existing budgetary constraints and insufficient capacity of the MoE and it seems that the results of this study have validated their concerns. The lack of other sources evaluating PA effectiveness in Cambodia as recently as the 2016-2018 period make it harder to identify what aspects of PAME have been most compromised by this change. Although if the results of the PAME assessments that have taken place were made publicly available then these could be cross referenced against the results of this study to provide further insights in this regard.

Moving on to the results of the spillover analysis the main finding of which was a significant positive spillover effect across all outcome periods (section 5.1.5). In practical terms this means that units in 5km buffer zones surrounding PA boundaries were significantly less likely to be deforested as compared to matched counterfactual units in the wider unprotected landscape. The fact that this result generally fits with that of the primary analysis is positive although it is noteworthy that the trend of changes in the magnitude of the treatment effect between outcome periods did not entirely match. More specifically in the primary analysis treatment effect sizes declined across all subsequent periods whereas for the spillover effect there was a decline between the 2010-2012 and 2013-2015 periods but an increase in 2016-2018 (Table 5, section 5.1.5).

Comparison to the other in-country studies offers little in the way of explanation for this as their results differed, with Clements and Milner-Gulland (2014a) observing a possible negative spillover effect although their analysis period was prior to that of this study. Whereas Ota *et al.* (2020) found a positive effect for protected forests but not for other PAs and they only analysed up to 2016. However other studies in the wider literature do offer some possible explanations. Firstly, the spillover effects resulting from PAs have been acknowledged as varying over different spatial extents as well as with regards to the effectiveness of PAs themselves (Pfaff and Robalino 2012; Pfaff *et al.* 2013; Ament and Cumming 2016). Given that the results of the secondary analysis observed different magnitudes of treatment effects between PAs established in different time periods it is possible that this is causing heterogenous spillover effects to occur. On this basis it would be useful to extend the spillover analysis by not just different buffer zone sizes but also differential effects using the PA establishment categories of the secondary analysis to see if the trends are similar i.e. do younger PAs exhibit stronger positive spillover effect that declines over time?

Although Pfaff and Robalino (2017) also highlight how the mechanisms by which spillover effects are generated are complicated and also play a role in determining the nature and size of effects observed. For example, the establishment of PAs alters localized economic conditions such as demand for forest and agricultural products as well the availability of land, the knock-on effects of which may differ for example stakeholders may choose to pre-emptively clear land thereby increasing deforestation or alternatively they may choose to migrate to areas further from PAs with less restrictions upon resource use (*p. 302*). Simultaneously the strategies/activities employed by PAs themselves also influence spillover effects, such as the relative deterrence effect of law enforcement vs. community education and development programs (*p. 304*). Considering that such factors were not controlled for in the matching process in this study in theory they be could responsible for the result in 2016-2018.

Alternatively, another possible explanation is the nature of the forest clearance that is occurring in the buffer zone in any given period. The spillover effect may have been greater in the third outcome period because by this point only low value timber was remaining and hence there was little incentive to clear it whereas those that would have engaged in this activity instead turned their attention to higher value timber left inside PAs (Singh 2014). This notion highlights a fundamental conceptual limitation of the form of analysis employed in this study, namely that deforestation is treated as somewhat of a uni-dimensional process to be predicted by external factors rather than the different motivations that drive it, something which will be discussed further in section 6.3.2.

Finally, there is the results of the secondary analysis which displayed the same overall trend of significant positive PA avoided deforestation over time albeit with differences in the magnitude of the treatment effect dependent upon the period in which PAs were established (section 5.3). In this regard the main finding was that the most effective group of PAs in each outcome period were those that had been established the most recently although between outcome periods it was these PAs that suffered the greatest reduction in treatment effect size. Again, this result is not comparable to the Cambodian studies by Clements and Milner-Gulland (2014a) and Ota *et al.* (2020) as they did not analyze multiple outcome periods. Although this phenomenon of younger PAs being more effective has been evidenced by other quasi-experimental studies such as Paiva *et al.* (2015); Blackman *et al.* (2015); Kere *et al.* (2017) and Bowker *et al.* (2017). However, it is not definitive as others have observed the inverse relationship (Butsic *et al.* 2017) or insignificant differences (Zhao *et al.* 2019) indicating that there are likely other factors involved. On this point Kere *et al.* (2017) make two important observations: First that there should be a contextual justification why younger PAs are more effective and second that any apparent differences in effects between PAs of different ages must be viewed in light of their respective localities.

Applying this logic to the findings of this study a possible explanation for why the PAs established between 2001-2010 were more effective in both the 2010-2012 and 2013-2015 outcome periods is that they were likely subject to better planning and design as well as increased financial and technical support from international conservation organizations than those of the early establishment group of 1993-2000. Indeed, Clements and Milner-Gulland (2014a) note that some PAs did not have any active management until they received donor-led support in the mid 2000's. In addition to this, it is a reasonable assertion that after close to a decade of managing the early PAs (1993-2000) that the level of capacity within the MoE and FA would have improved to some extent despite being acknowledged as still being limited (ICEM 2003b; Lacerda *et al.* 2004). The same argument can be made for the new PAs established between 2011-2016 that were the most effective in the 2016-2018 period.

The trend of more recently established PAs displaying a larger reduction in treatment effect between outcome periods in proportion to the older PAs is also interesting. Although, again, without detailed information on aspects of management effectiveness or funding it is only possible to speculate as to what is causing it. One logical explanation is that at the outset of establishment of new PAs, support from central institutions and external organisations is strong, management have targets to meet and something to prove and staff motivation is high. Thus, in the early years the PA achieves good results in terms of reducing pressure as evidenced by high avoided deforestation. However, as in other types of conservation programs, this early progress can sometimes represent 'low-hanging fruit' (Coll *et al.* 2015) i.e. objectives that are addressed first on the basis that they are relatively achievable and yet produce substantial results, perhaps at the expanse of tackling more pernicious problems. For example, in PAs this could be the implementation of law enforcement activities where none were previously present. Initially this is likely to achieve good short-term results by reducing the volume of illegal activities but in isolation it is unlikely to eliminate the persistence of said activities in the long term. The implication of this is that after short-term gains are made, PA effectiveness can effectively 'plateau' as the progress in addressing more complex threats is slower. This in turn can lead to declining motivation of staff which can be compounded by waning support from central institutions and donors as new projects become more attractive. This combined with the emerging trend of increased scrutiny with regards to the funding of PAs by larger multilateral donors evidenced both in Cambodia (Souter *et al.* 2016) and in a wider (global) context (section 1.2.1) could very well explain why the effectiveness of newly established PAs in Cambodia declined quickly.

However, the second observation regarding the localities of PAs of different ages representing a confounding effect cannot be overlooked (Kere *et al.* 2017). In this study treatment effects were estimated for the different establishment dates of PAs separately, this means that whilst matching was conditioning for the differences between these PAs and the whole control sample it did not then condition for the differences between the matched samples produced for each group. This allows the possibility that the control units matched to certain groups of PAs displayed substantially different average outcomes due to the differences in the distribution of deforestation occurrence. The implication of this is that it is these differences may be influencing the treatment effect sizes more than the age of the PAs themselves. Ultimately this phenomenon was not assessed in this study and thus no conclusion can be made as to whether it indeed explains the observed difference in PA avoided deforestation with age. In hindsight this could have been tested for by comparing the density distributions or summary statistics for the covariates of the matched samples across the different groups of PA establishment dates. Although, given that this analysis involved sub-sampling of uneven sample sizes this process would not only have been protracted but also potentially too coarse to highlight differences. Instead a better approach to mitigate for this confounding of effect of PA locality would instead be to utilise an alternative model of treatment effect estimation as part of the matching process that analyses the different groups in combination with one another. This would involve the treatment being conceptualised not as dichotomous (untreated vs. treated) but as polychotomous (untreated vs. treated^{X}: treated^{Y}) which can be achieved using a multinomial logistic regression model (Kere *et al.* 2017). Such a technique is something that could be employed if the analysis of this study is expanded upon in the future.

In summary the principal findings of this study namely: a positive effect of PAs albeit with a decline in magnitude over time, and that newly established PAs were more effective than older ones do hold up to comparison with both quantitative and qualitative sources from Cambodia as well as trends highlighted by the wider literature on quasi-experimental PA assessment. Even if the explanation for the latter trend is difficult to confirm conclusively without further analysis.

6.2 Limitations of the study

6.2.1 Practical limitations of analysis

The different processes involved in the quasi-experimental methodology used in this thesis presented their own limitations and compromises had to be made at different stages. At the data acquisition and preparation stage (sections 4.1 and 4.2), one of the primary examples of this was that no viable means of validating the forest cover and FCL data, upon which assignment to treatment and control groups as well as the outcome variable depend, was found. In addition to this, despite the dataset of ELC locations being comprised through a process of cross-checking the two most comprehensive sources available (Appendix F) there is still a recognized degree of uncertainty associated with this data which is significant given that ELCs represented a substantial amount of land excluded from PAs in the analysis. The implication being that if inaccuracies in the data meant that PA land was excluded when it shouldn't be or vice versa then this might have produced different results of avoided deforestation.

Overall these are the same concerns that plague all analysis of this type, and from a practical perspective there is little that can be done about them as even if it were possible to quantify the error in individual covariate data for example, methods of propagating this in the estimation of the treatment effect are yet to be developed (section 1.3.4.5).

As for limitations that were encountered in the preliminary analysis steps prior to matching the most significant issue was that neither of the methods employed to mitigate for the presence of spatial-autocorrelation (SAC) in the data were successful (Appendix J). In addition to this further testing found that SAC was still significant after sub-sampling in the primary analysis (section 5.2.2) meaning that it is likely influencing the results of the treatment effect estimates in this analysis as well as the spillover and secondary analyses. The persistence of SAC is certainly not an issue for this study alone, in fact a recent study by Negret *et al.* (2020) tested four different models to account for SAC in a matching based assessment of PA effectiveness and none were successful. This is particularly worrying given that a large number of studies of this kind in the wider literature either ignore SAC or assume that random-sampling of the treated and control populations will be sufficient to mitigate it (See Table A1: Appendix A) whereas this study has shown that this is evidently not the case. This a clear indication that further research on understanding the processes by which SAC occurs and how its effects can be accounted for in quasi-experimental studies is needed.

Finally turning to limitations in the statistical matching portion of the analysis, the most notable of which was that given the computational resources available it was only possible to analyze for treatments effects covering ~10-12% of the total populations of treated and control units available in each outcome period across the primary, secondary and spillover analyses (refer to sections 4.3.6, 4.4 and 4.5). This means that the region of support (Schleicher *et al.* 2019) for the estimates of ATT is possibly limited and hence it is debatable whether in fact they constitute measures of Sample Average Treatment Effect on the Treated (SATT). Similar to SAC this is an issue for many other studies in the literature, a good deal of which have analyzed total samples much smaller in size than that achieved in this study (See Table A1: Appendix A). Furthermore, these studies rarely discuss the implications of this with regards to the broader inferences they draw from their results.

In terms of addressing the computational constraints of statistical matching the simplest approach is to use a coarser resolution of the unit of analysis although this is hardly a panacea as it risks introducing aggregation-bias (Blackman 2013). Instead this study replicated the technique used by Nolte and Agrawal (2013) by analyzing successive sub-samples and calculating an average ATT from them however as mentioned there has been no detailed investigation of the implications of this in terms of the quality of matching achieved. This study has made a contribution to this discussion as testing showed that indicators of matching quality did not vary substantially under different samples sizes (Appendix K). Although this should still be expanded upon by more rigorous investigation.

Beyond this issue of constraints in sample size another limitation of the matching analysis was that neither technique that was tested resulted in a perfect post-matching covariate distribution (Appendix K), and neither was this achieved using PSM in the actual analysis (sections 5.1.5, 5.2.1, 5.3, Appendix O). In theory this weakens the causal inference of the treatment effect estimates even though the Abadie and Imbens (2011) bias-adjusted method of ATT calculation was used to mitigate for this (section 4.3.4). Of course, to avoid this it would have been possible to exhaustively test other specifications of matching that may have resulted in better covariate balance. An additional technique that was trialed but found to not be feasible was to use the 'GenMatch' function in the Matching package (Sekhon 2020) which minimizes variance by using an evolutionary search algorithm to iteratively test different covariate weightings schemes in the calculation of the desired multivariate distance metric (e.g. propensity score or Mahalanobis distance). When tested this matching algorithm ran for >18 hours without producing a result for a similarly sized sample as was tested in Appendix K, although there is nothing to suggest that with additional processing power it would not be successful. An alternative to this would be test further matching specifications under the PSM approach such as matching without replacement, different ratios of treated to control units, and a more restrictive caliper than the 0.5 SD that was used previously, all of which have the potential to yield improved post-matching covariate balance if this research is to be expanded upon in the future.

6.3.2 Conceptual limitations of methodology

Turning instead to the conceptual limitations in the quasi-experimental methodology of this thesis. Section 6.1.1 already highlighted that Cambodia's relatively high PA coverage meant that it was difficult to ascertain biases in PA location. However, this is also problematic when it comes to the estimation of treatment effects using matching methods as it meant that there was a ratio of less than 1:2 treated to control units in the populations for analysis (Table 3, section 5.1.1). Whilst it was possible to artificially adjust this ratio in the sub-sampling that was used for matching (a 1:3 ratio was used, section 4.3.4) this does not change the fact that in reality unprotected forest is not that abundant compared to protected forest in Cambodia. This in fact may explain why matching was not able to achieve better covariate balance improvements as the unprotected forest that does exist is too dissimilar to the protected forest. If this is true then it implies that perhaps a *with-versus similar without* study design as used in this thesis is not best suited to analyzing PA effectiveness in Cambodia. This does not mean that such analysis is not applicable but that instead a counterfactual BACI or fixed effects regression design, which would be more robust to differences between the treated and control groups, might be more appropriate (Wendland *et al.* 2015). Ultimately though there are no objective rules regarding what constitutes an acceptable ratio of treated to control units or the maximum acceptable level of dissimilarity between the groups, leaving the decision up to investigators which is something of an inherent weakness of the methodology.

