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Methodological Pluralism in Extreme Weather Event Attribution Studies

By

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Abstract

This thesis investigates the possibility of a pluralistic approach in the field of Extreme Weather Event Attribution Studies between the Probability-Based Approach and the Storyline Approach through a thorough philosophical comparison of these methodologies based on their approach to defining events, their attribution measures, their reliance on historical data, their proneness to selection bias, and their value and error preferences when communicating results. As a result, the proposed scheme is a form of Integrative Pluralism based on the results of the mentioned comparisons titled here as Non-Unificatory Local Integrative Pluralism which allows a non-unificationist integration between these methodologies on a local level while maintaining their boundaries along with their interaction on the other levels.

Keywords: Pluralism, Extreme Weather Event, Probability-Based Approach, Storyline Approach, Integrative Pluralism

Declaration of Originality

Hiermit versichere ich, Zhyar Nasruddin, die vorliegende Masterarbeit ohne Hilfe Dritter und nur mit den angegebenen Quellen und Hilfsmitteln angefertigt zu haben. Alle Stellen, die Quellen entnommen wurden, sind als solche kenntlich gemacht worden. Diese Arbeit hat in gleicher oder ähnlicher Form noch keiner Prüfungsbehörde vorgelegen.
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I hereby formally declare that I, Zhyar Nasruddin, have written the submitted thesis independently. I did not use any outside support except for the quoted literature and other sources mentioned in the paper. I clearly marked and separately listed all of the literature and all the other sources I employed when producing this academic work, either literally or in content. This thesis has not been handed in or published before in the same or a similar form.
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Acknowledgments

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Chapter One: Introduction

The idea of having one single methodology describing all the information about any phenomenon has been the ideal target to achieve for many philosophers and scientists since the time of the Ancient Greeks, perhaps even earlier (Cat 2023). Such a unificationist vision of methodology was, in many instances, accompanied by a reductive view of the different methodologies into a single grand unificatory one. But, the high complexity of both these methodologies and the target phenomenon they were describing has always been an obstacle in achieving this project. Although *Reductionism*, the idea of reducing a methodology in another that is more fundamental and encompassing, has been successful on certain fronts, it either failed to be established or was only partially achievable in many other areas. So, it is natural to see that in science, there can be multiple approaches, theories, models, etc. trying to explain the same phenomenon using different techniques, either at the level of attaining different explanations for the same phenomenon or different aspects of that phenomenon. Such situations in science are common, and they have been labeled under the general encompassing term of *Scientific Pluralism*; the coexistence of heterogeneous representations (including theories, models, and explanations) for understanding the world (ibid), where these different representations can provide competitive or complementary accounts of the target phenomena.

My aim in this thesis is to explore the possibility of Pluralism in the field of *Extreme Weather Event Attribution Studies*, which is the field of climate science that focuses on attributing the changes in Extreme Weather Events to Climate Change, between the different methodologies that are being used in the field, namely the Probability-Based Approach and the Storyline Approach. The nature of this exploration is based on a thorough comparison of these two methodologies in a multi-dimensional way to attain a compatible base for a potential pluralistic approach that would be fitting in this context. The approach I take in investigating Pluralism is a *pragmatic* one, meaning that the question of favoring a pluralistic approach in our context depends on the idea that the indispensability of any methodology is based on the pragmatic aspects it provides (Van Bouwel et al. 2021). Thus, the indispensability of the concluding remarks from the comparison will provide the explanatory ground for why Pluralism might be an option in this context, and it helps in identifying the compatible pluralistic approach.

This thesis consists of three other chapters and a conclusion:

In *Chapter Two*, I provide an overview of the field of Extreme Weather Event Attribution Studies as well as the definition of an Extreme Weather Event, which is abbreviated here as EWE, in this context. In *Section 2-1,* I distinguish between the two types of factors relevant to all EWEs, which are thermodynamic and dynamic factors, and discuss the basis for such a distinction as well as the nature of uncertainties involved in each type. In *Section 2-2,* I provide an overview of the two primary methodologies used in the field of EWE Attribution Studies, namely the Probability-Based Approach (abbreviated here as PBA) in *Section 2-2-1* and the Storyline Approach (abbreviated here as SA) in *section 2-2-2*.

In *Chapter Three*, I provide a comparison between the PBA and the SA on multiple levels. *Section 3-1* is dedicated to the way each methodology approaches and frames EWEs to investigate them as well as the limitations of these framing preferences. *Section 3-2* is where I discuss the differences between how each methodology attributes the amount of contributions of Climate Change (abbreviated here as CC) on changing an EWE, and discuss the limitations of their contributional methodology. *Section 3-3* is to investigate how reliant each methodology is on available historical data, and whether this is a crucial aspect of such a study for that methodology. *Section 3-4* is dedicated to discussing the types of selection bias that both methodologies are prone to and the reasons behind their proneness. *Section 3-5* is dedicated to discussions regarding the value discussions between both methodologies on many levels and their implications on the results produced. It is composed of four subsections: *Section 3-5-1* outlines the controversial debate regarding the methodologies' tendency to either overestimate the effects of CC on an EWE or underestimate it. The subsections focus on the discussions regarding the Error Type proneness of each methodology (*Section 3-5-1-1*) and the basis for their preferred Error Type Symmetry (*Section 3-5-1-2*). *Section 3-5-2* considers the underlying values of the results obtained from each methodology and how they are communicated. It also discusses the idea of value-free climate information. *Section 3-5-3* discusses the types of information that stakeholders and decision-makers require for their purposes, and how the methodologies could fulfill the different stakeholders' needs.

In *Chapter Four*, I rely on the results of the comparison of the previous chapter to see whether a pluralistic approach could be established between the methodologies. And if so, what should it look like? *Section 4-1* engages in discussions regarding the results of the previous chapter to serve as requirements that should be taken into account in answering the questions regarding Pluralism and its possible form. *Section 4-2* uses those conclusions to support the idea of Pluralism between the methodologies and outlines, investigates, and eliminates many forms of Pluralism based on their compatibility with the mentioned requirements. The two remaining possibilities among those pluralistic approaches are further explored in *Section 4-2-1* based on a case study of Shepherd's Common Framework for Approaches to Extreme Event Attribution (2016). In *Section 4-2-2*, I argue for the compatibility of a new form of Integrative Pluralism which I title as *Non-Unificatory Local Integrative Pluralism,* that argues for a non-unificationist integration of both methodologies at local levels where such an integration is possible while at the same time maintaining the boundaries and considerations of both approaches, and later explore some criticism and limitations. This outcome serves as the conclusion reached based on the comparisons and arguments outlined in the previous parts of the thesis, and is the concluding remark mentioned in *Chapter Five*.

It is important to emphasize that the comparisons I provide in this work between the PBA and the SA are not necessarily the only types of comparisons that can be made about the two approaches, for there could be other comparisons on many other levels. Although such a comprehensive comparison would require more access, collaboration, and research methods that go beyond the capacity and scope of this project, I still hope for this thesis to serve as a first step toward such a project.

Another point to mention here is that apart from the mentioned abbreviations, some other abbreviations in this thesis include: Sea-Surface Temperature: SST, Fraction of Attributable Risk: FAR, and Probability Ratio: PR.

Chapter Two: Extreme Weather Event Attribution Studies

The field of Extreme Weather Event Attribution Studies is a relatively recent subfield of climate science. The main difference between this one and other subfields is that it goes beyond some of the fundamental research questions studied by the other subfields. For instance, it assumes the idea that there is a Climate Change (CC), and it also assumes that parts of this change are due to anthropogenic factors. But, the main focus of these studies is on the way these anthropogenic forcing contributed to CC and changes in EWEs.

The phenomenon of interest in these studies is what is titled in climate science literature as an *Extreme Weather Event* (EWE). According to Lloyd and Shepherd (2020), an EWE is 'any fluctuation in the state of the physical climate system (ocean included) that has a component of natural variability'. To further analyze what is meant by the previous definition, more information is needed on what is meant by the word *extreme* in this context. In such studies, this word can be used in two different ways:

- One can be a definition of the word based on the statistics or rarity of the event, also referred to as a probability-based definition, in which the events occur above or below the given thresholds near the ends of a climatological distribution (eg. below 5% of the distribution), or with respect to a specific return frequency (e.g. 100-year event) (Seneviratne et al. 2012). For such a definition, two aspects are needed: A reference period for defining these thresholds, which is either a historical period like 1961-1990, or a model-simulated world, and a difference factor that can be either 10%, 5%, or 1% chances of occurrence (ibid). What needs to be noted here is that the threshold definition of what counts as 'normal' is not a fixed definition and changes over time, and today's extreme events might be considered normal in the future (NAS 2016).

- The other one is a definition based on the impact of the event, also referred to as threshold-based definition, in which a certain threshold for impact is decided, given different conditions, and any event exceeding that threshold would be considered an EWE. These thresholds can be based on sociological, ecological, or physical system impact. Factors relevant to this definition include the event's duration, affected spatial area, timing, frequency, onset date, continuity, and preconditioning (Seneviratne et al. 2012).

These mentioned definitions might not necessarily be opposing, for an EWE in the tails of the distribution can be impactful and exceed the statistical threshold at the same time (an example would be a 40°C threshold for a midday temperature in the mid-latitudes), but they are not equivalent either, for a rare event might not necessarily be impactful and vice-versa (tropical cyclones in some regions are not rare but are very impactful) (Seneviratne et al. 2012; Lloyd and Shepherd 2020). But how to define a EWE remains an open question given the non-unique and interchangeable usage of the word with other words, such as *rareness* and *severity* (D.B. Stephenson et al. 2008; Seneviratne et al. 2012). Considering the mentioned differences, many climate scientists prefer to use the statistical definition since it is less subjective compared to the alternative impact-based option (Lloyd and Shepherd 2020).