Another limitation as alluded to in section 1.3.4.2 is that because this study did not conduct analysis on an annual time step this meant that variables that are in reality are time-variant were constrained into a time-invariant form within each outcome period. Appendix G detailed how this meant that some ELC and PA land that was not established in the first year of the outcome period was excluded until the subsequent outcome period. However, constrains of data availability also meant that for some covariates the same data had to be used for all periods, for example distance to roads, which is especially problematic given that we should have expected that this is a variable that would show substantial variation over time. This is another issue with counterfactual matching study designs that goes largely unaddressed in the literature, likely because there is no easy solution to it. Given that such covariates as distance to roads have been consistently found to be strong predictors of both PA location and deforestation occurrence (Section 1.4.2) it doesn't make sense to drop them entirely. However, in future more effort should be made to source temporally partitioned data, perhaps by utilizing open access big data sources such as the OpenStreetMap project (OSMF 2020) (although admittedly crowd-sourced data has its own issues of accuracy). Failing this, researchers should at least provide more clarity as to the temporal limitations of the data they use.

Another conceptual issue associated with temporality in counterfactual analysis is that of problems of endogeneity with respect to causality (Antonakis *et al.* 2014). Within this thesis two such problems were identified at different scales. First the causal relationships between the covariates and the outcome variable of deforestation occurrence are not necessarily uni-directional, this is summarized aptly by Mertens *et al.* (2004) as follows: "it is hard to distinguish situations where human settlements, productive activities, and infrastructure are located in certain places because those places have environmental conditions that make them good to deforest from those where deforestation occurs because people settle, build roads, or make specific zoning decisions." (*p.90*). In the case of Cambodia, the conclusion of modelling by Beauchamp *et al.* (2018) led them to suggest that the latter causal relationship was occurring although this is unlikely to be homogenous across the whole country.

Second, with regards to the inferences made about PA effectiveness validated against crude indicators of deforestation pressure (Sections 5.2.3 and 5.3). This is because the relationship between the 'pressure' on a PAs resources and the effectiveness of PA management in terms of mitigating this pressure is complex. Both are multi-faceted concepts that are occurring simultaneously, changing continuously and influenced by separate exogenous factors, hence ascribing causality is difficult and discretizing them into outcome periods is inherently limiting. Logically one way to deal with these endogeneity problems is by continually improving the causal model to reduce uncertainty (Ferraro and Hanauer 2014; Baylis *et al*. 2016). In practical terms this can mean increasing the resolution of the analysis, temporally but also spatially, and possibly including additional variables, for example in the situation of inferring reduced PA effectiveness from lower avoided deforestation estimates by comparing pressure across different time periods (section 5.2.3) being able to also examine the trend in PA management inputs would have improved the strength of this conclusion. Of course, there will always be a tradeoff between what level of certainty with regards to casual inference is deemed acceptable in light of the costs (time and money) of sourcing and analyzing further data.

At a higher level of abstraction, it is important to reconsider the conceptual critique introduced in section 1.4.3 that perhaps avoided deforestation may not be the most appropriate ecological outcome by which to assess the effectiveness of Cambodia's PAs. The justification for choosing was that the country's PAs do contain a considerable portion of its remaining forest and this has always been a critical resource (section 2.2.3). However, this outcome obviously precludes the fact the PAs may have been effective in preserving other valuable non-forest habitat types known to be present and threatened such as wetlands (ICEM 2014).

A final limitation in the methodology of counterfactual PA outcome assessment used in this thesis is the conceptual boundaries that are applied to the outcome of deforestation. More specifically, whilst statistical matching improves causal inference by attempting to ensure that differences in the outcome variable are the result of the treatment only, it is inherently unable to account for the fact that outcomes such as deforestation are the result of decisions by human agents for which the rationale may differ (Eklund *et al*. 2016). For example, two widely recognised drivers of deforestation in Cambodia are the conversion of land for agriculture (Kong *et al.* 2019) and the selective extraction of high-value timber species (Global Witness 2015). However the very nature of PAs means it is highly likely that deforestation within them is being driven more by the latter process rather than the former as attempting to engage in agriculture within PAs is more risky as the time-scale of the activity means there is an increased probability of being caught and forced to abandon the cleared land. Whereas selective illegal logging, particularly at a small scale, is comparatively easier to perpetrate in PAs and is known to be an issue in Cambodia (Singh 2014). Vice versa in the unprotected forests conversion for agriculture is likely the more dominant driver especially as most high-value timber worth extracting will likely been cleared already given the history of natural resource exploitation in the country (section 2.1).

Hence even though the results of section 5.1.4 indicated that the predictors of deforestation in PAs and unprotected areas in terms of the covariates were similar, if in fact deforestation is occurring for very different reasons in each area then this weakens the counterfactual comparison created using matching. It would be possible to account for this phenomenon by using a polychotomous outcome variable i.e. Y_1= forest cover retained; Y_2 =FCL: conversion for agriculture; Y_3=FCL: timber extraction. Although this would require a robust means of distinguishing between these outcomes from the RS data and this has been widely acknowledged as a challenge (Eklund *et al.* 2016; Singh *et al.* 2018).

Overall, despite the limitations that have been described it is important to recognise that counterfactual analyses that are able to reduce some of the uncertainty with regards to impact evaluation of PA outcomes still represent a marked improvement over naïve assessments that do not account for biases (Ribas *et al.* 2020) and this can only improve with further development of the techniques involved.

6.3 Implications of study results

6.3.1 Practical implications for stakeholders

The fact that this study has demonstrated that PAs in Cambodia produce tangible benefits in terms of avoided deforestation as compared to the counterfactual of them not being present is something of a vindication for those responsible for their continued operation in the face of long-running criticism (section 2.2). This finding should be utilized by conservation practitioners as empirical justification for continued funding for PAs into the future. In this regard the trend of decreased PA effectiveness over time should be particularly highlighted to stress how increased funding of PA management is needed to reverse the trajectory of decline. In addition to this the positive spillover effects observed should also be leveraged to demonstrate that PAs in Cambodia have ecological benefits beyond their boundaries in combination with the socio-economic benefits found by Clements and Milner-Gulland (2014a) and Clements *et al.* (2014).

The results of this study should also be considered in decisions of how spending on PAs is allocated going forward. If indeed the decrease in effectiveness observed between the 2013-2015 and 2016-2018 period stems from the MoE being over-stretched in terms of managing an expanded PA estate (section 6.1.2) then it seems ill-advised to establish any more PAs in the near future. Instead, there is a clear incentive for funding to be used to persist with improving PA management. Ideally this should focus on the newly established (2016) PAs in an attempt to buck the trend of these becoming less effective over time as observed in this study. Further to this additional consideration should be given to placing REDD+ sites in these PAs given that they are more likely to be successful at least in the short term. More specifically these sites should be located in areas that show lower human populations as this was found to be the strongest factor affecting deforestation occurrence inside PAs (section 5.1.4). The same holds in terms of selecting potential locations for the re-introduction of the wild Tiger although of course this must be weighed up against the placement of REDD+ sites as logically the two are not compatible.

Finally, if it wasn't already clear from the previous evidence (section 2.2.2.1) this study has shown that the policy of allowing the development of resource-extractive industries inside PAs was likely a strong contributor to a reduction in their environmental effectiveness. Whilst this was primarily demonstrated for the period of 2013-2015 there is a chance that the effects extend beyond this as well (section 6.1.2). This serves as a clear message that the continued allocation of PA land in this manner in the future is highly inadvisable and indeed any remaining developments that have not yet started operations (particularly mining and hydropower) should either be revoked or at the least monitored closely.

6.3.2 Implications for the field of quasi-experimental PA effectiveness assessment

The previous sections 6.1 and 6.2 have already highlighted some of the contributions that this study can be considered as having added to the field of quasi-experimental PA outcome assessment although it is useful to summarize these collectively. From a methodological perspective this study has provided additional evidence that the use of sub-sampling can make the process of statistical matching computationally achievable when processing capacity is limited. Additionally, that the techniques of accounting for SAC in GLMs using either simplistic X/Y coordinate predictors or through the creation of a SAC autocovariate were not successful. As for the results, this study adds further weight to the growing consensus that PAs show positive but weak avoided deforestation as compared to counterfactual unprotected control areas (sections 1.4.1 and 6.1.2). The results of this study also serve to further the discussions around how the duration of time since PA establishment is related to the effectiveness of outcomes as well as relationship between trends in the sizes of spillover effects and those of overall PA effectiveness.

Finally, from a practical perspective this study has shown that given the increasing coverage and quality of big data available it is possible for researchers or conservation practitioners to conduct robust quasi-experimental analyses such as this without extensive costs or resources. Indeed, this was the motivation behind making the R scripts used for the analysis openly available through GitHub so that others can adapt them to their own needs in future (section 4.6).

6.3.3 Recommendations for further study

Again some of the discussion of results and limitations has already entailed some recommendations for further study including: testing spillover effects for PAs of different ages separately (section 6.1.2), more comprehensive methods of ATT calculation to mitigate differences in locality when analyzing PAs of different ages (6.1.2) and different matching approaches to improve covariate balance (6.2.1). However, in addition to these a number of other avenues for further research became apparent over the course of this study.

First, there is a clear precedent from both the wider literature (section 1.4.1) and within country sources (Beauchamp *et al.* 2018; Ota *et al.* 2020) to test whether the management categories or size of individual PAs are also predictors of their outcomes in terms of avoided deforestation. These results could then be compared to those of PA establishment period in this study to see if any interactions exist. Furthermore, given that the involvement of international conservation organizations was posited as an explanation for why PAs established between 2001-2010 were more effective than those created earlier (section 6.1.2) then there is a rationale to test for differences in PA outcomes for those being supported by NGOs vs. those that are not.

Another interesting possibility would be to perform a BACI analysis for the PAs established in 2016, using matching in combination with a fixed effects regression model. As mentioned in section 1.3.1 opportunities for such study designs are rare given data availability although the relatively recent PA establishment date makes it possible in this case. Such an analysis would offer insight into whether these PAs were effective compared to not just to matched counterfactual but to when the same land was actually not protected. A similarity in this regard would lend further credence to the results of this study by validating the process of counterfactual creation.

A final suggestion for expanding this research in Cambodia would be to build upon the work comparing avoided deforestation generated by the national PAs to other forms of protected land. Ota *et al.* (2020) analyzed the relative effectiveness of PAs vs. CFs but no investigation has been made including CPAs. Also given that many CFs and CPAs actually intersect national PAs it would be interesting to follow the approach adopted by Anderson *et al.* (2018) and analyze the effectiveness of these different conservation modalities where they overlap and where they exist in isolation.

7. Conclusion

In summary this thesis planned a series of objectives for a quasi-experimental counterfactual analysis of PA avoided deforestation in Cambodia based upon knowledge gaps identified in both the country-specific literature as well as the wider field of research. Propensity score matching with a combination of 10 biophysical and socio-economic covariates was used to account for the widely acknowledged confounding effects of non-random biases in PA location and the occurrence of deforestation across the landscape. This analysis produced results that addressed all of the stated objectives, with the principal findings being that PAs in Cambodia showed a significant positive effect in generating avoided deforestation between 2010-2018. In practical terms, in the period of 2010-2012, this equated to a 30x30m area of forest inside a PA being as much as 8% less likely be to deforested than a similar area of unprotected forest in the rest of country.

However, the magnitude of this effect and thus PA effectiveness was observed to decline over time. Placed in the context of the circumstances and events surrounding conservation in the country the nature of the factors responsible for this trend are likely to have also changed over time from being exogenous (increase in pressure upon PA resources due to the establishment of agro-industrial concessions between 2010 and 2015) to endogenous (an increased burden upon a managing institution with weak capacity and shortage of funding, from 2016 onwards). In addition to this it was found that newly established PAs consistently performed better than older PAs however they also exhibited greater declines in effectiveness over time. Although given the lack of available information on management input as well as the confounding effect of PA locality the explanation for this result is more difficult to confirm.

Finally, this thesis also found a significant positive spatial spillover effect of PAs corresponding to a reduced probability of deforestation in surrounding 5km buffer zones between 2010-2018. However, the trend of this result did not entirely match that of overall PA effectiveness which is further evidence that such PA spillover effects may be heterogenous in nature and warrant further investigation.

Through its results and methodology this thesis also highlighted several areas in the research domain that require further study. Perhaps most important of these is to develop better understanding of, and means of mitigating for, the influence of spatial autocorrelation when assessing PA environmental outcomes using statistical matching.

In conclusion, the most salient point to take away from this thesis is that whilst assessments of the ecological outcomes of PA networks are a useful tool to highlight to managers which PAs may need further attention, used in isolation they give little insight into the nature of the changes that are required in order to improve these outcomes. This is exactly why a holistic approach to PA assessment is needed with dedicated planning and ongoing monitoring of management effectiveness as well as both ecological and socio-economic outcomes. In essence this is what the newly established IUCN Green list program (section 1.2.2) hopes to achieve but it is clear that integrating these different components still requires a lot of work. In this regard perhaps the key achievement of this study is that it has shown that it is possible to conduct a rigorous quasi-experimental investigation of PA environmental outcomes using open source software and data, with limited computational resources. Thereby demonstrating that such an approach to assessment can, and should, be more widely adopted by institutions responsible for PA management as it requires minimal training and cost investment whilst being able to produce policy-relevant results.

8. References

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9. Appendixes

A. Details of recent quasi-experimental studies of PA ecological effectiveness

Table A1: Details of quasi-experimental studies of PA effectiveness in forest related outcomes

Study	Region of analysis	Treatment	Outcome variable	Comparison type/s	Empirical technique/s	Control for spillover effects	Consideration of spatial autocorrelation	Sampling method/size	Outcome periods	Outcome data resolution	Covariates/ confounders
Abman 2018	Global	Protected area's Rule of law	Forest cover change	With-versus-similar without	Matched: weighted least squares regression	10 km buffer	No	Random sample: 5 million pixels	2000-2012	30x30m	Elevation Slope Distance to Roads Distance to population centre Travel time to city Agricultural suitability (soil, climate and terrain) Distance to river/water body Terrestrial biome
Ament and Cumming 2016	South Africa	National parks	Natural land cover loss	With-versus-similar without	Covariate matching with caliper: one to one matching	10km buffer	Enforced distance between sample points: 100m	500 random sample points in each park; 250,000 random control points	2000-2009	30x30m	Terrain (elevation, slope) Climate (annual precipitation) Accessibility (distance to towns, distance to roads) Social-ecological state (land cover in 2000).

Andam *et al.* 2008	Costa Rica	Protected areas	Forest cover change	With-versus-without With-versus-similar without	Unmatched (naïve comparison) vs. covariate matching: Mahalanobis distance with calipers *Also tested:* Inverse weighted covariate matching Kernel propensity score matching covariate matching with a genetic algorithm	Tested different buffer widths: 0-2; 2-4; 4-6; 6-8km	No	Random: 20,000 plots		3 hectares	Distance to roads Distance to forest edge Land use capacity: slope, soil characteristics, humidity Distance to nearest major city *Extended covariates* Distance to railroads and rivers District level population density Proportion of immigrants Adults educated beyond secondary level Households fuel wood use Size of district
Andam *et al.* 2013	Costa Rica	Protected areas IUCN management category	Forest re-growth	With-versus-similar without	Covariate matching: Mahalanobis distance with calipers	5km buffer	No	Random	1967-1979 1981-1997	3 hectares	Distance to roads Distance to Major cities Distance to forested parcel at baseline
Beresford *et al.* 2013	Africa	Important Bird Areas	Forest cover change	With-versus-similar without	Covariate matching: unspecified distance measure	20km buffer	Enforced distance between sample points	Grid-point sampling	1990-2010	30x30m	Altitude Population density Distance from roads Land-cover

Blackman *et al.* 2015	Mexico	Natural protected areas	Forest cover change	With-versus-similar without	Propensity score matching with caliper Covariate matching: Mahalanobis distance Probit regression with matched controls	20km buffer	No	Intersected a 2-km rectangular grid with the study area and sampled the plots where the gridlines crossed. Sample= >500,000 plots	1993-2000	30x30m	Communal land tenure More than 75% pop. locality indigenous? Travel time to nearest city Elevation Slope Median annual precipitation
Bowker *et al.* 2017	Africa	Parks (Protected areas)	Forest cover change Park extent (size) Age Location	With-versus-similar without	Covariate matching: unspecified distance measure	10 km buffer	Randomly sampled points spaced at least 500m apart	Random: 500 treatment, 1,000,000 control	2000-2013	30x30m	Distance to roads Distance to Major cities Annual precipitation Elevation Slope
Bragina *et al.* 2015	Western Caucasus	Protected areas under different IUCN categories	Rate of forest canopy removal	With-versus-similar without	Propensity score matching with caliper: 1 to1 matching without replacement		Enforced distance between sample points: 300m	Random: 1%	1985-1990 1991-1999 2000-2010	30x30m	Distance to nearest road Distance to major city (Sochi) Slope
Brandt *et al.* 2015	Yunnan, China	Protected areas vs. Tibetan sacred areas Areas under logging ban	Forest cover change Forest type: Old growth, pine	With-versus-similar without BACI	Covariate matching: Mahalanobis distance	No	Enforced distance between sample points: 1000m	NA	1990–1999 1999–2009	200x200m	Road density Road density squared Distance to Shangrila Elevation Elevation squared Slope Slope squared % edge forest % core forest % old growth forest % pine forest % snow

Brun *et al.* 2015	Indonesia	Protected areas IUCN management category	Deforestation	With-versus-similar without	Autologistic and mechanistic spatial autoregressive models built upon the Bayesian framework using Markov Chain Monte Carlo simulation	-	Accounted for by model type	-	2000-2010	250m	Elevation Cost to transport timber to nearest city Gross rent (in terms of national production value of top 7 crops Historical illegal logging hotspots logging concessions and wood plantations
Carranza *et al.* 2013	Cerrado, Brazil	Protected areas	Forest conversion	With-versus-similar without	Propensity score matching with caliper (exact matching without replacement)	10km buffer	No	Random sample for control set	2002-2009	250 x 250m	Accessibility: distance to paved roads; nearest state or federal capital city; travel time to the nearest city with population ≥ 50,000 Agricultural suitability: soil fertility; slope; salinity; risk of flooding Rain-fall Vegetation type Elevation
Chai *et al.* 2009	Jamaica	National Park	Forest clearance re-growth fragmentation	BACI	Unmatched comparison: ratio of means	N/A	N/A	-	1983–1992 1992–2002	30x30m	-
Clements and Milner-Gulland 2014a (Clements *et al.* 2014 represents the same study)	Kulen Promtep Wildlife Sanctuary and Preah Vihear Protected Forest, Cambodia	Protected areas Payments for Ecosystem services schemes	Deforestation rate Household well-being	BACI	Covariate matching: Mahalanobis distance with calipers	4-12km from PA boundary	No	-	2001/2002 - 2005/2006 2005/2006 - 2009/2010	1km 2	Forest cover within 5 km of the village Village size Distance to roads Distance to nearest village Baseline forest cover (2001/2002) Slope Distance to markets

Eklund *et al.* 2016	Madagascar	Protected areas	Deforestation	With-versus-similar without	Covariate ratio matching (1:500): Mahalanobis distance	No	No	Random: 10%	1990-2000 2000-2010	30 x 30m	Distance to roads Distance to major cities Distance to rivers Annual rainfall Elevation Slope Forest type
Eklund *et al.* 2019	Madagascar	Protected areas	Deforestation	With-versus-similar without	Covariate ratio matching (1:500): Mahalanobis distance	No	No	Random: 10%	2005-2010	30 x 30m	Distance to roads Distance to major cities Distance to rivers Annual rainfall Elevation Slope Forest type PA shape and area PAME scores
Ferraro and Hanauer 2011	Costa Rica	Protected areas	Avoided deforestation and poverty alleviation	With-versus-similar without	Propensity score matching	No	No	random sampling 23,000 units	1960-1997	3ha	Land productivity class Distance to forest edge Distance to road Distance to major city
Ferraro *et al.* 2013	Bolivia; Costa Rica; Indonesia: Thailand	Protected areas IUCN management category	Deforestation	With-versus-similar without	Covariate matching: Mahalanobis distance or inverse covariance	Tested 5km buffer	Random sampling	(20,000 unit sample in Bolivia, Costa Rica and Thailand, 26,154 for Indonesia	Bolivia: 1991-2000 Costa Rica: 1960-1997 Thailand: 1973-2000 Indonesia: 2000-2006	(100m^2 pixels for Bolivia, 3ha pixels for Costa Rica, 900m^2 Thailand and 1km^2 pixels for Indonesia	Country specific covariates including: Distance to roads Distance to major city Slope Elevation Soil measures Distance to forest edge Distance to port
Gaveau *et al.* 2012	Sumatra, Indonesia	Protected areas	Deforestation	With-versus-similar without	Propensity score matching	No	No	11% sample	1990-2000	25km^2	Slope Elevation Distance to forest edge Distance to roads

Gaveau *et al.* 2013	Indonesian Borneo	Protected areas vs. agricultural concessions	Deforestation	With-versus-similar without	Propensity score matching, with replacement and caliper (0.25 SD)	No	2km minimum distance between sample plots	Given sampling restriction small sample of n= ~3400 units	2000-2010	1km^2	Slope Elevation above sea level Travel time to roads Travel time to cities Distance to oil palm mills and plantations Soil type Administration
Green *et al.* 2013	Eastern Arc Mountains of Tanzania	Protected areas	Forest and woodland conversion	With-versus-similar without	Generalised additive model	-	Included dummy variable as a random effect in models	-	1975-2000	500m	Distance to Forest edge Travel time to nearest city Distance to Roads Distance to Markets Altitude Slope Land value Distance to water Annual precipitation Water deficit Population density
Haruna *et al.* 2014	Panama	Protected areas	Deforestation	With-versus-similar without	Propensity score matching	No	No	-	1992-2000 2000-2008	100m^2 (1992-2000) 900m^2 (2000-2008)	Elevation Slope Distances from roads, urban areas and rivers Provinces Life zones/ecoregions Agricultural suitability
Knorn *et al.* 2012	Romania	Protected areas	Forest disturbance	With-versus-without	Unmatched comparison	5; 10; 15; 20 km buffers	No	-	1987-2009	1 ha	-

Miranda *et al.* 2016	Peru	Protected areas	Deforestation Household socio-economic impacts	With-versus-similar without	1:1 covariate matching using Mahalanobis distance	Spillover analysis for socio-economic outcomes but not deforestation	No	Intersected a 1-km rectangular grid with the study area and sampled the plots where the gridlines crossed. Sample= ~337,000 units	2000-2006	30x30m	Elevation Slope Aspect Average precipitation Average maximum/mean temperature Distance to nearest population centre Proportion of land suitable for forestry
Negret *et al.* 2020 (under review)	Colombia	Protected areas	Deforestation	With-versus-similar without	Propensity score matching with caliper (0.25 SD), without replacement with matching performed at different scales	No	Tested four different models for including SAC in estimation of treatment effects from matched data	-	2000-2015	$1km^2$	Initial forest cover Biotic regions Population density Intensity of armed conflict Distance to major rivers Distance to mining concessions Distance to exploited oil wells Distance to coca plantations Distance to paved road, Distance to unpaved road Elevation Surrogates of land-use potential (Slope; biotic region)
Nolte and Agrawal 2013	Amazon basin ecoregion	Protected areas: subdivided into those with low and high PAME (METT) scores	Forest fire occurrence	With-versus-similar without	Covariate matching: Mahalanobis distance with replacement and caliper of 1 SD	No	Randomly sampled a small percentage of forest parcels (2%) from the entire population	see left	2000-2010	$1km^2$	Elevation Slope Travel time to major city Distance to forest edge Average annual precipitation

Nolte *et al.* 2013	Brazilian Amazon	Strict vs. sustainable -use protected areas	Deforestation	With-versus-similar without	Matching with replacement and caliper (1 SD)	Control units further than 10km from PA boundaries	No	5% random sample	2000-2005 2006-2010	$1km^2$	Baseline forest cover Distance to forest edge Travel time to major cities Slope Terrain Probability of flooding State
Ota *et al.* 2020	Cambodia	Protected areas, protected forests and community forest	Deforestation	With-versus-similar without	Generalized boosted models using the inverse probability of treatment weighting based on the propensity score	0–2 km, 2–4 km, 4–6 km buffers	No	Random sample of 25,000 units from each treatment type	2006-2016	30x30m	Elevation Slope Distance to the nearest main road Distance to district centres Distance to the nearest village Distance to the nearest economic land concession Forest cover in 2005
Pfaff *et al.* 2009	Costa Rica	Protected areas	Deforestation	With-versus-similar without	Covariate matching: Propensity score	No	No	Random sample: 4229 points	1986-1997	28x28m	Distance to roads (national and local) Distance to rivers Distance to major cities Distance to forest clearing Rain Elevation Slope
Pfaff *et al.* 2013	Acre, Brazil	Protected areas of different governance and management types	Deforestation	With-versus-similar without	Propensity-score matching Covariate matching (exact matching for key variables and similarity matching for others)	No	Tested for it in the residuals of regression model with a non-significant result	Random sample (~21,000 units per outcome period)	2000-2004 2004-2008	90 x90m	distances to the nearest road distance to nearest city distance to forest edge soil quality rainfall slope
Rayn and Sutherland 2011	Mexico	Protected areas	Rate of forest cover loss	BACI	Unmatched comparison: ratio of means	10 km buffer zone	No	-	1973-2000	1:250,000	Road density Population size

Ren *et al.* 2015	China	National level nature reserves Natural Forest Protection Program	Forest cover change	With-versus-without With-versus-similar without	unmatched comparison vs. covariate matching: Mahalanobis distance with calipers	10 km buffer	No	Random	2000-2010	MODIS pixel size: 231.7 m Landsat 30x30m	Elevation Slope Distance to forest edge
Spracklen *et al*. 2015	Global	Protected areas	Deforestati on	With versus without With versus similar without for a sub-sample	ratio of fractional forest loss between protected and buffer areas	Tested extensive widths of PA buffers	No	-	2000-2012	30 x 30m	Slope Elevation
Vergara-Asjeno and Potvin 2014	Panama	Protected areas and Indigenous territories	Deforestati on	With-versus-similar without	Covariate matching: Mahalanobis distance with caliper	No	No	-	1992-2008 2000-2008	200m x 200m	Elevation Slope Distance to roads Distance to towns
Wang *et al*. 2013	Hainan Island, China	National nature reserves	Forest area change Forest fragmentat ion	With-versus-without	unmatched comparison: General linear model	10 km buffer	No	Random	2000-2010	30x30m	Elevation Slope Distance to Roads Historical forest area
Wendland *et al.* 2015	European Russia	Strict vs. multiple use protected areas	Forest disturbanc e	BACI With-versus-similar without	Matched: One to one propensity score matching without replacement using a caliper followed by fixed effects panel regression	No	Enforced distance between sample points: 300m	Random sample of 1% of all pixels within each PA: 36,000 pixels	1985-2010	30x30m	Distance to forest edge Distance to closest town Distance to Moscow Distance to closest road Elevation Slope

Yang *et al.* 2019	China	Protected areas	Deforestation	With versus similar (nearby) without	Propensity score matching with caliper	No	No	Random sampling based on proportion of forest cover	2000-2015	300m	tree cover, distance to forest edge, elevation, slope, aspect, terrain roughness, topographic wetness index, human influence index, travel time to the nearest city, precipitation, temperature, soil carbon, soil depth, soil acidity, and amount of bulk and clay in the soil.
Zhao *et al.* 2019	Southwest China	Nature reserves: comparison of management levels and establishment age)	Deforestation	With-versus-similar without	Propensity score matching, without replacement and caliper (0.25 SD)	10km buffer zone exclusion	Minimum distance of 300m between units	Random sample within control area defined for each reserve	2001-2012	30x30m	Elevation Topographic position index (TPI) Annual precipitation Distance to the nearest major road Distance to the nearest major settlement

B. Sources of forest assessments for Cambodia

At the national level the the Forestry Administration (FA) under MAFF of the RGC has produced 9 forest cover assessments (FCAs) between 1965 and 2016. Although production of these only began in 1993 with subsequent assessments following approximately every 4 years (1997; 2002; 2006; 2010; 2014 and 2016) (GDANCP 2018). The RGC's FCAs have been criticized for being inconsistent in their definition of forest cover (different % canopy cover thresholds) and their inclusion of rubber and palm plantations as non-distinguished from natural forest (Broadhead and Izquierdo 2010; Brun 2013). Although the RGC have now updated their definition of forest cover to be used in subsequent assessments to match the commonly used definition under the REDD+ scheme (MoE 2016; JICA 2017). Additionally, accuracy assessments were only performed for two of the historic FCAs 2006 and 2010 (GDANCP 2018).