Returning to the earlier definition, the field of EWE attribution studies focuses on identifying attributable changes in the frequency and severity of EWEs to anthropogenic forcing. The two main methods used for such purposes are the Probability-Based Approach (PBA) and the Storyline Approach (SA) (more on this in section 2-2). The way these researches are done by the PBA can be roughly summarized in two steps: the first being the 'detection' process, where unusual events are detected given the availability of long homogeneous records[1], and the second is the 'attribution' process, where an investigation of the contributions of the human influence on the event is measured (Stott et al. 2010; and Trenberth 2011), while for the SA it is

[1] In this step, a possible aspect of uncertainty would be the unavailability of such a long dataset, or the presence of one which includes observational, instrumental, and spurious effects uncertainties. (Trenberth 2011).

carried out through building a plausible causal pathway of factors of how a single EWE came to be given recent data on the event (Shepherd 2019).

An important theme to be mentioned here is related to the causal relationship between EWEs and anthropogenic forcing. The information tying CC and EWEs comes in the form of contributions of the former to increasing the frequency or severity of the latter. There are two reasons why we cannot provide deterministic inferences stating that CC was the cause of the formation of an EWE: The first reason is that the formation of an EWE is a complex phenomenon that requires the alignment of different environmental factors of the climate system, and it is hard to determine how CC affects this since it does not affect a single aspect of the system, but it affects the system as a whole. So, it makes it hard to draw deterministic conclusions here (NAS, 2016). The second one is that EWEs are not a new phenomenon that rose with the rise of CC but have been recorded in history long before the pre-industrial era (IPCC 2021, AR6, chapter 11, sections 2.3 and 2.4). Thus, one cannot infer deterministically that CC is the cause of the formation of an EWE but can only make contributional statements regarding the way CC affects changes in an EWE.

Nonetheless, one of the core reasons why these studies are epistemically relevant lies in the fact that they are a very good indicator of a changing climate. For instance, suppose that we want to investigate changes in global mean temperature. In an instance of a small change, most of the weather conditions would continue to be the same (as can be seen in the intersection areas of the two distributions in **Figure 1**), thus showing little to no indication of a changing global temperature. But it is at the tails of the distribution, where many EWEs happen, where significant changes can be detected since it would result in changes in EWEs (either becoming less or more frequent). Whether the change is a shift in the mean, an increase in variability, or a change in the symmetry of the distribution, the tail points are the most significant spots for change detection. Thus, studies performed on EWEs can provide epistemically significant evidence for detecting such changes in the climate (Trenberth 2011).

It is also important to add here that not all types of EWEs happen due to purely meteorological factors. Certain EWEs, such as wildfires and floods have a component of external anthropogenic factors, like forest burning, dams, and land management, that contribute to the severity and frequency of these events, as well as increasing the complexity of integrating these factors into the studies (NAS 2016). It is also worth mentioning that these events may not be necessarily single events, for there can be compound events of either 1) two EWEs happening simultaneously, 2) a combination of an EWE and amplifying factors, or 3) a combination of events which are not individually EWEs themselves, but would result in an EWE or impact when combined (Seneviratne et al. 2012). But I will leave this issue here since it is beyond the focus of my research. In **Table 1** and **Figure 2**, you can see which EWEs provide the most confident results and which factors contribute to our confidence levels regarding the results.

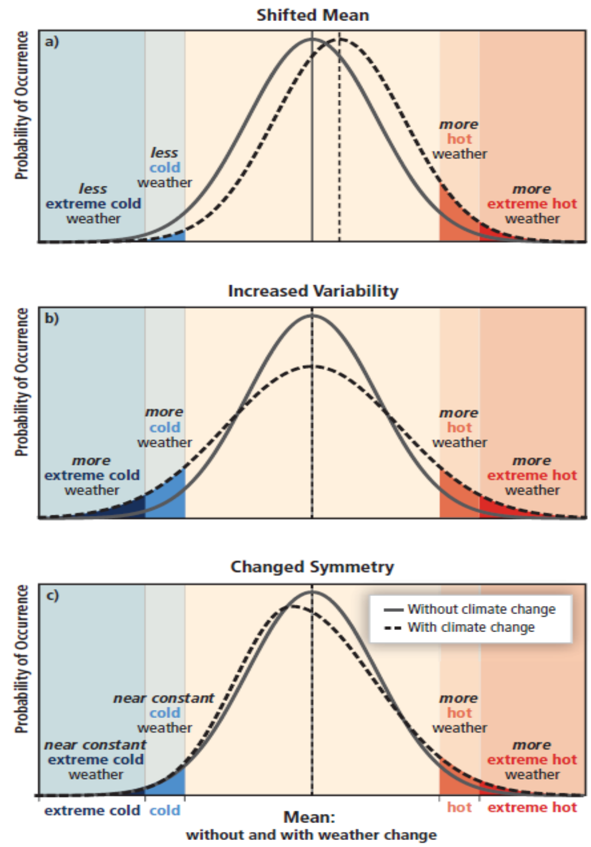

Figure 1: With changes in the global mean temperature, many of the climate conditions would seem unaffected (the intersection zone), it is only at the tips of the distribution where significant evidence for a changing climate can be detected (NAS 2016).

Before diving into the main content of this work, I believe it is important for us to get a clear grasp of some essential distinctions regarding the climatological factors involved in EWEs. This will be the topic of the next section. And in the section following that, I will outline the two main methodologies used in EWE Attribution Studies.

● = high **O** = medium ○ = low	Capabilities of Climate Models to Simulate Event Type	Quality/Length of the Observational Record	Understanding of Physical Mechanisms That Lead to Changes in Extremes as a Result of Climate Change
Extreme cold events	●	●	●
Extreme heat events	●	●	●
Droughts	**O**	**O**	**O**
Extreme rainfall	**O**	**O**	**O**
Extreme snow and ice storms	**O**	○	**O**
Tropical cyclones	○	○	**O**
Extratropical cyclones	**O**	○	○
Wildfires	○	**O**	○
Severe convective storms	○	○	○

Table 1: This table of NAS (2016) indicates the state of attribution science to different types of EWEs in the form of confidence estimates (circles) for modelling capabilities, length and quality of the records, and the understanding of the physical climate system behind the EWE.

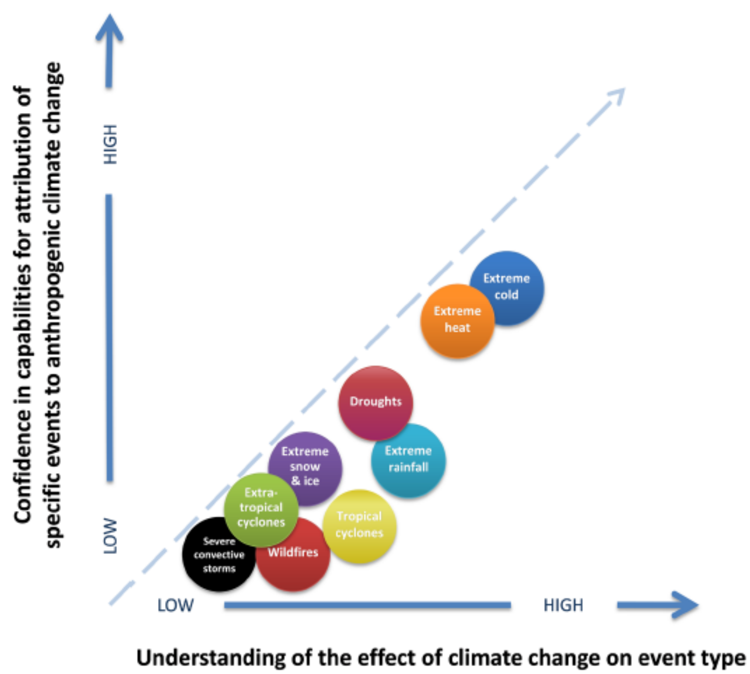

Figure 2: Graphic representation of Table 1 by NAS (2016) outlining the difference between types of EWEs as a measure of our understanding of the physical system vs the confidence in the results about a particular EWE. The 1:1 line indicates the amount of improvements needed in the understanding of the system to reach a confidence level corresponding to the current amount of understanding. The reason why all types of EWEs lie below the 1:1 line is because it is impossible to have more confidence in attribution results with the absence of the adequate understanding of the EWE.

2-1 Thermodynamic vs. Dynamic Factors

In this section, I will try to briefly explain the distinction between the two forms of factors that lead to the formation of EWEs, for understanding these factors is an essential part of understanding both methodologies, their differences, and the results they obtain based on their considerations of these two factors.

In climate science literature, especially those related to EWEs, the meteorological factors contributing to the formation of EWEs are divided into two broad categories. This division is made because, on a physical level, the atmosphere is roughly composed of two forms of processes: heat exchange and wind circulation patterns. According to Shackley (2001), climate scientists as well are divided among two camps; the Thermodynamicists treat the climate system as a box of input and output of heat flux and radiation in which the dynamics do not appear to change the heat balance, thus the system can be treated thermodynamically while Dynamicists believe that neglecting such factors leads to many model errors that the Thermodynamicists would be unaware of, and they argue that the results provided by only considering thermodynamic treatment would end up in unrealistic results because dynamics can play mitigating roles in many of those cases (ibid). Based on this division among climate scientists, a distinction can be made between the two forms of factors involved in the atmosphere in general and the formulation and effect on EWEs in particular:

- Thermodynamic factors: These are factors that are related to heat and heat exchange in the atmosphere and their associated feedbacks. These include factors such as global, continental, land versus ocean, and polar regions temperature changes, the water-holding capacity of the atmosphere (Clausius–Clapeyron), melting of ice, sea level rise, arctic sea ice, glaciers, and other factors.