The RGC's FCAs have also been the subject of skepticism by national civil society organizations (Banks *et al*. 2014) under the assumption that the RGC has a vested interest in under-reporting FCL to deflect criticism from international donors. This led the prominent organization ODC to produce its own FCA for 6 different time points with similar temporal coverage as those of the RGC (1973-2014) (ODC 2019a).

Overall, the efforts of both national sources of FCAs have been hampered by data availability, lack of coordination and reticence in data sharing as well the capacity to perform advanced classification techniques (ADB 2001; Aruna Technology Ltd. 2013). By contrast these are typically non-issues for international bodies that produce FCAs. Perhaps the most well-known of these being the University of Maryland's Global Land Analysis & Discovery (GLAD) lab (Hansen *et al.* 2013) who have pioneered some of the most advanced image RS image classification techniques. GLAD provide their data through the interactive Global Forest Watch (GFW) platform which provides annual estimations of forest cover, FCL and forest re-growth at a global scale (GFW 2020a).

Whilst the GLAD/GFW data is arguably the most comprehensive FCA resource available, its accuracy has been critiqued, with Sannier *et al.* (2016) suggesting that its scale makes it unsuitable for national level analyses. Additionally, Grogan *et al.* (2019), who performed their own FCA for Cambodia, found that GLAD data underestimated cover with regards to the particular tropical forest type present, although ultimately this led to only minor deviations in trends as compared to their own data (*p. 48*).

An additional source of FCA for Cambodia that is intermediary in scale between the national scale sources (FA and ODC) and the global (GFW), is the recently released 'forest monitoring system' as part of the SERVIR-Mekong regional land cover monitoring project (SERVIR-Mekong 2020b). This has been developed in collaboration with GLAD and a host of other influential partners to provide geospatial data covering the countries of Cambodia, Lao PDR, Myanmar, Thailand and Vietnam. The intent behind this service is to provide geospatial data on forest dynamics that is more accurate at a regional scale than that of the GFW data, achieved through leveraging the same cutting-edge processing techniques developed by GLAD, combined with validation against field data from national sources (Potapov *et al*. 2019; SERVIR-Mekong 2020a). However, differences in accuracy between SERVIR-Mekong data and other sources have yet to be formally compared.

C. Synthesizing dataset of PA boundaries for assignment to treatment

As alluded to in section 4.1.1.1 the flaw in the WDPA dataset for Cambodia (UNEP-WCMC 2020b) is that it has not yet been updated to reflect the fact that 'protected forests' (formerly a sub category of land under the national forest estate managed by the FA) were converted to 'wildlife sanctuaries' to match the MoE's designations in 2016. Indeed, Nagendra *et al.* (2013) and Bowker *et al.* (2017) noted similar problems with consistency in the WDPA database for African parks.

The national level alternative is ODC's natural protected areas (1993-2019) dataset (ODC 2019b). This dataset includes a number of PAs missing from the WDPA dataset as visible in Figure C1 below. Although by contrast the WDPA dataset did contain one PA not present in the ODC data: "Ta Moa" protected forest.

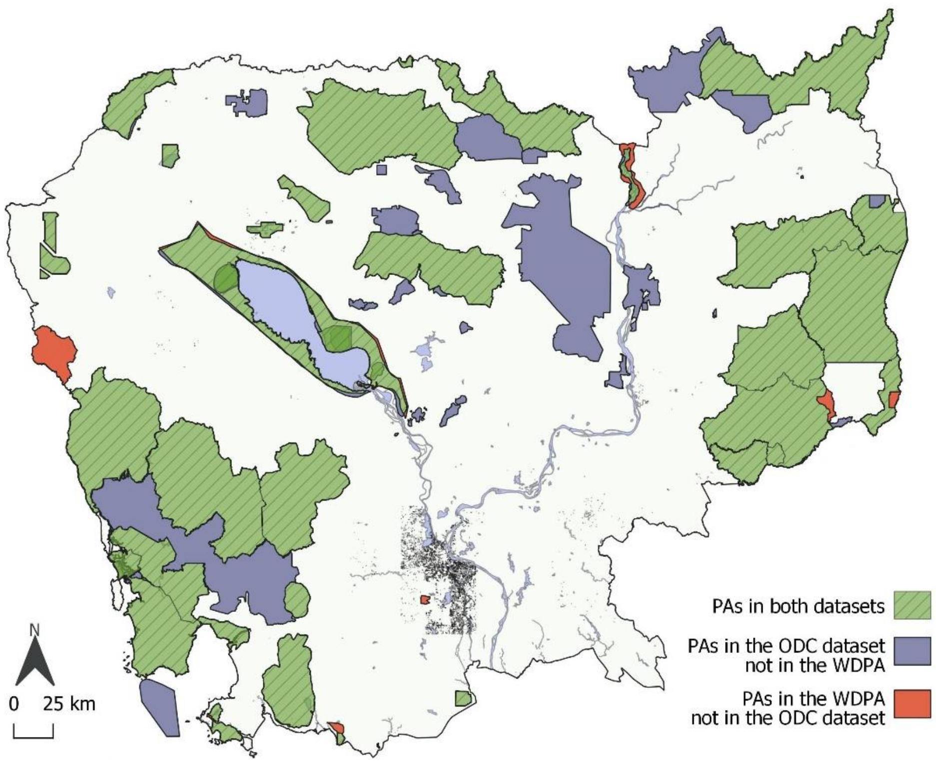

Figure C1: Disparity in the extent of protected areas in Cambodia between the WDPA and ODC datasets (data sources ODC 2019b; UNEP-WCMC 2020b)

However, the ODC dataset is limited by the fact that it does not contain the historic designations of PAs. For example, 'Keo Seima Wildlife Sanctuary' is listed as being established in 2016 although formerly the same area was 'Seima protected forest' and was established in 2004. These changes in PA names primarily affected the former protected forests which were re-designated in 2016 (section 2.2.4).

Additionally, there were several minor discrepancies in the sizes of PAs that were common to both datasets, for example the WDPA data showed larger boundaries for both Kep national Park and the Stung Treng Ramsar site ('or Middle Stretches of the Mekong River north of Stoeng Treng' as it referred to as).

For these reasons it was deemed best to combine records into a single dataset with a column noting any changes in names, designations and establishment dates. In the case of discrepancies in boundary sizes and locations the ODC data was given priority over the WDPA because it provides comprehensive references to the RGC ministerial declarations by which the PAs were established. After data cleaning the total PA dataset included 67 PAs across all categories (most recent designations).

Following this the data was filtered on several accounts, firstly the decision was made to remove the single marine national park, as whilst it contains islands that do display forest cover (SERVIR Mekong 2020b) for the purpose of this analysis these cannot be said to face the same deforestation pressure as mainland forested areas. Another PA category that was considered for exclusion was multiple use management areas. Lacerda *et al.* (2004) defined the management objective of these as: "conservation of biodiversity, sustainable use of resources in natural ecosystems." However, this is problematic as no information is available regarding whether these areas are indeed managed differently to other PAs. Ultimately the decision not to exclude these PAs was made in light of the fact that the relationship between PA effectiveness and the strictness of protection under different management objectives has thus far proved inconclusive (See section 1.4.1). Also cursory examination showed them to contain relatively little forest cover in the period of proposed analysis which suggests that their inclusion was unlikely to skew the results.

A related consideration to this is the fact that Cambodia has begun zoning its PAs in accordance with land use plans (GDANCP 2017). In theory this should have implications for the management activities and ultimately the avoided deforestation occurring within them. However, given that only a small number (3) of the total PAs have been zoned and this process only took place well after their establishment date (MoE 2017) (and hence the zoning would have to be applied in some outcome periods and not others) the decision was made to not to include this in this analysis.

Ramsar sites were assessed for exclusion on an individual basis as many are contained entirely within PAs of other designations and whilst this would not create the possibility of doubling sampling it does add an additional element to the data complexity that does not provide any benefit. On this basis only Ramsar sites for which the majority or entirety of their area lay outside of other designated PAs were included in the final PA dataset.

The decision was also made to selectively exclude several PAs deemed unrepresentative for the purpose of the analysis: Kep national Park (KNP); the aforementioned Ta Moa protected forest (TMPF) and Angkor Wat protected landscape (AWPL). KNP was removed because its land cover has been extensively modified and it is afforded no formal protection by the relevant management authorities (ICEM 2003b). TMPF was removed as it is no longer a protected area as it has become the Phnom Tamao Zoological Park and Wildlife Rescue Center. AWPL was excluded as the forest cover data there is clearly not accurate as it covers the main temple complex. This left a total of 51 PAs in the final dataset (dubbed: ‘filtered PAs’) to be used for the analysis, the names and details of which are included in Table C1 below.

Table C1: Protected areas included in the filtered dataset for analysis

Name	Category	Establishment year	Area (ha)
Phnom Aural Wildlife Sanctuary	Wildlife Sanctuary	1993	253,750
Phnom Prich Wildlife Sanctuary	Wildlife Sanctuary	1993	222,500
Lomphat Wildlife Sanctuary	Wildlife Sanctuary	1993	250,000
Preah Soramrit-Kosomak "Kirirom"	National Park	1993	35,000
Roniem Daun Sam I Wildlife Sanctuary	Wildlife Sanctuary	1993	16,565
Preah Vihear Temple Protected Landscape	Protected Landscape	1993	5,000
Kulen Promtep Wildlife Sanctuary	Wildlife Sanctuary	1993	402,500
Preah Monivong National Park	National Park	1993	140,000
Phnom Kulen National Park	National Park	1993	37,500
Beng Per Wildlife Sanctuary	Wildlife Sanctuary	1993	242,500
Phnom Namlear Wildlife Sanctuary	Wildlife Sanctuary	1993	47,500
Dong Peng Multiple Use Area	Multiple Use Management Area	1993	27,700
Roniem Daun Sam II Wildlife Sanctuary	Wildlife Sanctuary	1993	21,335
Protected Landscape Banteay Chmar	Protected Landscape	1993	81,200
Tonle Sap Biosphere Multiple Use Area	Multiple Use Management Area	1993	316,250
Cultural Resort of Banteay Chmar Temple	Protected Landscape	1993	780
Virachey National Park	National Park	1993	332,500
Snuol Wildlife Sanctuary	Wildlife Sanctuary	1993	75,000
Botum Sakor National Park	National Park	1993	171,250
Roniem Daun Sam III Wildlife Sanctuary	Wildlife Sanctuary	1993	2,121
Peam Krasop Wildlife Sanctuary	Wildlife Sanctuary	1993	23,750
Prasat Bakan Protected Landscape	Protected Landscape	1993	2,124
Phnom Yart Natural Heritage Site	Natural Heritage Site	1993	31.951
Samlaut Multiple Use Area	Multiple Use Management Area	1993	60,000
Phnom Samkos Wildlife Sanctuary	Wildlife Sanctuary	1994	333,750
Ream National Park	National Park	1995	150,000
Prek Teuk Sap Kbal Chhay Multiple Use Area	Multiple Use Management Area	1997	5,520
Chheb Wildlife Sanctuary	Wildlife Sanctuary	1999	190,027
Srepok Wildlife Sanctuary	Wildlife Sanctuary	1999	372,971
Central Cardamom Mountains National Park	National Park	1999	401,313
Ang Trapeang Thmor Protected Landscape	Protected Landscape	1999	12,650
Stung Treng Ramsar Site	Ramsar Site	1999	14,600
Koh Kapik and Associated Islets	Ramsar Site	1999	12,000
Sambor Prey Kok Temple Cultural Resort	Protected Landscape	2003	2,982
Cultural Resort of Banteay Top Temple	Protected Landscape	2003	108
Koh Kae Protected Resort	Protected Landscape	2004	3,508
Beng Mealea Protected Area	Protected Landscape	2004	315
Keo Seima Wildlife Sanctuary	Wildlife Sanctuary	2004	292,690
Boeung Prek Lpeou Protected Landscape	Protected Landscape	2007	8,305
Ou Ya Dav National Park	National Park	2009	101,348

Techo Sen Russey Treb Cambodian Royal Academy National Park	National Park	2014	11,435
Siem Pang Wildlife Sanctuary	Wildlife Sanctuary	2014	133,708
Prey Lang Wildlife Sanctuary	Wildlife Sanctuary	2016	431,683
Veun Sai-Siem Pang National Park	National Park	2016	57,469
Preah Rokar Wildlife Sanctuary	Wildlife Sanctuary	2016	90,361
Anloung Pring Protected Landscape	Protected Landscape	2016	217
Ponhea Kraek Multiple Use Area	Multiple Use Management Area	2016	199
Southern Cardamom Mountains National Park	National Park	2016	410,392
Tonle Sap Northern Lowland Protected Landscape	Protected Landscape	2016	31,159
Tatai Wildlife Sanctuary	Wildlife Sanctuary	2016	144,275
Phnom Tbeng Natural Heritage Site	Natural Heritage Site	2016	25,269.41

D. Defining unprotected land for assignment to control

In terms of identifying the ‘unprotected’ control region, Gaveau *et al.* (2012) and Schleicher *et al.* (2017) highlight the importance of both considering the existence of other types of land governance regimes, as well as any legal frameworks, whose purposes are to either prevent or allow natural resource extraction or land conversion. In the case of Cambodia examples of land governance modalities intended to prevent change from natural land cover are CFs and CPAs. However, including these in the proposed analysis by excluding them from the unprotected control region was not deemed appropriate for several reasons. Firstly, the data available on the boundaries and degree of management of CFs and CPAs is inconsistent meaning that any quantification of their impact could hardly be said to be robust. Secondly whilst their stated purpose is to prevent large scale land conversion there is provisions under the laws governing their establishment to allow for sustainable use which in the case of CFs would mean timber extraction and thus the question would be how to determine whether any FCL observed in CFs or CPAs can be considered sustainable or not especially as very little evidence exists to quantify this.

Conversely in terms of land governance modalities in Cambodia that allow for natural resource extraction or land cover alteration, the principal examples are ELCs; SLCs and mining concessions (introduced in section 2.1). Given the likely importance of their impact ELCs are already accounted for in the analysis as a covariate and as such it would not be possible for them to be excluded from the unprotected control region. SLCs were not factored in on the basis that whilst they represent land specifically allocated to for the resettlement households there is no clear directive for the occupants to alter land cover and hence they must be considered as equivalent to any other land under private title. Mining concessions on the other hand are more contentious as, like ELCs, there is some evidence linking them to the stripping of forest cover by operators (Work *et al.* 2018). However, unlike the ELCs, Cambodia's mining concessions are less well documented and detailed information on their boundaries and states of operation is not available and thus on this ground they were not included.

Collectively these considerations led to the decision described in section 4.1.1.2 that the control region by defined as all land use modalities aside from the nationally recognized PAs included in the filtered PA dataset (see Appendix C).

E. Further details on the selection of covariates and confounders

In terms of additional covariates that were considered for the analysis another that was investigated was the spatial distribution of species of high-value hard wood timber. This on the basis that there is evidence suggesting that considerable selective logging of certain species in Cambodia has taken place (Global Witness 2015) and hence it should be expected that the distribution of these species correlates with both the occurrence of deforestation as well the location of PAs (as they are located in rare habitat types such as DDF). However, whilst maps of these species' distributions were produced by the Cambodian Tree Seed Project (CTSP 2004) they rely upon very scant sampling data and digitized versions are not readily available, hence this covariate was discarded.

It has been widely noted that the suitability/desirability of land for conversion to agricultural is one of the driving forces behind deforestation in tropical forests (Jones and Lewis 2015). Although this is particularly hard to capture as a covariate as it depends on a multitude of factors, which has often led others to utilise composite variables i.e weighted calculations of several interdependent variables such as soil fertility; salinity and slope etc. (Carranza *et al.* 2013). However, this is problematic as little attention has been made to quantifying the effect of any multicollinearity, or heterogeneity of explanatory power between the variables of which the composites consist, on the results. For this reason, composite variables were deliberately avoided for this study, instead covariates aimed at capturing the same phenomenon were included individually. In this regard one other covariate that was investigated but subsequently discarded was data on commune level agricultural yields from national socio-economic censuses available from the Cambodian

National Institute of Statistics (NIS MOP 2020). Combined with information on the percentage land area under cultivation this would give a good insight into productivity of the land. Unfortunately, the data was not consistently available for the correct time periods and the number of households surveyed differed between censuses further limiting the robustness of this as a data source.

F. Synthesizing data for ELCs

Two principle data sources are available for the boundaries and extents of ELCS in Cambodia: One from ODC (2017a) and one from LICADHO (2020). Both of which acknowledge that their datasets contain inaccuracies and missing information due to the fact that the RGC's data of ELCs is itself inconsistent and incomplete. On this basis the decision was made to filter, combine and cross-check both of these datasets in order to synthesize a new dataset of ELC records suitable for this analysis.

The first level of filtering that was performed was to remove any records that did not contain a declaration date or start of contract date for the ELC thereby making them unsuitable for the temporal nature of the analysis. In this regard the LICADHO dataset originally contained 281 records which was reduced to 245 and the ODC (2017a) dataset contained 285 records which was reduced to 264 after removal.

Amongst these records in the ODC dataset there were a number that were represented by circles as polygons features the attributes of which described then as 'government data partial' indicating that their correct boundaries were not included in the dataset. However, when these ELCs were overlaid with the LICADHO dataset it was clear that they did contain the boundaries for some of these unknown examples (matching performed on the basis of concession names) and removing those records left a total of 217 ELCs. Further to this though, the LICADHO dataset contained 41 ELCs that did not show consistent spatial overlap with the equivalent ELCs from the ODC dataset although many are attributed to the same developers and hence can be construed as likely the same concessions but with different boundary data. Vice versa there were 17 ELCs in the ODC dataset that did not substantially overlap with those in the LICADHO dataset. In these cases, preferences were given to the ELC boundaries contained in the ODC dataset on the grounds that it includes citations to supporting documentation for each entry whereas the LICADHO dataset does not.

Beyond these removals of records on the grounds missing temporal and spatial information the decision was made to include all of the ELCs from both the LICADHO and ODC datasets regardless of whether they are listed as downsized or revoked as a result of Directive 01 (section 2.1.2.3). The justification for this being that there is limited information as to when these changes were affected and also whether or not they were implemented successfully. This resulted in a final dataset containing 266 ELCs in total which was then temporally partitioned into three separate layers based upon the ELC establishment dates to be intersected with the different outcome periods.

G. Testing durations outcome of outcome periods for analysis

Given that annual (1 year) outcome periods were deemed unfeasible for this analysis the decision was made to test outcome periods of both 2 and 3 year durations between 2010 and 2018. This consisted of calculating how much PA and ELC land that was established during this period would effectively be excluded from the analysis by virtue of the fact that it was declared within the outcome periods (only land established prior to and within the first year can be included). The results of this are demonstrated in Figure G1 below from which it was concluded that three-year periods were preferable because the 2-year period of 2015-2016 excludes the new PAs established in 2016 and hence has no concrete policy/event underpinning that makes it worth investigating. Whereas comparatively this increase in the PA estate is better captured by the 3-year 2016-2018 period. Although the use of 3-year periods would result in the omission of a relatively large amount of ELC land in the first outcome period of 2010-2012, this is justifiable as this land is included within the following period and it is unlikely that the effects the ELCS established in 2011 and 2012 will be observed immediately and hence the period of 2013-2015 best captures the period of greatest deforestation pressure due to ELCs. A final benefit of the 3-year periods is that it results in only having 3 outcomes periods to analyze versus 4 under the 2-year approach, which may not sound significant but given the amount of data processing required (which will be detailed in the following sections) is advantageous. Thus from this point on the outcome periods for this analysis will be: 2010-2012; 2013-2015; and 2016-2018.

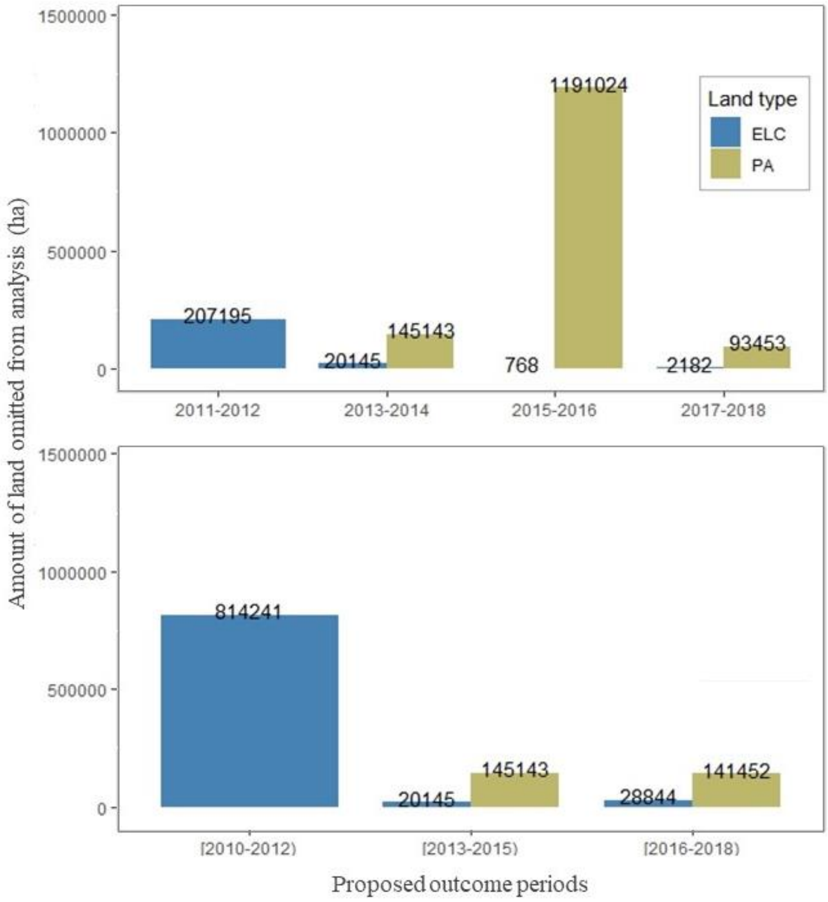

Figure G1: PA and ELC land omitted from analysis under different outcome periods

H. Process of creating relational datasets

Chronologically the first stage to be undertaken was the creation of initial datasets required for the preliminary analysis, with the intent being that if the results of the preliminary analysis were favorable these initial datasets could be used as final datasets for both the primary and secondary analysis without modification. Or if changes to the covariate selection were required on the basis of these results then initial datasets could easily be modified. This required separate datasets to be created for each of the outcome periods defined in the previous section (2010-2012; 2013-2015; 2016-2018).

As previously described the conditions determining assignment to treatment for these datasets was whether a given unit was located within the boundaries of a PA that had been established during or prior to the first year of the outcome period but not within the boundaries of any ELC that had been declared within that PA by that same year. This meant temporally dividing up the dataset of PAs minus ELCS (section 4.1.1.1) into layers representing the total PA extent in the years of 2010, 2013 and 2016. Then the treated unit population was identified within these as being those pixels (30 x 30m) resolution that were shown to be forested within in the first year of the outcome period according to the appropriate layers of the SERVIR Mekong forest cover data (see section 4.1.1). As for identifying the control unit population these were all units that were forested in the first year of the outcome period located within the wider un-protected landscape as defined in section 4.1.1.2.

Both the treated and control populations for each of the outcome periods (datasets) were all of an order of magnitude between $3x10^6$-$5.7x10^6$ units which would make a matching analysis using the total populations computationally unfeasible. Hence in line with other studies (section 1.3.4.1) it was elected to take a random sample of 10% of both treated and control units for each outcome period to use in the subsequent analysis.

The next stage of creating the initial datasets for each outcome period was to intersect the samples of the treated and control units with the layers representing the outcome variable and covariates/confounders. This was achieved using a combination of the QGIS 'join attributes by location' tool for the vector layers and the SAGA GIS 'Add raster values to points' tool using the bilinear interpolation method. The outcome variable being the data of FCL events occurring within each outcome period (section 4.1.2) (FCL outcome) resulting in each unit being ascribed a dichotomous value of forested (0) or deforested (1). Whereas the covariates and confounders were incorporated using all of the separate data layers detailed in section 4.1.3 (Table 2) and had discrete/continuous values. The final task was to remove any units from the sample datasets that displayed 'null' values for any of the covariates, with a table of the final number of treated and control units in the samples for each outcome period included in section 5.1.1.

As alluded to above the testing for spillover effects of PA establishment as part of the preliminary analysis (described in section 4.3.6) necessitated the creation of three additional datasets (one for each outcome period).

The process of creating these datasets was essentially very similar with the key difference being that assignment to treatment was not based on whether or not a unit was inside a PA but instead whether it was located inside a 5km buffer zone of a PA that was established during or prior to the first year of the outcome period. The buffers were created from the layers of temporally partitioned PA boundaries that were generated as part creating the initial datasets. However, this meant that because some PAs shared contiguous boundaries to ensure that no buffer area overlapped with that of other PAs it was necessary to edit the buffer polygons manually. Manual edits were also made to ensure that external buffers were not present in the locations where the boundaries of the PA intersected with that of ELCs, either when the ELCs were external to the PAs or contained within them.

Once the buffer areas had been established the treatment population was identified as all of the units that were forested inside them within the first year of the outcome period. The control population for these datasets was identified in the same manner as for the initial datasets with the obvious exception being that units were only identified within the unprotected landscape external to the buffers and not within the PA land internal to them. The same random sampling strategy was employed to generate samples of these treated and control populations. Again, the last step for these datasets was to intersect the treated and control units with the other data fields for the outcome variable and the covariates in the same manner as the primary datasets above and units displaying null values removed to make them ready for analysis.

I. Refinement of covariates

I.1 Testing covariates with GLMs

A GLM was an appropriate model choice in this circumstance as the primary dependent variable being investigated for the purpose of covariate refinement is the binary outcome of whether a unit remains forested or has been deforested at the end of the outcome period. Thus, given that deforestation is rarely uniformly distributed across landscapes (and indeed it shows patterns in Cambodia: section 2.13-2.14) it is reasonable to hypothesise that this variable and the residuals associated with it, are likely to be non-normally distributed and hence simple linear regression is inappropriate (Dytham 2011) However, for accuracy this assumption was confirmed by creating a linear model and evaluating the normal-QQ plot, which visibly confirmed non-normal distribution of residuals (Figure I1 below).

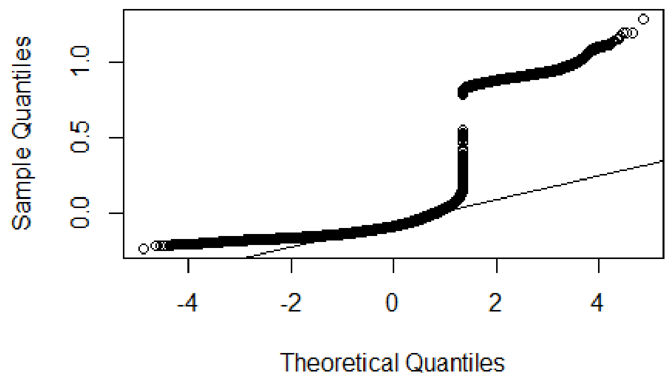

Figure I1: Normal-QQ plot for the linear model of the 2010-2012 outcome period data

There are numerous types of GLMs representing logistic regression under different error distributions (Zuur *et al.* 2007), in the case of this study a GLM using binomial distribution is appropriate given that we have a categorical dependent variable and for this distribution the default transformation is a logit link function (*p. 120*).

The first step was to create an initial GLM with the dependent variable being the dichotomous outcome of forested (0) or unforested (1), and including all 11 covariates as main effects (i.e. without testing for interaction between them). This model was created using the R stats package's 'glm' function (R core team 2020) with separate models created for each outcome period. The reason for this was that there is a possibility that control and treated samples of different outcomes periods contain the same units of analysis (given the conditions for assignment detailed in section 4.1.1). Thus, appending the data for all of the outcome periods together would likely have violated the GLM assumption of independence (Zuur *et al.* 2007).

The rationale behind starting with initial GLMs including the full selection of covariates was to follow the advice of Andam *et al.* (2008) and Stuart (2010) who highlighted the importance of including all possible variables that have a significant predictive effect. However, this suggestion precludes the fact that just because all variables have a significant effect does not mean that the resulting model is the best fit for the observed data as some may be redundant. Hence there is a clear incentive to at least trial different combinations of covariates (predictors) to see if model fit (explanatory power) can be improved.