- Dynamic Factors: These are factors that are related to wind and oceanic circulation along with their associated factors. These include factors such as circulation patterns, ocean currents, storm tracks, persistent atmospheric circulation anomalies, El Niño conditions, and other factors.

The distinction between thermodynamic and dynamic factors is not as clear in practice as in theory, and there are two reasons behind this. The first reason is that many climate factors are not necessarily on one side of this categorization, for they can be the result of both dynamic and thermodynamic factors as well. An example of such factors would be precipitation patterns which happen as a result of both the water-holding capacity of the atmosphere (thermodynamic) as well as circulation patterns (dynamic). The second reason, which is related to the complexity of the climate system, is that it would be naive to think of these factors as being independent of each other since in many cases, a change in one of the factors would lead to a change in multiple other factors. Thus, making it more complex to trace back whether the cause of change of one factor was due to change in another, such as anthropogenic forcing, or multiple other factors since in many instances changes in one factor leads to changes in the whole system.

The thermodynamic aspects of the climate have been labeled as the robust aspects across models since little deviation can be found across models related to these variables (even on a global scale), and the epistemic uncertainty here is quantitative (Trenberth 2011; Lloyd and Shepherd 2020). On the other hand, the dynamic aspects of the climate are not as robust and they lead to many uncertainties in model results, and the epistemic uncertainty here associated with atmospheric circulation are deep uncertainties (or Knightian uncertainty) which mean they are not expressible in numbers and we are not in a position to provide probabilistic statements about them (Shepherd 2016). Added to this is the fact that dynamic aspects modulate thermodynamic aspects on a regional level and they must be taken into account, making the system more chaotic and harder to predict. This is not the case at a continental scale since it is primarily driven by thermodynamic aspects (Shepherd 2016; Shepherd et al. 2018; Shepherd 2019).

The reason behind this difference in model predictions regarding the dynamic aspects of the atmosphere lies in the fact that these factors can have a mitigating effect on thermodynamic factors. This results in either small or undetectable changes in the overall change that sometimes can be misattributed as natural variability (Shaw et al. 2016; Shepherd 2019). Another reason is that detecting these observations is related to the small signal-to-noise ratio of forced circulation changes (Shepherd 2019). The basis for these issues, outlined by Shepherd (2014), is due to the vast variability of multidecadal data (the data being so variable that does not exhibit any systematic changes), the multifactorial nature of circulation patterns (being related to both a thermodynamic aspect and a dynamic one), and uncertainty related to the physical understanding of circulation patterns (the physical basis of circulation being unclear and there being no consensus on the mechanism) (Shepherd 2014). **Figure 3** is a clear indication of the difference in robustness between thermodynamic and dynamic factors in ensemble models using multi-model means.

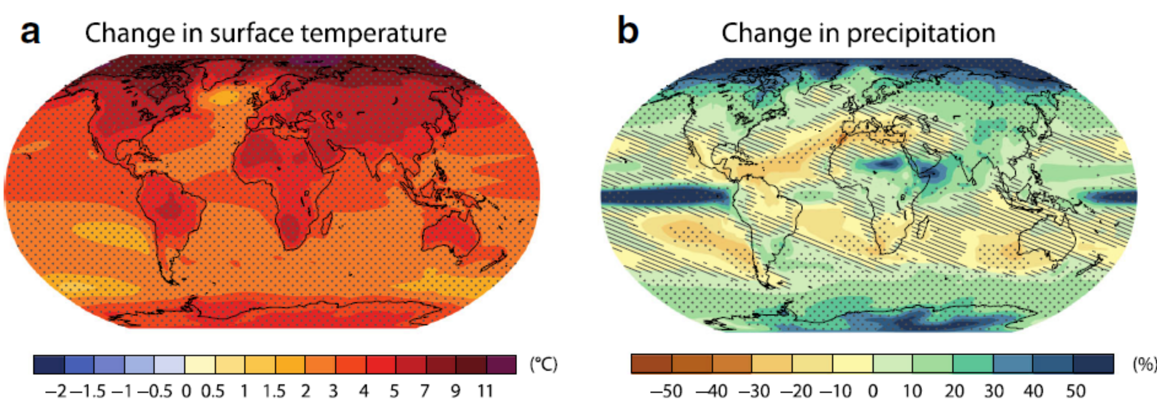

Figure 3: Projected changes for the 21-century in (a) mean surface temperature and (b) precipitation under RCP 8.5 forcing using CMIP5 model ensemble. *Dots* indicate the areas where there is robustness across results with changes in multi-model mean being small compared to natural variability, while the *lines* indicate a large change in the multi-model mean compared to natural variability (Shepherd 2016).

Understanding the distinction here is an important part of understanding aspects of the differences between the two main methodologies discussed in the next chapter. But before that, these methodologies will be briefly explained in the next section.

2-2 A Tale of Two Methods: Storyline vs. Probability-Based

In EWE Attribution Studies, the two main methodologies that are used to investigate EWEs are the Probability-Based Approach and the Storyline Approach. In this section, I explain how these two methodologies differ in the way they approach EWEs.

2-2-1 The Probability-Based Approach

The area of EWE Attribution Studies was first initiated in around 2003 and 2004 by Peter Stott, Myles Allen, Friederike Otto, and their colleagues at the Environmental Change Institute in Oxford and Met Office (Winsberg et al. 2020). Their approach has been used in the literature of science under many names, all referring to the same approach; the Probabilistic Approach, the Risk-Based Approach, the Probability-Based Approach, and the Conventional Approach (mostly used by advocates of the Storyline Approach). Since the name 'Probability-Based Approach' has been suggested by its advocates (Otto et al. 2020), I will use the same term throughout this research as a description of this methodology.

As mentioned in the previous section, these studies happen in two steps. In the detection phase of this approach, observations on possible statistical changes in the climate are carried out through the reliance on available long-term data or (multi-)model simulations (Stott et al. 2010; Trenberth 2011). For this purpose, long and accurate datasets are required to identify trends. I will further discuss issues of such datasets in the next chapter (section 3-3).

The distinguishing step of this approach is in the attribution phase in which our question is to ask whether these detected changes are attributable to *external forcings* (agents that are not a part of the climate system but alter its properties), specifically *anthropogenic forcing* which is the alteration of the climate structure through human-induced emissions or other means (ibid). The way the PBA makes attribution studies can be summarized by stating that it measures the probabilities of classes of EWEs in a world with current anthropogenic forcing conditions against a world with no or less anthropogenic forcing conditions to find the amount of the attributable change.

As mentioned earlier, there can be either a reliance on observational data or model simulation approaches for this attribution. The observational data method can either use historical data to find changes in the distribution of a type of event similar to the event in question (an example of this is King et al. 2015) or to look for a historically analogous event like the event in question in terms of circulation states (an example for this would be Cattiaux et al. 2010) (NAS 2016). The modeling approaches could be global atmospheric models, coupled climate models, or event-specific models (models used to represent a specific event). They can be either used as models to simulate an EWE under different initial conditions (factual conditions vs. counterfactual conditions) or to find how likely the event with the given characteristics is compared to a distribution of possible conditions in a world without human-induced emissions (ibid).

In the attribution process, the PBA relies on two methods for calculating how much of these changes are attributable to CC. The first one is the *Fraction of Attributable Risk (FAR)* which measures the fraction of the risk attributable to a variable that is present at an instance compared to another instance where it is absent. The equation has the format FAR = $p_1 - p_0 / p_1$, where p_1 is the probability of the occurrence of an event in a world under the current anthropogenic forcing, and p_0 is the probability of the occurrence of the same event under a counterfactual world without anthropogenic forcing. The second one is *Probability Ratio (PR)* which is the suggested new title for Risk Ratio (see Otto et al. 2020). It is a measure that describes by what factor the presence of the questioned variable led to an increase in the likelihood of the event. The way this is done is through the equation PR = p_1 / p_0, with the same definitions of p_1 and p_0 as the previously mentioned one. It is important to note here that FAR has a causal interpretation, meaning it ties back the changes to the questioned variable's presence or absence while PR is providing us with the same thing without any causal commitments. I will further discuss the implications of these two methodologies in section 3-2.

The PBA provides attributions for many types of EWEs, considering that they can be assigned to a class. These studies are mostly through unconditional attributions in which all the involved factors (both the thermodynamic and the dynamic aspects of the atmosphere) are represented in the investigation. Thus, the PBA results are thought of as results based on the interaction of all the climate variables in an unconditional manner. It is also worth mentioning that not all are unconditional since some studies conditionalize on certain factors, such as SST (NAS 2016). But overall, the PBA advocates for the inclusion of all the factors affecting an EWE regardless of the uncertainties arising from this inclusion.

Apart from the mentioned details, what needs to be added here is that the results provided by the PBA come in terms of changes in the probability or intensity of classes of events. Thus, the way single events are framed is by assigning them to a class of similar events and using the magnitude of the event as a comparison criterion to see how likely events of such magnitude would be in a world with no or less anthropogenic forcing. Thus, attributing a certain single event to a class of similar EWEs is necessary for this method to produce results.