This can be achieved through the use of stepwise model selection in which variables are iteratively added (forward selection) or removed (backward) from the model and the results compared using a standardised value. In this study given that a full model of all variables had already been produced, backwards selection was utilised with the Akaike

Information Criterion (AIC) (a value of model fit that penalises the addition of unnecessary predictors: Zuur *et al*. (2007)) used as a means of comparison.

Table I1 below shows the results of the initial GLMs (all covariates) for each outcome period. Given that the model uses a logit link function, for ease of interpretation the influence of the various covariates are expressed as odds ratios (the exponential of the coefficients estimates). In light of this and the model specifications variance is appropriately expressed through low/high confidence intervals (CI) rather than standard error.

Table I1: Results of the GLMs including all covariates produced for each outcome period

Predictors	Dependent variable: Forest cover outcome		
	Odds ratio$^{(signif.)}$ (2.5% CI; 97.5% CI)		
	2010-2012	2013-2015	2016-2018
Surrounding FCL	0.999*** (0.999; 0.999)	0.999*** (0.999, 0.999)	0.999*** (0.999, 0.999)
Distance to ELC	1.000*** (1.000; 1.000)	1.000 (1.000, 1.000)	1.000(1.000, 1.000)
Surrounding population	1.000*** (1.000; 1.000)	1.000*** (1.000, 1.000)	1.000*** (1.000, 1.000)
Slope	0.992*** (0.991; 0.993)	0.999*** (0.998, 1.000)	0.998*** (0.998, 0.999)
Average annual precipitation	1.000*** (1.000; 1.000)	1.000 (1.000, 1.000)	1.000*** (1.000, 1.000)
Average annual temperature	0.981*** (0.977; 0.985)	1.023*** (1.020, 1.027)	0.982*** (0.978, 0.985)
Elevation	0.997*** (0.997; 0.998)	1.000*** (1.000, 1.001)	0.998*** (0.998, 0.999)
Distance to provincial capital	1.000*** (1.000; 1.000)	1.000*** (1.000, 1.000)	1.000*** (1.000, 1.000)
Distance to land border	1.000*** (1.000; 1.000)	1.000*** (1.000, 1.000)	1.000(1.000, 1.000)
Distance to major roads	1.000*** (1.000; 1.000)	1.000*** (1.000, 1.000)	1.000*** (1.000, 1.000)
Soil type	1.027*** (1.026; 1.029)	1.012*** (1.011, 1.014)	1.017*** (1.015, 1.018)
Constant	321.813*** (112.046; 924.300)	0.001 (0.0002, 0.001)	255.900*** (101.469, 645.369)

Observations	927,603	841,903	787,322
Null deviance	547860	486853	557018
Residual deviance	450378	398406	484941
Akaike Inf. Crit.	450,401.700	398,429.900	484,965.400
Note: **p<0.05***p<0.01			

Table I1 shows that the reductions in deviance resulting from the model including all covariates as predictors as compared to the null deviance from the intercept alone for all outcome periods, indicate that the models are a good fit for the observed data. This is further confirmed by the fact that the majority of predictors are relatively consistent in their significance across all 3 outcome periods. However, it is slightly concerning that 'distance to ELCs' is non-significant for both the 2013-2015 and 2016-2018 outcome periods given that it is highly significant for the period of 2010-2012. A possible explanation for this is that the dataset for the first outcome period contained relatively few ELCs compared to the subsequent two (Appendix G). This is exacerbated by the method by which this variable is calculated, namely through the use of a Euclidean distance matrix which means that all forest cover loss events were likely in closer proximity to ELCs (making it a non-significant predictor). The differences in significance for certain predictors between the outcome periods is also reflected in the results of the stepwise model selection as shown in Table I2 below. For the first outcome period Table I2 shows that the GLM including all covariates produced the lowest AIC score indicating that it represents the best fit as compared to when any covariates are systematically removed. However, for the period 2013-2015 the results show that the removal of either the average annual precipitation or distance to ELCs predictors resulted in a lower model AIC score. This is also the case for the distance to ELCs and distance to land borders predictors in the 2016-2018 period. Ordinarily it is custom to retain the model that results in the lowest AIC score however as the removal of any of these predictors in either of the latter two outcome periods only results in a difference in AIC score of <=2 and the removal of distance to ELCs for the first outcome period would increase in AIC score by 397. then it seems more pertinent to overlook this and maintain consistency between the outcome periods by using the same model including all covariates for each.

Table I2: Results of stepwise model selection from initial GLMs

Predictor removed	(2010-2012)		(2013-2015)		(2016-2018)	
	Deviance	AIC	Deviance	AIC	Deviance	AIC
None	450378	450402	398406	398430	484941	484965
Average annual precipitation	451882	451904	398407	**398429**	486610	486632
Distance to ELC	450777	450799	398407	**398429**	484943	**484965**
Elevation	450850	450872	398412	398434	485165	485187
Slope	450576	450598	398413	398435	484953	484975
Average annual temperature	450475	450497	398562	398584	485064	485086
Surrounding population density	450469	450491	398626	398648	485978	486000
Distance to provincial capital	451874	451896	398687	398709	489628	489650
Distance to major roads	452320	452342	398703	398725	486065	486087
Soil type	452074	452096	398718	398740	485639	485661
Distance to land borders	450548	450570	398946	398968	484941	**484963**
Surrounding FCL	513315	513337	465244	465266	523106	523128

Note: AIC results displayed in **Bold** highlight models that reduced AIC compared to the full model

I.2 Testing for multicollinearity

As previously alluded too (section 1.3.4.2), in confirming the covariates to be used for matching methods analysis there is a need to check for the presence of any multi-collinearity between them that may introduce bias. This can be identified by calculating the variance inflation factors (VIF) for each covariate, which in the case of this analysis was performed using the ‘vif’ function as part of the R package ‘car’ (Fox *et al.* 2020). This process was repeated for all outcome periods however given that the results were analogous for efficiency they will be reported for one outcome period only, as such Table I3 below shows the results for 2010-2012. Generally, VIF values >5 are considered to a conservative ‘cut-off’ point with regards to the presence of collinearity of variables (with values ~1 indicative of no collinearity) (Zuur *et al.* 2009). Thus, Table I3 shows that only two variables are likely to collinear: average annual temperature and elevation.

Table I3: Variance inflation factors for predictors in the GLM of the 2010-2012 period

Predictor	Variance Inflation Factor (VIF)
Surrounding FCL	1.022988
Distance to ELC	1.798822
Surrounding population density	1.184162
Slope	1.28565
Average annual precipitation	1.699616
Average annual temperature	**13.342755**
Elevation	**11.834353**
Distance to provincial capital	1.176646
Distance to land border	1.570873
Distance to major roads	1.177708
Soil type	1.081652

This implication of this is that one of these should be removed from the final selection of covariates with the decision made on the basis of which removal results in the smallest increase in model AIC score. Referring back to Table I2 it is evident that the removal of average annual temperature as a predictor results in the smallest increase in AIC across the three outcome periods and hence it should be removed from the covariate selection for the matching analysis. This was further justified on the basis that recalculation of the VIF following the removal resulted in all predictors having a VIF value <1.75.

I.3 Confirming overlap in covariate distributions

As highlighted in section 4.3.1 the second consideration when finalising the selection of covariates prior to matching is that there is sufficient overlap between their distributions with regards to the treated and control samples (to meet the assumption of SITA: section 1.3.4). To check this a range of summary statistics for the samples from each outcome period were calculated using the bal.tab function within the Cobalt package in R (Greifer 2020a). Typically, the standardized mean difference (SDM) between the samples for each covariate is used to as an indicator of dis/similarity between samples. However, Greifer (2020b) highlights that it is also important to consult other statistical measures that give a better picture of covariate distribution beyond measures of central tendency. In this regard the variance ratio (Austin 2009a), Kolmogorov-Smirnov (KS) statistic (Greifer 2020b) and the complement of the overlapping coefficient (COC: Franklin *et al.* 2014) were used to provide summary values of covariate overlap (or sample balance) in easily interpretable terms.

Table I4 below shows these summary statistics in addition to the means and standard deviations (SD) for each covariate within each outcome period. The most important result to take away from this table is that the treated and control samples do indeed show sufficient overlap between their covariate distributions across all of the outcome periods. This evidenced primarily by the values of the KS statistic and the COC, whereby in both measures a value of 0 indicates a perfectly overlapping distributions and 1 indicates perfectly non-overlapping distributions (Greifer 2020b). Thus, in this case there appears to be substantial overlap for all covariates.

Table I4: Covariate summary statistics for all outcome periods

Covariate	Outcome period	Control		Treated		SDM	Variance ratio	KS statistic	COC
		Mean	SD	Mean	SD				
Surrounding FCL	2010-2012	2238.710	2363.923	3047.904	2781.443	0.314	1.384	0.147	0.146
	2013-2015	2074.022	2317.133	3081.585	2818.749	0.391	1.480	0.185	0.183
	2016-2018	1694.996	2555.326	2581.290	2228.686	0.370	0.761	0.228	0.227
Distance to ELC	2010-2012	47537.423	33315.271	36732.979	34366.149	-0.319	1.064	0.233	0.264
	2013-2015	32827.661	26206.881	20261.185	21610.574	-0.523	0.680	0.286	0.333
	2016-2018	22195.524	21026.606	22551.149	17828.224	0.018	0.719	0.168	0.141
Surrounding population density	2010-2012	439.208	1142.967	132.090	377.547	-0.361	0.109	0.292	-
	2013-2015	376.196	976.493	126.502	361.779	-0.339	0.137	0.268	-
	2016-2018	503.288	1089.343	113.095	301.055	-0.488	0.076	0.403	0.217
Average annual precipitation	2010-2012	2031.183	662.432	2334.821	765.963	0.424	1.337	0.285	0.280
	2013-2015	2070.140	687.392	2379.492	774.321	0.423	1.269	0.278	0.274
	2016-2018	1953.546	505.211	2434.737	841.535	0.693	2.775	0.309	0.307
Slope	2010-2012	8.771	9.117	14.012	14.061	0.442	2.379	0.190	0.189
	2013-2015	9.231	9.673	14.818	14.424	0.455	2.224	0.203	0.200
	2016-2018	8.719	9.323	14.275	13.689	0.474	2.156	0.224	0.223
Elevation	2010-2012	140.097	126.142	316.096	279.815	0.811	4.921	0.367	0.356
	2013-2015	150.763	145.072	333.458	283.308	0.812	3.814	0.382	0.372
	2016-2018	140.135	143.903	303.467	263.216	0.770	3.346	0.390	0.389
Distance to provincial capital	2010-2012	42651.145	18307.442	52139.976	17470.097	0.530	0.911	0.241	0.239
	2013-2015	43818.881	18713.115	51696.173	17618.395	0.433	0.886	0.204	0.203
	2016-2018	42554.544	18789.669	50668.024	18272.894	0.438	0.946	0.184	0.183
Distance to land border	2010-2012	64735.015	38722.663	52892.368	41973.695	-0.293	1.175	0.169	0.186
	2013-2015	63751.652	38685.042	52739.307	41982.770	-0.273	1.178	0.168	0.183
	2016-2018	59990.226	39256.338	58905.856	41269.323	-0.027	1.105	0.044	0.091
Distance to major roads	2010-2012	12580.703	10141.931	16146.330	11896.479	0.323	1.376	0.140	0.137
	2013-2015	13318.155	10641.368	16359.564	11784.912	0.271	1.226	0.124	0.123
	2016-2018	11813.565	10229.678	17067.137	11516.210	0.482	1.267	0.230	0.229
Soil type	2010-2012	7.843	6.269	6.316	6.320	-0.243	1.016	0.170	0.193
	2013-2015	7.480	6.256	5.965	6.202	-0.243	0.983	0.169	0.193
	2016-2018	7.922	6.136	5.750	6.193	-0.352	1.019	0.232	0.218

J. Spatial autocorrelation analysis

Section 4.3.3 highlighted that SAC can affect the results of regression models used for covariate refinement, this occurs because by its nature spatially auto-correlated data violates the assumptions underlying regression. Primarily the requirement for independence and homoskedasticity of variance in the model residuals (Bolker 2007: Dormann *et al.* 2007). The practical implication of this is that regression models under SAC tend to underestimate the standard errors of the coefficients of the predictor variables, thereby increasing the probability of a type I error occurring by not rejecting predictors that are in fact non-significant (Negret *et al.* 2020).

This problem is clearly applicable for the GLMs used for covariate refinement in this study (Appendix I) and thus it was important to confirm the presence of SAC to affirm the validity of the covariate selection generated by them. A simple initial means of investigating this is to visualize the residuals of the GLM in a spatial context, i.e. by plotting them against the X and Y coordinates of the data points (units of analysis). Given the assumption that the residuals are independently distributed, if this visualization shows clear clustering of similar residual values then SAC is likely present. Figure J1 below shows such a visualization for the GLM residuals of the 2010-2012 outcome period.

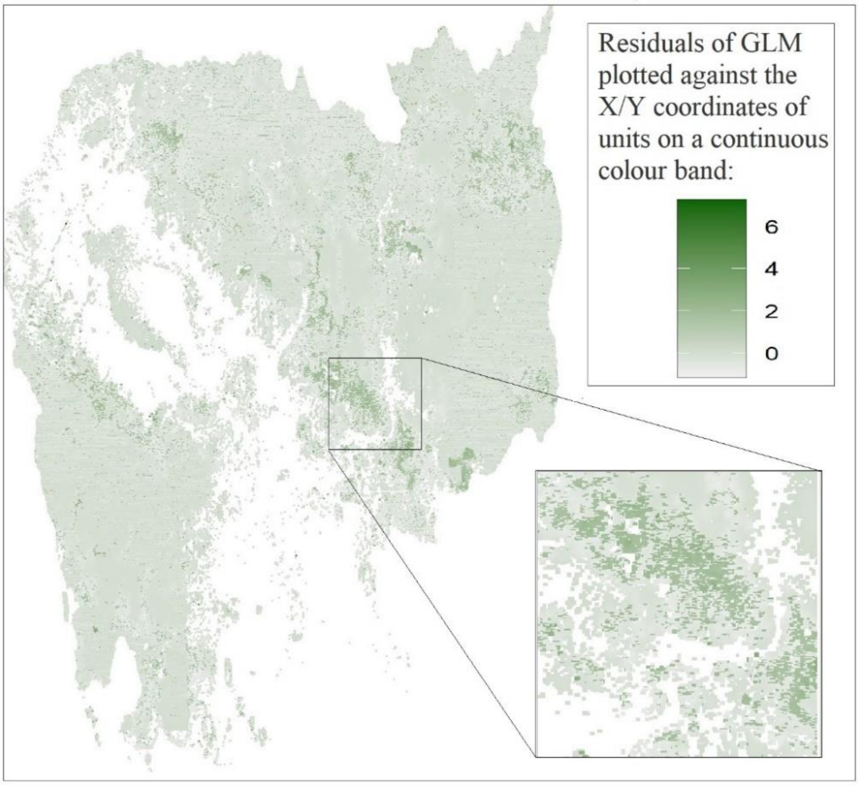

Figure J1: Spatial visualisation of the GLM residuals from the 2010-2012 outcome period

From an observational perspective Figure J1 does appear to show some clustering of higher value residuals in the data meaning that SAC is likely influencing the results of the GLM. However, as the substantial number of units in the dataset (<950,000) confounds this observation, it was deemed pertinent to confirm the presence of SAC using a statistical measure, namely the Moran's I test.

Whilst it would be possible to test for SAC with respect to each predictor (covariate) separately to ascertain which of them contribute the most to the cumulative effect of SAC. In the case of this study this would have been superfluous as there are a quite a number of predictors that would logically be expected to be spatially autocorrelated. Hence for expediency, the Moran's I test was performed on the residuals of the GLM containing the 10 refined covariates from Appendix I. However, given the large number of units in the combined (treated and control units) datasets it was necessary to test a sample from one outcome period in order to make the processing possible. This was performed on a 10% random sample of the 2010-2012 outcome period data (consisting of 92760 units) using the 'moran.test' function as part of the Spdep package in R (Bivand *et al.* 2019). With results of the Moran's I test confirming the presence of SAC (Moran I statistic standard deviate = 86.901, p-value < 2.2e-16).

With the presence of SAC confirmed within the data it was then necessary to return to the GLMs used for covariate refinement (Appendix I) to test whether the inclusion of SAC in the model structure affected the results in terms of the significance of the covariates and the AIC scores. There are two means of achieving this, a more simplistic approach as performed by Schleicher *et al.* (2017) is to include the X and Y coordinates as well as the interaction between them (X*Y) as separate predictors in the GLM alongside the other variables. However, the robustness of this approach is questionable, by contrast Dormann *et al.* (2007) describe a more comprehensive option whereby the effect of SAC is included the model as an autocovariate. In practical terms this represents the extent of SAC through a distance weighted neighbourhood index that quantifies how much the value of the response (dependent) variable for each unit reflects the values of those in proximity to it, essentially resulting in a spatial GLM.

The latter method was trialled first, however the creation of the neighbourhood index for a sample of the size of the 2010-2012 data (>950,000 units) was too computationally intensive (the script failed to complete after 18+ hours using parallel processing). On this basis the decision was made to instead perform the process for a spatial sample (control and treat units sampled within a polygonal region) from the 2010-2012 dataset. This was achieved using the ‘autocov_dist’ function from the R package Spdep (Bivand *et al.* 2019) using the default ‘style = B’ (symmetrical neighbourhood matrix) and trialling an increasing neighbourhood radius (0.1 increments) until all units had neighbours associated (3.8 km).

Given that it was only possible to create an autocovariate adjusted GLM for a sample of the data, the former method of Schleicher *et al.* (2017) was also implemented for the full 2010-2012 dataset. Table J1 below shows the results of the GLMs produced by the two processes

Table J1: Results of the two spatially adjusted GLMs

Predictors	*Dependent variable:* Forest cover outcome	
	Odds ratio$^{(signif.)}$ (2.5% CI; 97.5% CI)	
	GLM with X/Y terms	GLM with autocovariate
Surrounding FCL	0.999*** (0.999, 0.999)	0.999*** (0.999, 0.999)
Distance to ELCs	1.000 (1.000, 1.000)	1.000*** (1.000, 1.000)
Surrounding population	1.000*** (1.000, 1.000)	1.000*** (1.000, 1.000)
Slope	0.992*** (0.991, 0.993)	0.987*** (0.980, 0.994)
Average annual precipitation	1.000*** (1.000, 1.000)	0.998*** (0.997, 0.998)
Elevation	0.998*** (0.998, 0.999)	1.003*** (1.001, 1.004)
Distance to provincial capital	1.000*** (1.000, 1.000)	1.000 (1.000, 1.000)
Distance to land border	1.000*** (1.000, 1.000)	1.000*** (1.000, 1.000)
Distance to major roads	1.000*** (1.000, 1.000)	1.000*** (1.000, 1.000)
Soil type	1.026*** (1.024, 1.027)	0.980*** (0.974, 0.985)
X coordinate	1.000*** (1.000, 1.000)	
Y coordinate	1.000 (1.000, 1.000)	
X*Y interaction	1.000*** (1.000, 1.000)	
Autocovariate		1.042*** (1.041, 1.043)
Constant	1.199*** (0.673, 2.138)	2.514*** (1.336, 4.730)
Observations	927,603	69,598
Log Likelihood	-223,604.800	-19,177.840
Akaike Inf. Crit.	447,237.700	38,379.680
Note: *p **p<0.05 ***p<0.01		

Table J1 shows that the inclusion of predictors that make the spatial context of the units explicit results in models with lower AIC scores (better model fit) than the standard model presented in Table I1 (Appendix I). Of course, given the significance of SAC as confirmed by the Morans I result this is hardly unexpected. Of more importance is the fact the models resulted in a change in the significance of 1 covariate although these were different in each case. In the simple model including the X/Y coordinates distance to ELCs was no longer significant whereas in the autocovariate model distance to provincial capitals was not a significant predictor.

These observations are tenuous grounds to discount these covariates from the matching analysis. However given the limited appraisal for the robustness X/Y coordinate inclusive model as well as the fact that a Morans I test of the autocovariate GLM still found significant SAC (Moran I statistic standard deviate = 107.07, p-value < 2.2e-16), it was decided that the best course of action was to continue using the set of 10 refined covariates. Although in light of this the matched data produced by the primary and secondary analysis should be re-tested to see if SAC is still present.

K. Results of matching methods trials.

Before presenting the results generated by the two different matching approaches it is important to define the aspects upon which they are to be compared. Given that the primary purpose of matching is to reduce the variance in the covariate distribution between the treated and control groups (i.e. create more covariate balance) then metrics that quantify this should be the primary concern (Section 1.3.2). However, the number of treated and control units, as a proportion of the total sample, that a given technique is able to match must also be considered to ensure that ATT estimates are robust (Section 1.3.4.3). Also, in the case of this study, given the constraint of computational resources, the time taken for the matching process to complete (as an operation in RStudio) was a factor.

Firstly, in terms of assessing the improvements to covariate balance, following the discussion in section 1.3.4.3 the decision was made to compare the values of absolute standardized mean difference (ASMD) and the KS statistics between the unmatched and matched samples without formal hypothesis testing for differences. Figure K1 shows the results of this for the 10% random samples from the 2010-2012 outcome period (treatment: located within PAs) under both the PSM and MDM approaches in the form of love plots.

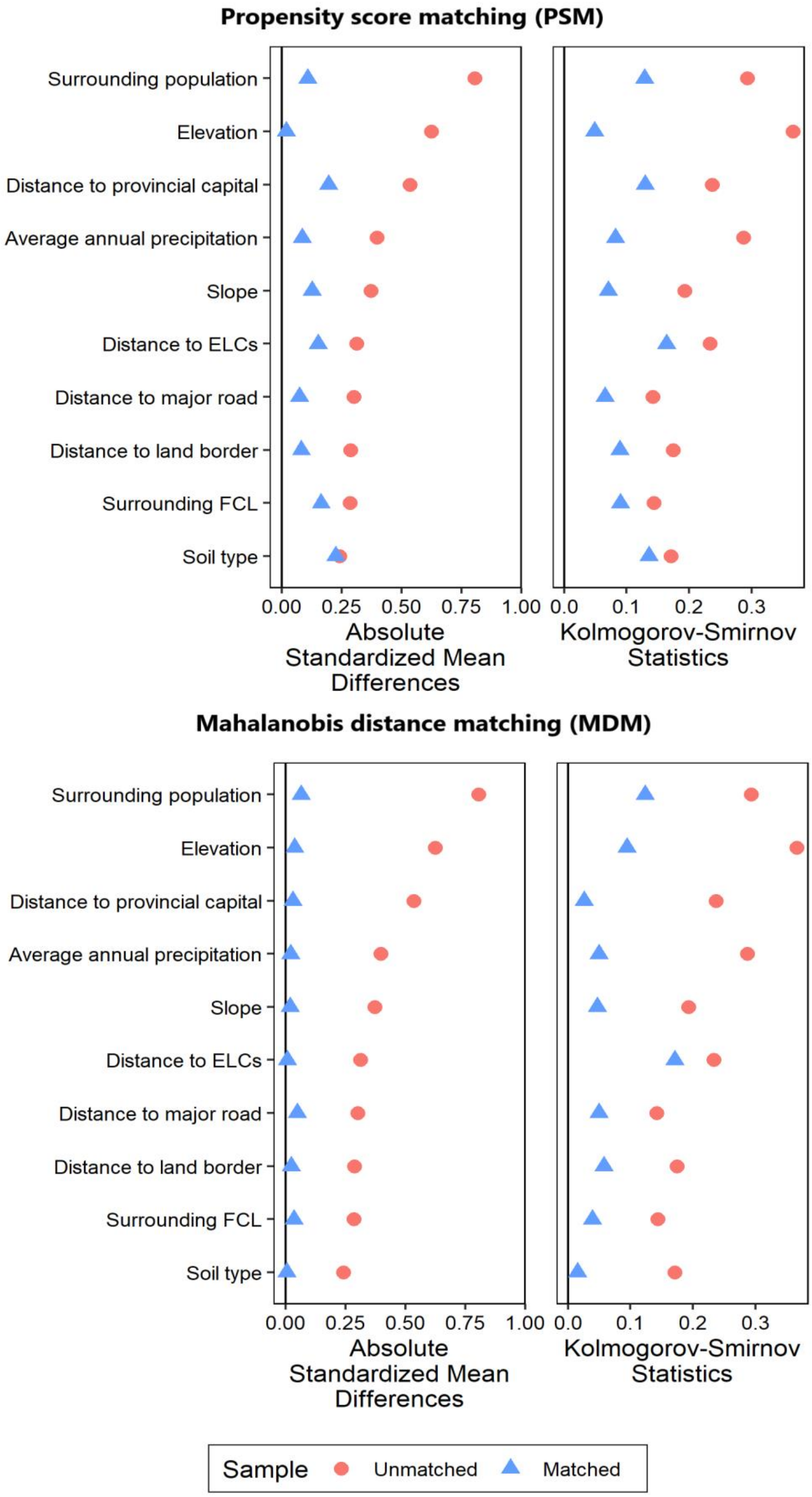

Figure K1: Summary statistics for covariates in the 2010-2012 outcome period under different matching methods

From Figure K1 it is clear that the MDM approach results in greater reductions with respect to both ASMD and the KS statistics values across all of the covariates as compared to PSM. On the basis of this observation alone it would be reasonable to conclude the MDM is the more favorable of the two approaches, however this becomes less clear-cut when taking into consideration the number of observations matched under each approach which are presented in Table K1 below.

Table K1: Summary information from the trials of increasing sample sizes under different matching approaches

	Propensity score matching (PSM)			Mahalanobis distance matching (MDM)		
Random sample size:	10%	20%	30%	10%	20%	30%
Time to completion (mins)	10	86	188	10	118	309
ATT estimate	-0.0925	-0.0911	-0.0917	-0.0888	-0.0795	-0.0875
Abadie-Imbens SE	0.0019	0.0014	0.0011	0.0026	0.0017	0.0014
T-stat	-47.826	-67.092	-82.869	-34.032	-46.146	-61.751
p value	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001
Sample size (no. of units)	92760	185521	278281	92760	185521	278281
no. of treated units	35227	70778	106446	35227	70778	106446
no. of control units	57533	114743	171835	57533	114743	171835
Number of treated units matched	35227	70778	106446	19518	41730	64579
No. of control units matched	18991	37860	56697	5816	10403	14555
No. of unmatched control units	38542	76883	115138	51717	104340	157280
% of control units matched	33.01	33.00	33.00	10.11	9.07	8.47
Number of matches dropped by caliper	0	0	0	15709	29048	41867

Table K1 shows that whilst MDM for the 10% sample did achieve better covariate balance it was only able to match 19518 of the total 35227 treated units in the sample with 15709 matches excluded by the caliper (0.5 SDs of the Mahalanobis distance value). Furthermore, because matching with replacement was specified, these matches utilized only 5816 control units approximately 10% of the total available. By contrast the PSM approach for the 10% sample was able to match all treated units (35227) with 18991 of the control units (33% of the total). These trends in the proportions of treated and control units matched under each approach were fairly consistent across the additional trials using 20% and 30% random samples of the 2010-2012 data respectively. To attempt to increase the proportions of treated and control used the MDM approach was repeated with the specification of matching without replacement however this did not result in a notable difference. This led to the conclusion that despite it achieving less impressive improvements to covariate balance PSM was preferable to MDM as the potential to weaken the results of ATT estimation through reliance on only a small proportion of the treated and control units was too strong to ignore.

The next issue to be addressed was the question of what size of sample was feasible to pursue for the matching to be completed as part of the spillover, primary and secondary analyses. Table K1 shows that the time taken for completion of the matching script in RStudio do not increase linearly with sample size, for example the 10% sample was completed in ~10 minutes whereas the 20% took 86 mins. Logically then it is preferable to utilize a smaller sample size and bootstrap the matching analysis by testing multiple random samples and average the results to produce ATT estimates. Indeed, this is an approach that has been utilized in other studies of PA effectiveness such as Nolte and Agrawal (2013).