2-2-2 The Storyline Approach

The second method of studying EWEs is called the Storyline Approach which was developed after the PBA. Multiple names have been used to describe such an approach, including Tales, Narratives, Scenarios, and Storylines (Shepherd et al. 2018), but I will stick to using the term 'the Storyline Approach' as a description for this methodology. When it comes to defining the

SA, one can use the definition provided by Shepherd (ibid) as 'a physically self-consistent unfolding of past events, or of plausible future events or pathways'. The motivation behind this approach for studying EWEs came as a way of dealing with the limitations of the former approach both on an epistemological level and a pragmatic one.

The way the SA performs these studies is particularly different from the PBA. The first aspect of it is that the SA focuses on EWEs as single events rather than attributing them to a class of events. According to the SA, each EWE is unique given the different conditions that can lead to the rise of such an event and therefore does not necessarily belong to a class. This difference and its implications are further explored in section 3-1.

Instead of focusing on assessing the changes in probabilities using statistical approaches to studying EWEs, the SA tries to build causal chains of possible driving factors that could have possibly led to the event and assess how plausible such a causal chain is relying on the data available for assessment. Shepherd (2016) compares it to accident investigations where multiple factors are assessed in a plausible recreation of how the accident came to be. The idea behind it is to try to identify and map out ways in which EWEs may have developed. The basis for doing these studies is to rely on observed events in terms of a forensic investigation mapping out the impacts considering the changes to the event's causal factors (Lloyd and Shepherd 2020). Each of the factors in the storyline are conditional factors and the results depend on the value of each factor and their relation to the other factors. So the type of attribution present in the SA is a conditional attribution.

It is not necessarily the case that a single event would only have a single storyline since the aim is not to arrive at a deterministic causal scenario of necessary conditions describing how the EWE had to be, but to attain a storyline of how the EWE plausibly came to be. Usually, it is the case that multiple storylines are built to account for a single EWE. In such cases, the way these studies are carried out is by mapping out the causal factors leading to the past EWE in different ways. But storylines are not necessarily only built for past events. The approach enables building storylines for future events by the creation of counterfactuals to imagine how events could have turned given the changes in the factors. In such cases, futuristic climate conditions are used in either a historical counterpart of the studied EWE or a hypothetical one (Hazeleger et al. 2015). One example of doing this is to integrate the expected future conditions as boundary conditions for a model (Attema et al. 2014). The limitations of the future information provided by this method and the former one are further explored in the next chapter.

The SA approach is causal. Unlike the quantitative descriptions of EWEs in the language of probabilities provided by the PBA, such as 'X event is a 1 in 100 years event', the focus here is on understanding the physical aspects behind an EWE with qualitative precision using a plausible causal structure of factors to provide results on the changes in the severity of an event (Shepherd et al. 2018). So, a statement here would be in the form of 'Climate change led to changes in the jetstream and warming, and these changes in the amount of rainfall, leading to an increase in the river flows and thus increasing the severity of the event' (Lloyd and Shepherd 2020). An example of a causal network for the storyline of an EWE is provided in **Figure 4** (More on the different types of information attainable by each methodology in section 3-5-2).

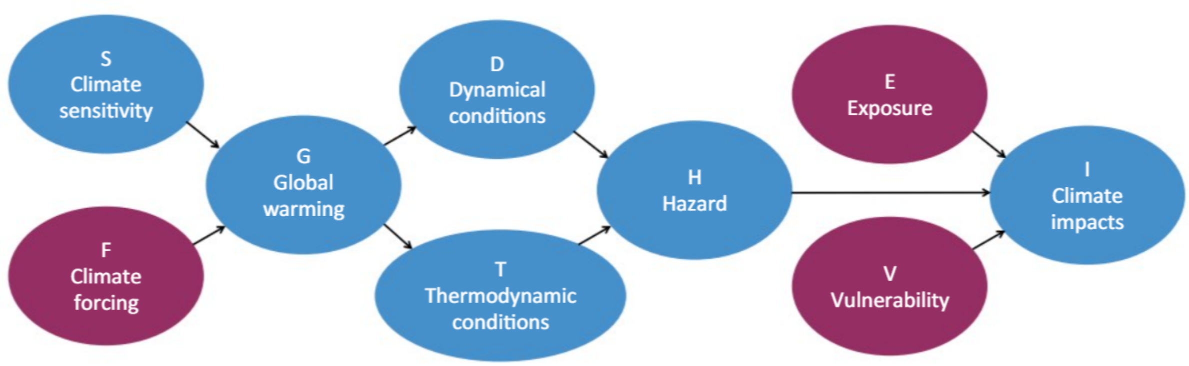

Figure 4: a causal network that represents a plausible storyline for an EWE. The factors that are in purple represents factors of direct human influence, blue represents the factors that aren't directly influenced by humans, and the arrows indicate the direction of causation. (Van Garderen 2022).

Unlike the PBA, the SA uses conditional attribution. The advocates of the SA argue that the approach either focuses on events that are thermodynamic or conditionalizes on the dynamic factors involved (for instance, using dynamic factor data from 5 days before the event as a condition (Swain et al. 2020)). The aim here is not to fall under the burden of deep uncertainties coming along with integrating dynamic factors (this is further discussed in section 3-5). Thus, they are closer to the Thermodynamicist side on the idea that emphasis should be put on the thermodynamic aspect of EWEs than dynamic.

In this chapter, I have briefly outlined the field of EWE Attribution Studies and its importance in climate science, the distinction between the dynamic and thermodynamic aspects of the atmosphere and the approaches towards their consideration, and the two main methodologies of the field of EWE Attribution Studies, namely PBA and SA. In the next chapter, I will thoroughly examine the differences between these approaches and the implications of such differences.

Chapter Three: Where the Conflict Lies

As mentioned in the previous chapter, there are differences between both methodologies in the way they deal with EWEs. These multidimensional differences are the main topic of this chapter. I will provide a list of the differences in procedures, methodologies, considerations, uses, results, and applications and also discuss the implications of these differences to use them as a compatibility basis for our possible pluralistic approach (which will be in the next chapter).

3-1 Event Definition and Framing Limitations

The PBA is an approach that heavily relies on statistical and probabilistic methods to produce outcomes related to changes in the frequency and severity of EWEs. In doing so, they need to assign the EWE in question to a class of similar events to find out how likely an event such as the questioned event would be under no anthropogenic forcing.

But these classifications are not necessarily done based on similar definitions across these events. In other words, there can be different results for an EWE given scale, season, period, region, etc. This creates an issue in terms of classifying these different types of the same event into one class. The way this is handled by the PBA is to provide robust attribution analysis; defined here as results that are qualitatively similar across a range of event definitions (NAS 2016). But there is a quantitative trade-off for this procedure since taking multiple definitions would increase the uncertainties in the results. Although one can argue that this trade-off would be epistemically better since we would not end up cherry-picking the most fitting definition, it still fails to account for the classification of compound events, given that they cannot be assigned to a specific class due to their compound nature (See Chapter 2 for the definition of compound events).

The SA uses conditional attribution in terms of building causal structures of plausible ways an EWE would have come to be. It is mentioned how this is done in the previous chapter (section 2-2). Given this procedure, the SA can provide us with changes in the severity of an event considering different climate conditions or the influence of CC on the factors involved in a causal sense, as well as analyzing counterfactual instances of the event by integrating different climate conditions into the factors.

On the other hand, the SA focuses on attributing the effects of CC on single EWEs. As mentioned by the advocates, this consideration is because they treat each one of these events as unique, given that the same events in different locations or times could have different arising factors. But there are certain limitations of this treatment. One issue with these single EWE attributions lies in the fact that they neither can be generalized to draw conclusions about changes in class types, nor to draw general conclusions about the impact of CC on EWEs as a whole (NAS 2016). The unique treatment of individual EWEs prevents them from being classified and generalized into classes, thus results from the SA would only be limited to either that single event or an analogous event to that event in terms of having similar storylines (can be either past events or be used for making statements about future similar events). This is a bit problematic since for events that are expected to become less frequent given CC, such as extreme colds, the PBA can provide us with an expectation of such an occasion and a justificatory means for occurrence bias (more on this in section 3-4). Another implication of treating each event as a unique one is that it prevents us from using the records of non-analogous but similar in nature EWEs. What this means is that we cannot use the records related to any other, for example, flood to compare it with the flood we are investigating unless the flood in the record is analogous with the current flood; in other words, they have the same storyline of factors leading to the event. While the PBA avoids this by assigning an event to a related class, the uniqueness assigned to each EWE by the SA restricts their usage of historical data to only analogous events. More on the relationship between historical data and these attribution methodologies can be found in section 3-3.

3-2 Interpretations and Limitations of FAR and PR against Causal Networks

One method used by PBA to attribute the amount of added risk is through the Fraction of Attributable Risk, abbreviated by FAR (this is briefly described in section 2-2). There are specific issues and limitations regarding the use of this method for risk attribution. The first one is that if we were to describe events that would be very rare in a counterfactual world (p_0 being very close to zero), the FAR would give us a value very close to 1 regardless of what the probability of the event be in a world with current emissions (p_1), and this is due to the mathematical nature (p_1-p_0/p_1) of the FAR. This is problematic since in many cases not much noticeable might have occurred in these values given CC, but the methodology provides a very high result compared to the probabilities. Not only that but also we would end up with a negative FAR or a zero value for EWEs that are becoming less probable given CC (such as extreme colds), which is hard to interpret (NAS 2016). Apart from the mathematical issue, the causal nature of the method is also problematic, since it causally attributes the changes in the probabilities to CC which might not be the case considering that EWEs are complex events that involve many causal factors. In many cases, there can be aspects of natural variability or some anomaly patterns also affecting the way the EWE is. Thus, the change in the probability of the EWE might not be wholly casually related to CC (ibid). The alternative for this is the Probability-Ratio (PR) which is a factor for measuring relative probabilities. One advantage of using this methodology is that it lacks the causal commitments of FAR, communicating the results in the form of 'Climate change increases the probability of wildfires by a factor X' (ibid).

The SA is a causal approach. The way it operates is by building a plausible storyline for a single event and identifying the factors that CC could affect, and also analyzing how these were influenced by CC. Thus, the results provided by the SA are causal, identifying how CC influenced the severity of an EWE. But one limitation of the SA is that the causal storyline it builds for the EWE under question is not a factual (what it was) causal reformulation of the event, but rather a plausible way of how these factors interacted forming the EWE. As mentioned by the advocates of the SA, there can be multiple storylines for a single EWE, and thus multiple ways to represent the causal factor influences leading to the EWE. There is a trade-off for uncertainty here; while we can attain information about the plausible causal nature of the EWE, which is not provided if we follow PBA, it is still plausible, not what it was or how it happened. Another limitation of this approach is that while it can build plausible causal storylines for single EWEs, it is not the case that it can do so for all types of EWEs. Specific requirements for the EWEs studied by the SA is that they have to be thermodynamic EWE in nature, or the dynamic aspects are not heavily present risking the trade-off of confidence over accuracy (see section 3-5-2).

To sum up the differences, The FAR and PR employed by the PBA are good for causally identifying changes in the amount of risk in classes of EWEs due to CC with hopes that the probability values are not beyond its mathematical limitations and the event is not very complex to misattribute, while casual chains employed by the SA is good for providing plausible causal information on changes in the severity of single events due to CC, with the work scope for particular events (being events that are highly thermodynamic-related).

3-3 Reliance on Historical Data

As mentioned in the previous chapter, the detection phase of the PBA relies on the presence of historical data for detecting the changes in the trends of EWE. But this is problematic in two aspects: the first aspect is related to the unavailability or the lack of quality of such data from containing observational uncertainties, instrumental errors, shortages, external influences, etc. given that they might not be relied upon for observational attribution or integrated into model simulations (although they can be used as an assessment measure for models) (Stott et al. 2010), and the second aspect is related to uncertainties regarding natural variability, which causes some trends to last decades (NAS 2016). These issues also transfer into the attribution phase of the PBA since calculating the p_o (probability of a certain class of EWE given no or less anthropogenic forcing) in many cases also depends on how the frequency of these events was in the previous times. Any misestimations of the p_o value used in the FAR and PR measures makes the PBA results prone to either overestimating or underestimating the effects of CC on changes in the frequency of these events (More on this is in section 3-5).

This case is different when it comes to the SA. What the SA tries to do is to find the plausible chain of causality that led to a specific EWE. Because neither the SA looks at these events in terms of classes nor it aims at finding statistical trends, they do not need to dive into the historical data to find trends and other aspects to detect changes in those trends. What they mostly require is recent records related to the climatological factors related to the EWE under question. For example, it uses atmospheric conditions from 5 days before the event as initial conditions for the model (Swain et al. 2020). Thus, they differ from the PBA by being less reliant on available historical data. The only two circumstances where the SA uses historical data are either for rebuilding the storyline of a recent EWE by using up-to-date records or to use historical records to rebuild a past EWE storyline with futuristic climate conditions to know how it would change given those different climate conditions. Even for such purposes, having long historical data on analogous EWEs would be good to have since it leads to a better rebuild of the storyline, yet it is not required as it is not needed for statistical purposes like the PBA. They also use data for comparing model outputs against the real world, but not as much in terms of amounts as the PBA (Van Garderen et al. 2021). Thus, these data do not necessarily need to be so long since the focus here is not on finding statistical trends requiring vast and accurate amounts of data.

3-4 Selection Bias

With CC being a phenomenon affecting everywhere differently around the globe, one of the issues arising from this would be the choice of events or geographical locations in which the study is carried out. There are limitations and potential problems arising with the ability to attain information on every EWE happening in the world. Therefore, choices and selections need to be made. This leads to *selection bias*, which refers to the lack of proper randomization in the data due to unintentional but systematic choices and the inability to ensure a total representation of the larger target population (NAS 2016). While the report outlines the different types of selection bias and their effect on the quality of research of EWE studies in general, I will only outline here how prone each attribution method is to the mentioned biases. It is also important to mention here that to argue that one approach is more prone to a certain bias than another is not an indication of the liability or a failure measure of the approach, but rather to point out how different biases affect these approaches due to their intrinsic considerations in the way they investigate EWEs as a way of finding possible ways to

The first form of selection bias is *occurrence bias*, which is the idea that the studies will primarily focus on the events that are happening and leave the events which will become less frequent. Since there are events, such as extreme colds, that are expected to occur less due to CC, fewer studies will be carried out on them. I argue here that the SA is more prone to occurrence bias than the PBA. While both methods might be equally affected by certain events becoming less likely, the ability of the PBA in producing results related to changes in the frequency of classes of events provides it with explanations and expectations for such a scenario, making it less prone to occurrence bias. This is not the case for the SA, since it does not focus on changes in the frequency of events. While the SA might be able to argue for single cases that future analogous cases might become less due to the effect of CC, these results are not generalizable to measure the changes in frequency for certain events. Thus, the SA is more prone to occurrence bias than the PBA.

Another form of selection bias is *choice bias*. It is the idea that researchers choose events that tend to have a specific aspect. The report (ibid) mentions that the aspect here can be an increase in the likelihood of anthropogenic forcing. Meaning that scientists tend to be more interested in such events. But this bias is equally true for both approaches. But I argue that while the previous condition can be equally true for both approaches, there is another condition that the SA is selective about, which is the relation of the EWE to thermodynamic and dynamic factors. As mentioned earlier (section 2-2), the PBA does not distinguish between providing attributional analysis for EWEs based on climatological factors, but this is quite the opposite for the SA, since for them to provide results, the EWE needs to be highly related to thermodynamic factors of the atmosphere in such a way that dynamic factors are either not involved or so small that their effects can either be negligible or conditionalized upon. This fact makes the SA more prone to choice bias, as it further specifies the events they choose for investigation.

The last selection bias I will be talking about here is *type bias*, which is to choose certain definitions or geographical regions that are more addressable by the methodology used. Although both methodologies can be prone to this bias, I argue here that the PBA is more prone both in terms of event definitions and geographical regions. The reason for the former is that, unlike the SA, the PBA needs to define events in terms of classes (this is discussed in the previous chapter). While they might be able to do so for certain events, not all the events might fall under the same definition. Even if certain definitions might be fitting, it does not necessarily mean that they would provide robust results across different definitions. Considering that the focus of the SA is on single events and each one is considered unique, they do not have the same problem. In terms of geographical limitation, I mentioned in the previous section how the PBA is highly reliant on available historical data for an event. Given this reliance, it could potentially lead many researchers to focus on regions in which such data is available to produce more quality results, making many other areas left out (more on this in section 4-1). The same is not true for the SA considering that it does not rely as much on available historical data, so it is more compatible with different regions of the world regardless of this factor, leading to more diversity of results and more inclusivity of different geographical areas than the PBA. Overall, the PBA is more prone to type selection bias than the SA.

3-5 Value Preferences and Interpreting Results

In section 2-1, I discussed the distinction made between the thermodynamic and dynamic aspects of the atmosphere. These two factors play different roles in the formulation, continuation, and severity of an EWE. Thermodynamic aspects are the robust part between the two with the uncertainties here being one of a quantitative nature and producing high-confidence results. Both approaches have no issues when it comes to these factors. However, the point of disparity concerns the considerations each approach has regarding the dynamic aspects of the atmosphere, and it is where the main discussion of this section takes place.

The PBA treats both types of factors in EWEs in the same way. They argue that the dynamic aspects of the atmosphere play a major role in many EWEs, and to provide a robust attribution, one needs to include all the factors relevant to an EWE. In such cases, one would include all the known factors that play a role in an EWE to get the probabilistic results. But, such a focus on representing all the aspects comes at a price since (as it is discussed in section 2-1) the inclusion of the dynamic aspects leads to different results across the models, and it makes way for deep uncertainties along with decreasing the confidence of the results attained.

But the SA has different considerations in this regard which is why this issue could be regarded as the departure point between the SA and the PBA. Advocates of the SA argue that the way these results are obtained is an area of concern for producing any actionable results in the field of climate science (Shepherd 2019). They argue, in the spirit of the Thermodynamicists, that the way to achieve such information is by focusing on the aspects of the climate which are more certain to us, instead of focusing on integrating aspects that lead to deep uncertainties. With this, advocates of the SA are setting aside the dynamic factors in favor of thermodynamic aspects and trying to only focus on events that are temperature-related than dynamic-related. The idea that this approach only works for temperature-related events or events with less dynamic influence is something also admitted by the advocates of the SA since they also mention this limitation of their approach. But, they argue, that this proves to be very effective in dealing with the events lying within their boundaries as well as providing more confident results than those provided by the PBA (Shepherd et al. 2018).