This method appears viable given that the covariate distributions between treated and control groups were already close prior to matching (Appendix I.3) although it can only be acceptable if there are no clear differences in matching ‘performance’ when comparing different sample sizes under the same approach. In this regard Table K1 shows that whilst the Abadie-Imbens SE and T-stat do change slightly between the sample sizes, the ATT estimates (all of which are significant: $P<0.001$) and the % of control units matched are fairly consistent. Additionally, the love plots of covariate balance for the 20% and 30% random sample runs showed only slight differences in reductions in ASMD or KS statistic values across the covariates as compared to the 10% sample in Figure K1 above.

This confirms that the use of sample sizes equating to that of the 10% random sample tested is appropriate. Of course, given that the full samples of treated and control units vary in size for the spillover analysis as compared to the primary analysis (Table 3, section 5.1.1) then there is a need to frame this sample size as a relative number. On this basis it was decided that sample sizes for matching should not exceed ~100,000 units and should contain a ration of approximately 1:3 treated to control units.

L. Sensitivity analysis for unobserved covariates

As described in section 4.3.5 the matched results from the 10% random sample of the 2010-2012 outcome period that were tested in Appendix K were subjected to Rosenbaum bounds sensitivity analysis for unobserved covariates. Given that the dependent variable is dichotomous the 'binarysens' function in the rbounds package (Keele 2014) was used to calculate the lower and upper bounds of the p-value (Mc Nemar's test) for differences in the probability of treatment assignment at different values of the sensitivity parameter (Γ) (Keele 2010). These are presented in a redacted form for Γ values of 1-4.6 at increments of 0.1 in Table L1 below.

Table L1: Rosenbaum bounds sensitivity analysis for the matched sample from the 2010-2012 period

Sensitivity parameter (Γ)	P-value (McNemar's test)	
	Lower bound	Upper bound
1 – 4.1	0	0
4.2	0	0.00005
4.3	0	0.00053
4.4	0	0.00401
4.5	0	0.02043
4.6	0	0.07286

Table L1 shows the that the upper bound of the p-value did not exceed the 95% confidence limit (P>0.05) until a Γ value of 4.6. In practical terms this means that for an unobserved covariate to significantly alter the rate of deforestation occurrence in the matched sample that unobserved covariate would have to produce a 4.6-fold increase in the probability of treatment assignment (*p. 10*). This is considered a high Γ value (Keele 2010; Leite 2017) and indicates that the matching results are robust to unobserved covariates i.e that the current selection of covariates is strong and requires no adjustment.

M. Average values of covariate summary statistics pre- and post-matching in the 2013-2015 outcome period (primary analysis)

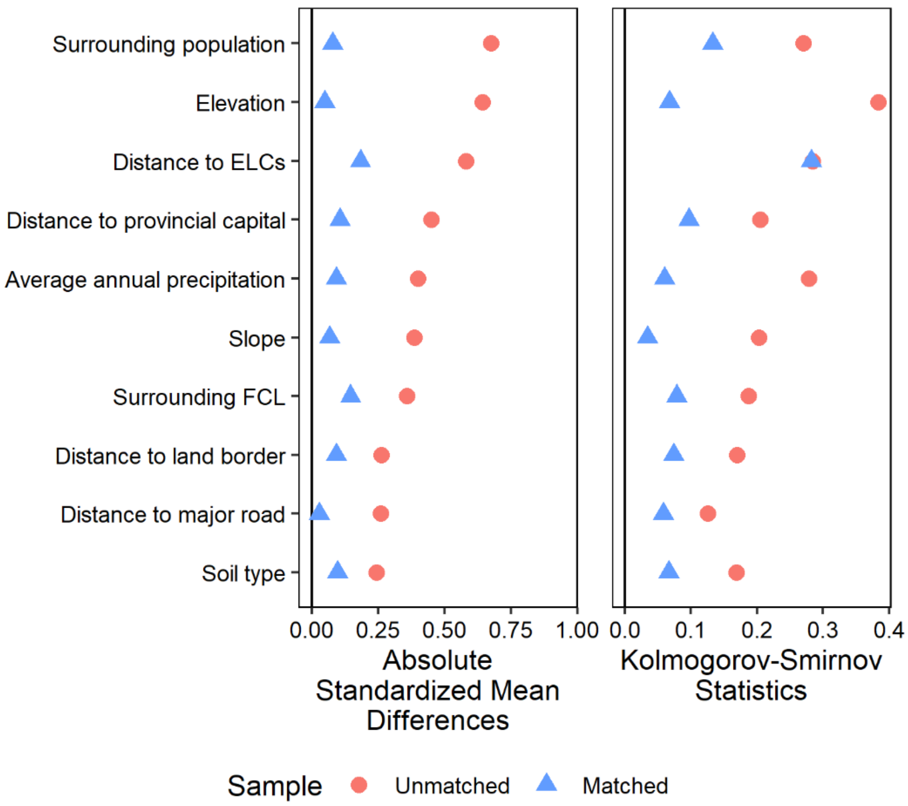

Figure M1: Average values of covariate summary statistics pre- and post-matching in the 2013-2015 outcome period (primary analysis)

N. Treatment effect estimates for all matched-samples (secondary analysis)

Table N1: Treatment effect estimates for all matched-samples (secondary analysis)

		Outcome period					
		2010-2012		2013-2015		2016-2018	
PA establishment period	Sample No.	ATT^{signif}	bias-adjusted SE	ATT^{signif}	bias-adjusted SE	ATT^{signif}	bias-adjusted SE
1993-2000	1	-0.077***	0.00222	-0.067***	0.00215	-0.024***	0.00244
	2	-0.069***	0.00215	-0.062***	0.00208	-0.021***	0.00243
	3	-0.070***	0.00213	-0.061***	0.00210	-0.022***	0.00242
	4	-0.075***	0.00217	-0.063***	0.00211	-0.027***	0.00246
	5	-0.074***	0.00215	-0.058***	0.00209	-0.025***	0.00245
	6	-0.075***	0.00216	-0.059***	0.00210	-0.025***	0.00242
	7	-0.066***	0.00208	-0.064***	0.00211	-0.035***	0.00252
	8	-0.070***	0.00214	-0.062***	0.00209	-0.025***	0.00246
	9	-0.075***	0.00218	-0.055***	0.00205	-0.024***	0.00244
	10	-0.070***	0.00216	-0.058***	0.00213	-0.032***	0.00253
	11	-0.070***	0.00210	-0.056***	0.00210	-0.029***	0.00245
	12	-0.071***	0.00217	-	-	-	-
	13	-0.070***	0.00213	-	-	-	-
2001-2010	1	-0.214***	0.00291	-0.119***	0.00277	-0.016***	0.00306
	2	-0.217***	0.00291	-0.117***	0.00278	-0.013***	0.00303
2011-2016	1	-	-	-	-	-0.061***	0.00236
	2	-	-	-	-	-0.068***	0.00242
	3	-	-	-	-	-0.054***	0.00234
	4	-	-	-	-	-0.060***	0.00237
	5	-	-	-	-	-0.074***	0.00249

O. Secondary analysis covariate balance for 2013-2015 and 2016-2018 periods

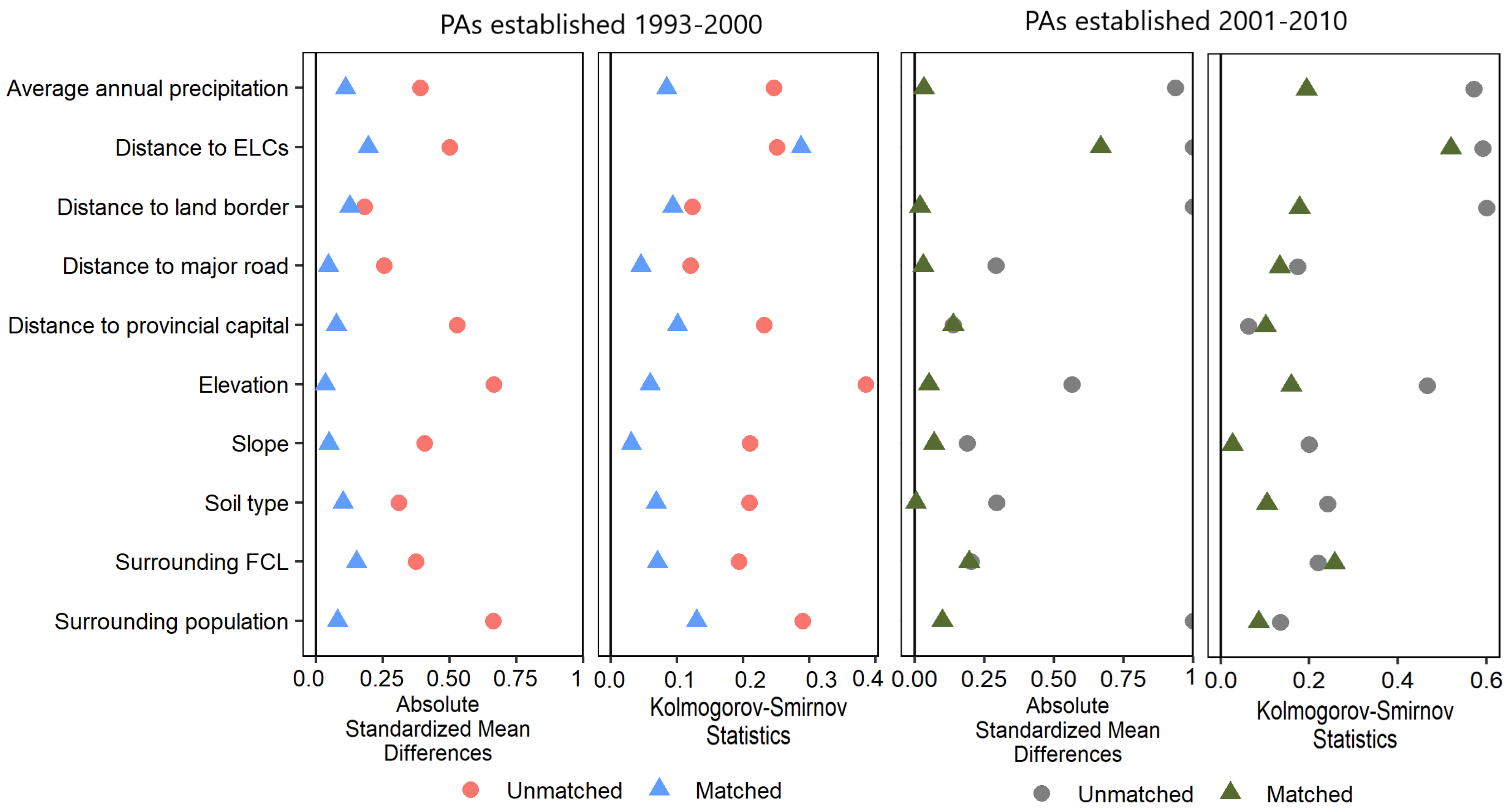

Figure O1: Average values of covariate summary statistics pre- and post-matching for PAs grouped by establishment dates in the 2013-2015 outcome period

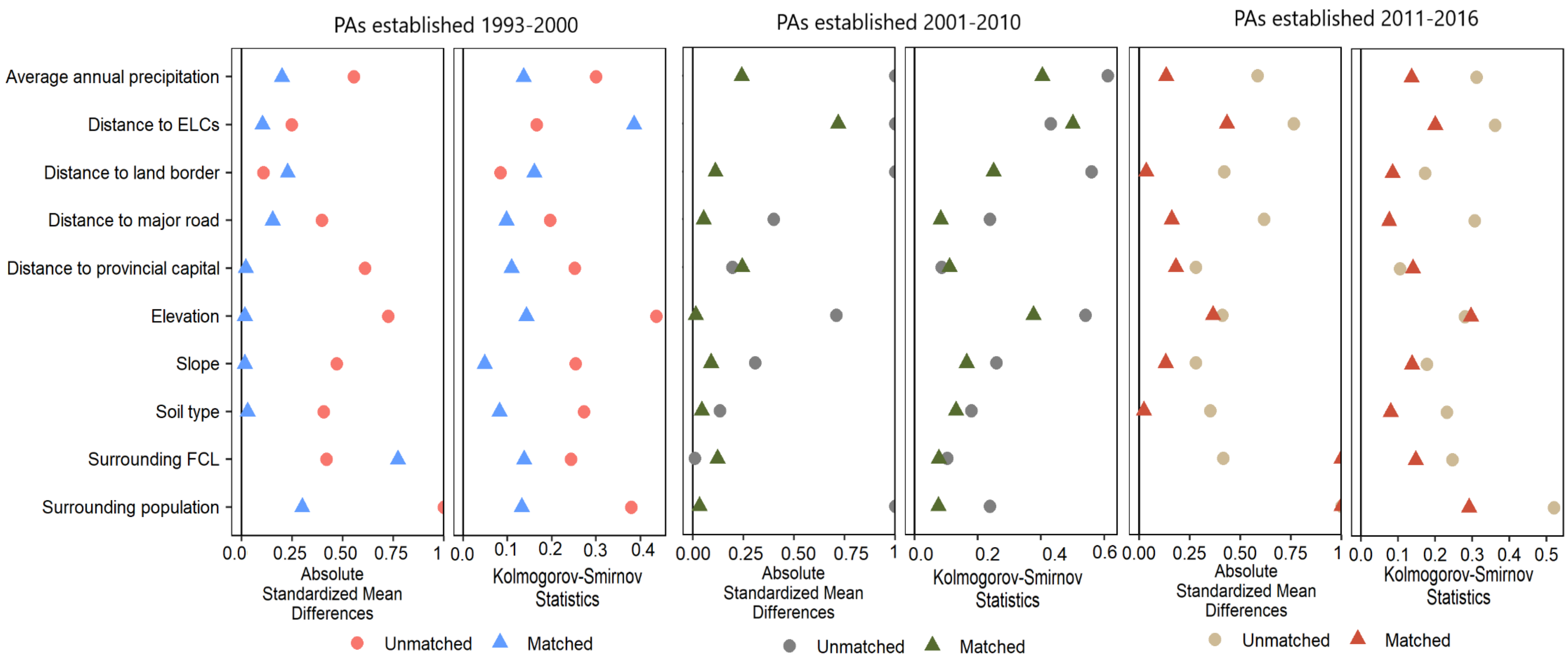

Figure O2: Average values of covariate summary statistics pre- and post-matching for PAs grouped by establishment dates in the 2016-2018 outcome period