The different considerations towards these factors have been a source of major concern for the PBA, and it served as a reason for questioning the scientific credibility of the SA by the advocates of the PBA (Lloyd and Oreskes 2018; Winsberg et al. 2020). I divide this issue and its implications into three subsections. I will be discussing each one of them in the scope of this section.

3-5-1 Overestimation vs. Underestimation

One of the major debates regarding the scientific credibility of the SA is related to the issue of whether these methodologies are prone to overestimating the effects of CC or underestimating it. And, whether the latter is better than the former or vice versa. In the following subsections, I will explain these debates along with the justifications provided by each methodology for the way they argue about this debate.

3-5-1-1 Proneness to Error

One of the main problems why the advocates of the PBA argue against the SA is that it is prone to overestimate the effect of CC on EWEs. But, what do they mean by that? And, why is this a problem?

Type 2 Error, which in this sense is the same as overestimation or a false positive, is for the methodology to argue that a certain property or factor exists or contributes to another while in reality, that is not the case. What is meant by this in this context is to say that the SA can attribute certain EWEs to CC while it might not be the case that CC contributed to that particular event or at least not as much as the SA argues that it does. In other words, if the SA advocates that CC impacted a certain EWE by a factor X, being prone to overestimation means that the methodology used can sometimes misread these factors as higher than they are, so the true results are lower than the result obtained by the approach.

The reason for such proneness relates to the discussion earlier about the methodologies' considerations regarding the climatological factors (discussed in section 2-1). The SA considers the thermodynamic aspects of the atmosphere to be the factors that are to be focused on in investigating an EWE, and set aside or minimally conditionalize upon the dynamic factors. But advocates of the PBA argue that ignoring the dynamic factors plays an important role in these results attained by the SA, and they argue that it can be criticized both on the level of usage and its effects on thermodynamic factors. For the first concern regarding the usage, one can argue that the fact that the SA conditionalizes on these dynamic factors is problematic since the climate is not a partially stationary system in which some factors can stay the same regardless of changes in other factors. But, this issue can be disregarded on two bases. The first one is that the studies done by the SA are single event attributions, meaning that in many cases the temporal duration of these events is not long enough for observing drastic changes from the conditioned boundaries, and the second one is that the PBA uses conditional attribution in many ways as well, for example conditionalizing on SST (NAS 2016). Thus, even though there are concerns that conditionalization might not be as accurate in representing the climate system as unconditional attributions, these concerns are a double edge sword affecting both methodologies. As for the second concern, which is regarding the effect on thermodynamic factors, it is the case that in many instances the dynamic factors can play a role in acting in the opposite direction of the thermodynamic factors and mitigating the overall effects of thermodynamic factors (Shepherd 2019). Since the SA's focus is on thermodynamic aspects, it would likely miss on such mitigatory effects and estimate the overall result as higher than the actual result, falling prey to overestimating the effects of CC.

At the same time, the advocates of the SA argue that the PBA is prone to underestimating the effects of CC on EWEs. In his research, Trenberth (Trenberth 2011; Trenberth et al. 2015) argues that the reason why the PBA is prone to underestimation is because it starts from a null hypothesis of 'no effect' (CC did not affect the EWE). Providing a statistically significant result (exceeding 95%) to disprove this null hypothesis of 'no effect' is very hard to achieve in the context of climate science. Trenberth (2011) himself argues that the null hypothesis should be the other way around, i.e. the idea that there is a human influence should be the null hypothesis to disprove with significant results given the IPCC reports (IPCC 2007). He argues that the reason for this is that the sources of uncertainties in the observational records and models transfer into underestimating the human influence, something that would not be the case if perfect data and models were to be present (Trenberth 2011, 927) which is hardly the case.

Although one side is primarily represented as being prone to overestimation and the other to underestimation, it is important to understand that it could be the case that the reverse of this is also possible. As mentioned in section 3-3, one issue that the PBA might fall into is the estimation of p_o for a counterfactual world with no or less climate forcing. As discussed there, the misestimation here could lead to either an overestimation or an underestimation depending on the circumstances. Another reason that might lead the PBA to overestimation lies in the selection of a small group of models in an ensemble, since if, for example, the ensemble were to only include zero or positive changes while the true effects were to be a decrease in the occurrence of the probability of an event, then this would count as an overestimation (Garcia-Portela and Maraun, 2023), but this might be compensated by the inclusion of more models to provide more robust results.

On the other hand, the SA can also be prone to underestimation. While I mentioned earlier that in many cases the inclusion of the dynamic factors mitigates the thermodynamic changes, it is important to know that the reverse case could also be true, meaning that the dynamic effects could worsen the impact of an EWE. In such a scenario, while the SA approach only provides results based on thermodynamic factors involved, in reality, the dynamic factors also contributed to worsening the EWE, thus making the attributional result provided by the SA as underestimating the effects of CC (Garcia-Portela and Maraun, 2023).

But setting that aside, the proneness of the SA is one of the reasons why the scientific credibility of the SA is questioned by the PBA (Lloyd and Oreskes 2018). But, why does this issue entangle with the question regarding the scientific credibility of a methodology? And, what are the philosophical bases for these arguments? I will talk about this in the next subsection.

3-5-1-2 Error Type Preferences: Which Approach is Better?

The argument regarding the scientific credibility of the SA by being prone to overestimating the effects of CC is one of the core conflicts between these two methodologies and many debates surrounding which one should we prefer over the other (Stott et al. 2016; Lloyd and Oreskes 2018; Shepherd 2019; Lloyd and Shepherd 2020; Winsberg et al. 2020). One might ask here:

Why is this a relevant concern at all? And if so, which one should be preferred between the two? The issue here concerns the scientific credibility of a methodology. Advocates of the PBA argue that in most scientific disciplines Type I Error (underestimation) is not as problematic as Type II Error (overestimation), and given that the SA is prone to overestimation while the PBA is prone to underestimation, then the PBA is scientifically more credible. This common approach of preferring an error type over another can be titled here the *Symmetrical Approach to Error* which is simply the idea that Type I error is preferred over Type II error, or in this context, underestimation is more favorable to overestimation. The SA advocates argue against this form of favoritism by arguing that even if in many disciplines of science it is the case that underestimation is preferred over overestimation, it does not make it applicable to all disciplines in science. They argue that favoring one type of error over another is context-dependent (depends on the risk involved), and it can be the case that in certain contexts overestimation is preferred over underestimation. This opposite approach of the one mentioned earlier is called the *Asymmetrical Approach to Error* (Allen 2011) which is the idea that Type II error is preferred over Type I error, and in this case, overestimating the effects of CC is better than underestimating the effects of CC.

But, if the preference towards error types is context-dependent, as argued for by the SA advocates, what is the basis for arguing that in this context overestimation is better than underestimation? They base their argument on the idea of the risk comparisons between the two options. I can further illustrate this with an example: Suppose we are making clinical trials for a new drug. In this context, a false negative (underestimation) would mean to underestimate the effects of a life-threatening drug by giving it to the patient, therefore risking the life of the patient while a false positive (overestimation) would be to overestimate the risks of a safe drug, but eventually not risking the life of a patient. It is clear here that in such contexts prioritizing the lives of patients requires us to prefer overestimation over underestimation, proving the context-dependency of error type preferences.

The advocates of the SA argue that the context here is very similar to the clinical trials example. They argue that in the context of EWE Attribution Studies, the phenomena of concern, namely EWEs, are also life-threatening and damage-causing in many contexts. In such a circumstance, if we were to overestimate the effects of CC on changes in EWEs, we would be overprepared for an impact and thus would eventually lose funds (the argument that overestimation leads to misallocation of funds) but at the cost of saving human lives, while if we were to underestimate the effects of CC on changes in EWEs, we would be underprepared for the impact, and that would be at the cost of many human lives. Thus, just like in the earlier case, prioritizing the safety of human lives would justify our appeal to favor overestimation over underestimation in this context (Lloyd and Oreskes 2018).

The argument provided by the advocates of the SA is convincing and on point. The cost of saving lives is far greater than misallocating funds, and one can justify favoring overestimation over underestimation, diminishing the claims made by the PBA. But there is a potential aspect of this argument that can also be used by the PBA to argue in favor of their case. The way the advocates of the SA interpret the part about the misallocation of funds is the idea that we would be over-prepared for a specific EWE. It is true that in such cases funds can be taken for granted for the sake of human lives. But, what if we interpret the same argument differently; that is to say, to be overprepared for a certain risk might be overlooked in terms of financial losses, but to be on the verge of multiple risks instead of one (as is the case in reality), overpreparedness can indirectly lead to life-threatening consequences since the misallocated funds could have been used in other projects to mitigate the hard effects of other risks. Let's illustrate this with an example: suppose that there is a limited amount of money to be spent on risk preparedness, but there are multiple risks (multiple forms of EWEs) threatening the same location (not necessarily at the same time, but close). In such situations, overestimation might lead to overpreparedness in many areas in which these extra funds could have been spent to better prepare for the other risks. By integrating a more realistic element to the argument, namely that there can be multiple risks of different EWEs, favoring overestimation might seem to fall under the same issue it wanted to avoid, which is life-threatening consequences.

The point of this argument is neither to illustrate that PBA is still on point when it comes to error-type preferences since it is still not enough to support such a conclusion nor can it fully undermine the strength of the argument provided by the SA approach for their case. In fact, such discussions regarding whether misallocation of funds or underpreparedness is a worse life-saving strategy can further be extended in the form of trolley problems investigating more potential outcomes determining the fate of the debate which is beyond the aim and scope of this research. But, the main point to be taken here is that both approaches have justifiable and defendable grounds in supporting their adherence to preferring a different type of error over the other.

3-5-2 Communicating Results and Underlying Values

It was discussed in the previous sections how both methodologies approach EWEs in different ways. What I will investigate in this section is how each methodology communicates its results and what value preferences underlie such choice of communication.

As discussed before, the PBA's way of looking at EWEs is class-specific and the results produced are probabilistic, meaning that the results communicated through using this approach would be like 'The probability of a drought in X geographical location has increased by A amount compared to a world with less or no anthropogenic forcing'. These sorts of results reflect the way the PBA defines and mathematically approaches EWEs. They have been titled by the advocates of the SA as results that value reliability and probability (I will discuss what is meant by these terms further below).

It has also been clear from the previous sections how the SA deals with EWEs differently trying to build causally plausible storylines of how an EWE came to form. The way the SA communicates their results is more causal than the PBA, particularly specifying which factors CC contributed to and how those led to a change in the severity of a particular EWE. An example is Trenberth's (Trenberth et al. 2015) paper regarding the Boulder Flood of 2013, where the authors mention that CC led to the rise of Sea Surface Temperature by 1°C over the normal August SST of the area, leading to an increased amount of moisture in the air and rainfall which affected the severity of the flood (ibid).

Shepherd describes the results produced by the SA as preferring informativeness and plausibility over reliability and probability (Shepherd 2019; Lloyd and Shepherd 2021). Shepherd explains the meaning of these terms in the same paper. In his definition, the terms reliability and informativeness are related to the underestimation vs. overestimation debate, where reliable results mean the PBA's results of full consideration of all the factors involved. This is more likely to capture the true value but might lead to false negatives while informativeness means SA's more informed analysis of the EWE with more confidence in the results that might lead to false positives. To be reliable (specified to EWEs), in Shepherd's terms, means to prefer attaining true values and avoiding false alarms while to be informative means to prefer attaining confident results and avoiding missed alarms (Shepherd 2019). The other two terms, probability and self-consistency, are related to the nature of the results where the SA produces results in terms of the plausible causal structure of the EWE while the PBA produces results in terms of probabilistic estimations (ibid).

With these value preferences outlined above, and as a response to the previous debate regarding the scientific credibility and value-freeness of one approach over the other, Shepherd continues to argue that there is no value-free climate science since both approaches differ based on having different value preferences, and it is not that one of them is value-free while the other is not. Thus, he concludes that there is no objective basis for the PBA to argue that their results are a more *objective* version of climate information compared to the results produced by the SA (Shepherd 2019).

Another point that needs to be mentioned here is regarding the nature of the results communicated by each approach regarding the way we think about risk. Shepherd (Shepherd et al. 2018) classifies two types of risk information in this context: *semantic* (knowing the facts) which is the type of information provided by the PBA when providing results on changes of frequency of a class of EWE (i.e. 'this is a 1 in a 1000 year event'), and *episodic* (reliving events) which is the type of information provided by the SA approach when asking how such an EWE would look like in the future (i.e. 'this happened before, so how would the next event be like'). These two different forms of results are a product of the previously mentioned underlying values of each of these two methodologies. I will turn back to the relevancy of such an issue in the next section.

3-5-3 The Pragmatic Debate: What Stakeholders Want

In the previous sections, I analyzed some of the epistemic and value-related debates regarding both methodologies. In this section, the question I am aiming at is related to how pragmatically both approaches are evaluated, especially in the context of the stakeholders' needs.

As mentioned in the previous sections, the PBA advocates prioritize reliability and consider it to be the decisive factor in terms of scientific credibility since it tries to integrate all the relevant climate variables of an EWE. The epistemological limitations and the value debates were also discussed in the previous sections. But how can this be analyzed pragmatically in the context of decision-making purposes?

In many cases, such reliable results aiming at full consideration of the factors related to an EWE are essential for a full-scale picture of the phenomenon at hand. Advocates of the PBA approach argue against the SA approach by saying 'attribution analysis aiming to answer questions of most relevance to stakeholders should … follow a holistic [FAR] approach in order to prioritize an understanding of the changes of overall risk rather than the contribution of different factors (Otto et al. 2016)' (Eden et al. 2016, p. 9). But this point here has been criticized by Lloyd and Oreskes for two reasons. The first reason is that it assumes that scientists know what issues are relevant to stakeholders, which is not true in many instances and different stakeholders might be interested in different things (Lloyd and Oreskes 2018, p. 316). The second reason is important here since not necessarily all stakeholders might be interested in knowing how the frequency of a class of EWE would change given changes in anthropogenic forcing. Quite the opposite, many stakeholders might be interested in knowing what factors contributed to their local EWE to have better management plans for future analogous events. This aspect of the SA approach in providing such information valuable for future management plans is important for many stakeholders (Shepherd 2016; Shepherd et al. 2018).

Another argument that can be made in favor of the SA approach is the idea that, although the PBA results are of importance to decision-makers, that does not necessarily mean it is the only type of information relevant to decision-making purposes. This idea of having only one source of information has been criticized by Shepherd through the idea of the *availability bias* which is the idea that people are more responsive to episodic knowledge (relating to their experience) than semantic knowledge (quantitative knowledge) even if there is a large amount of the latter available (Shepherd et al. 2018). So, the SA's way of building knowledge, as described in the previous section, pertains more to people's understanding of risk than the PBA. It is also more effective for planning against future analogous EWEs. These are the two reasons why the SA advocates argue for their results being a better form of *actionable climate information* than the PBA.

Another aspect of the SA is that it is less reliant on available historical data compared to the PBA (see section 3-3). The reason why this aspect can be of pragmatic importance is specifically related to the geographical areas in which there is either less historical data or the available historical data cannot be fully translated into the current required modeling or observational approaches to produce reliable results. At its core, the PBA is a statistical approach that relies heavily on available historical data for a specific region to be able to produce reliable results. But, this requirement leads to excluding many geographical areas for their lack of such requirement which in turn leads to type bias (see section 3-4) and many years of compensatory research and data collection (given it happens) to be able to produce reliable results in the long run. With the urgency of the CC issue, the task of understanding EWEs with the PBA for those regions might either be impossible or hard to achieve. But, the SA could be a better alternative for such regions. This is because the SA does not have the same historical data availability requirement, and this makes it easier to produce research on EWEs for such regions. Thus, it provides us with a form of understanding about the EWEs related to those areas until and after the PBA can do reliable research, leading to a more inclusive science in which no areas are left out due to their late access to climate data collection and measuring instruments.

Although I have discussed many pragmatic advantages of the SA for stakeholders, it is also important to emphasize here that this does not mean PBA does not have its pragmatic arguments to put forward. One of the limitations of the SA approach, as it was discussed in section 3-4, is that it is prone to occurrence bias since it does not make statistical nature research into EWEs, nor that it aims to do so. This information identifying which EWEs would be more or less frequent in the future is interesting for many stakeholders who are specifically aiming at better adaptation and mitigation strategies related to the preparation for possible future EWEs.

Added to that, one can argue that the PBA does not have the limitation of only producing results for specific types of EWEs (as is the case for the SA in providing results only about highly thermodynamic events) but can provide results regarding many more forms of EWEs (see section 2-2 and section 3-5). This aspect of the PBA makes it more widely applicable than the SA approach, given the availability of the historical data related to that class of EWE under question.

Overall, though both approaches have different limitations as well as different value preferences when it comes to researching EWEs, each one has pragmatic aspects which correspond to different stakeholders' needs and requirements.

To sum up, I have analyzed the different aspects of each methodology in their way of handling EWEs, including the way they frame and define EWEs, the type of attributional methodology they use, their reliance on historical data, their proneness to different selection biases, and their value preferences and its implications. In the next chapter, I will investigate the possibility of a pluralistic approach to this issue and see if Pluralism would be the approach to have here. Based on the answer to that question, I will also investigate what form of Pluralism would be compatible here based on the conclusions I will draw from these comparisons.

Chapter Four: Pluralism

In the previous chapters, I have explained the two main methodologies of EWE attribution studies and outlined many of the core differences between both methodologies regarding their approach to studying EWEs. I have also related many of these discussions to some of the philosophical discussions related to values in science and scientific credibility measures. The core question I will be investigating in this chapter is the topic of methodological Pluralism for EWE attribution studies. The question of concern is 'What form of Pluralism, if any, should be used as a basis for approaching EWEs in the field of EWE attribution studies?' For such an answer, I will be relying on the results of the exploration in the previous chapter to serve as a basis for investigating the plausibility of a pluralistic approach in the field of EWE Attribution Studies.

4-1 Discussion and Evaluation

In the previous chapter, I outlined some of the essential differences between the way both methodologies approach EWEs. In this section, I will make use of the results of each section of the previous chapter to further explore the possibility and the nature of the Pluralism that can be attained based on those results.

When it comes to defining and framing events, both approaches use intrinsically different ways to perform such a task along with their different limitations. The PBA uses class assignments of each EWE for statistical-purpose research while the SA treats each single EWE as being unique from other EWEs (apart from those which are causally analogous to its storyline). This is an important point in the discussion since it assumes that each of these methodologies works intrinsically differently from the other, and the limitations each one of them has (PBA: class-assigning limitations, and compound events. SA: ungeneralizability, and inability to use non-analogous similar event records) come from this intrinsic difference in the way how they define events. Thus, it can be concluded here that these two methods are different in such a way that combining them would be a hard task to achieve since these differences are intrinsic to the way they approach the issue.

The type of attribution measures (FAR and PR vs. Causal Chains) is also a core difference between these two approaches. The PBA uses the FAR which can tell you the amount of contribution of one causal factor (in this case CC) to the change in the frequency, and PR which is a factorial measure of the increased frequency of EWEs. These two approaches are used when doing coarse-grained analysis of complex systems, in which the results can give you the amount of contribution of the factor to the change without providing you with the causal details of how such a change came to be. On the other hand, the SA builds a storyline composed of the plausible factors that contributed to an EWE, thus giving you the details of how CC affected the factors that led to changes in the severity of the event. While each measure has its limitations (FAR: P_o being close to zero, interpretability of negative and zero results, contribution of other factors unaccounted for, SA: No inclusion of the dynamic factors), the difference in the way they approach EWEs leads to providing different types of results specified to work for different stakeholder's needs.

The practice of climate science has changed over time, and with this change, the requirement of what counts as reliable data for climate monitoring processes has also changed. There have been many developments since the early days of collecting climate information. Now, there are demands on recording new aspects of the climate system, the nature of recording the variables has changed, as well as the instruments and technologies used for this purpose have changed as well (IPCC 2013, Chapter 2). As discussed earlier, the SA might be at a considerable advantage here since it does not need as much historical data as the PBA (as discussed in section 3-3). This is especially important for the development of more inclusive climate science, especially more inclusive EWE studies. The reason for such an argument is the fact that the PBA heavily relies on available historical data for providing reliable results. This is a barrier for many areas of the world that have been or are still underdeveloped in terms of model-adequate data collection or climate data collection. For such places, this requirement proves to be a barrier in terms of knowing the nature of EWEs in such locations, and also making them a less attractive area for EWE studies given that the lack of such data makes the results less reliable compared to more developed areas (choice bias, see section 3-4). These results also create further obstacles for decision-makers in terms of implementing plans based on results with lower confidence. And even granting that those areas would engage from now onwards in intensive practices of data collection, the outcomes of such a quest for reliable results will only blossom in the long-term future since trend detection requires data from a long time and creates a gap period of unavailability of reliable information for climate-related decision-making processes. But, the presence of the SA proves to be very crucial in these contexts since it would not only help in terms of providing reliable results on EWEs for such transitionary phases of those contexts, but the causal nature of the approach itself is a very attractive option for many decision-makers in terms of planning and mitigating the effects of EWEs. Considering that it does not rely as much on available historical data, it provides an opportunity for the areas which are less developed in terms of climate data collection to be able to participate in the practice of science and enables many scientists to not refrain from conducting research in those areas. Thus, having diverse methodologies creates a more inclusive environment for science which is a target value for climate science, given that the risk of CC affects us all together.

In terms of selection biases mentioned in the previous chapter, I argue here that the availability of different approaches to studying EWEs might mitigate the effects of those biases leading to more robust and insightful results. For instance, since the SA approach does not provide any results on the changes in the frequency of EWEs, it is more prone to missing out on EWEs that are becoming less frequent and falling for such a bias. But through the reliance on information from the PBA about the changes in the frequency of such events, the SA can become more informed of such changes and consider them when planning to study an EWE. A second area in which bias can be lessened is occurrence bias. Given that both methodologies have choice preferences in terms of which EWEs to study, having different methodologies providing studies on the areas in which the other does not prefer leads to more events being studied compared to one methodology alone. As I discussed in section 3-4, the SA is more prone to this bias since it can only focus on highly-thermodynamic events, having a diversity of methodologies that can provide results on other EWEs, such as the PBA, provides us with an opportunity to investigate those EWEs as well. The last example concerns type bias. As discussed (in section 3-4), since PBA is more prone to this bias due to its reliance on defining events in classes and the availability of historical data, the SA would prove to be a helpful alternative for studying events that do not fall under class categories (such as compound events) or EWE of regions with less historical records (as discussed before). Thus, the point to be made here is that having a diversity of methodologies in EWEs would provide a good opportunity to fill the gaps that one is prone to by the other and mitigate the proneness of these biases as an overall result.

The last part of the issue is regarding value preferences. In the last section of the previous chapter, I have outlined how climate information is value-driven, and the types of preferences determine what form of information would be favorable under what sort of circumstance. As discussed before, the PBA's inclusion of dynamic factors for reliable results comes at the price of less confidence and less knowledge regarding the causal nature of the EWE while the SA's results prioritize confidence and plausibility over reliability at the cost of only focusing on thermodynamic-related EWEs. This supports Shepherd's suggestion that the debate here is not one of 'which of these approaches is more objective/value-free?' because none of these are value-free. I have also argued about how each methodology prefers different symmetries to error types, and how each can also have a valid basis for their preferences which can be both justified. And, I have also outlined how the results produced by each methodology, which can be different in form from the other's results, are desired by different stakeholders for different purposes. The conclusion that can be drawn from these is that there is not one argument for scientifically favoring a methodology over the other, and the grounds for favoring error types is context-dependent, where both sides can argue in favor of their preferred methodology based on different arguments.

Thus, based on these comparisons, I have identified the following characteristics to use as a possible guide for our approach towards Pluralism: 1) The differences of these two methodologies in terms of defining and approaching EWEs are intrinsic differences with a hard possibility of combination, 2) each one uses different attributional measures that leads to different forms of results that satisfy different stakeholders needs, 3) the SA approach is less reliant on available historical data than the PBA, thus a diversity creates grounds for further inclusivity of climate science to other less-developed areas, 4) both approaches are prone to selection bias, some affecting both in different ways, others affecting them individually, thus the diversity of methodologies used would lead to results being less prone to some types of selection bias, and 5) the climate information we get out of the two approaches are not value-free, and each has their own defense for preferring one type of error over the other.

In the next section, I will examine the potential of a pluralistic approach based on the conclusions I arrived at by comparing the two methodologies.

4-2 Pluralism? And What Form?

In the previous section, I outlined the major conclusions that I reached from the comparisons made between the PBA and the SA. I have also established the argumentative ground each methodology has in favor of the way they approach EWEs. The important question that needs to be asked here is the following: Are there grounds for establishing Pluralism in EWE attribution studies? And if so, what would it look like?

It is important to note that the form of Pluralism I am investigating here is based on a *pragmatic* approach to Pluralism, that is to say, the reason we have Pluralism in this context is that both approaches are indispensable in terms of the pragmatic benefits they provide(Van Bouwel et al. 2021), whether it may be different forms of explanation, or different results fitting different purposes, etc. In other words, if any of the methodologies can provide us with an indispensable form of information, explanation, etc., which the other methodology cannot provide, it is enough reason to keep the methodology and argue for establishing Pluralism in that context.

There are two dimensions of the first question which I will be answering by relying on the conclusions I have attained from the previous comparison. The first one is related to the equal arguments each method provides as a justificatory base for their significance and presence. I argued in the previous sections how both methods have enough justificatory grounds for their approach as well as their value and error type preferences. In such a scenario where the presence of both approaches is equally justified, Pluralism is the approach to take here. The second is related to the pragmatic relevance of each methodology. As it has been argued in the previous sections, the results provided by both methodologies are different in form, each fulfilling different stakeholders' informational and explanatory needs. Thus, Pluralism here would be beneficial to fulfilling different stakeholders' needs. Added to that is the idea that has been mentioned about how such diversity is beneficial for more inclusivity and less bias. Based on these reasons, Pluralism here would be both a justified and a beneficial approach to take in this context. But what form of Pluralism?

The usage of multiple methodologies to investigate the same phenomenon, whether it is by approaching it in the same way or in different ways, is not a new thing to science. Many disciplines of science have pluralistic approaches to methodologies when trying to investigate the phenomenon under question. An example would be the study of human behavior in which multiple methodologies can try to provide an explanation for the same phenomenon, such as an evolutionary account, a cognitive account, a biological account, etc. Apart from the presence of Pluralism across science, many have categorized these forms of Pluralism either as a description of the scientific discipline or as a proposed mechanism for scientific inquiry in the specified field. Based on (Mitchell 2002, Mitchell 2004, and Van Bouwel 2014), some examples of such pluralistic approaches categorized in terms of the independence they provide to the constituent disciplines and theories include:

1- *Explanatory Reductivist Pluralism*: Though it is not mentioned by Van Bouwel and Mitchell, this is a form of eliminativist Pluralism in which scientists tolerate the usage of a reduced theory because total reduction has not been established yet (as with statistical thermodynamics and general thermodynamics). This form of Pluralism is temporary and has a reductionist unificatory aim.

2- *Competitive Pluralism* (*Moderate* or *Temporary Pluralism*): This is one of the most common views about the pluralistic approaches in science in which you have multiple theories or research programs competing for the explanation of the world as a form of a rational strategy against empirical uncertainty until one of these theories prevail against the others, thus the Pluralism is eliminable when that condition is satisfied (Mitchel 2002; Van Bouwel 2014). The difference between this form of Pluralism and explanatory reductionism is that in the latter unification is achieved through reduction, while here it is achieved through elimination. Examples of such views are common in science, like the Darwinian and Lamarckian theories of inheritance.

3- *Integrative Pluralism*: This is a form of Pluralism that lies between isolationist and reductive approaches by recognizing the independence of scientific disciplines, but at the same time arguing that integration of different approaches would end up in a unified understanding of the phenomenon. Mitchell's Integrative Pluralism could happen at three levels: *mechanical laws* which is using two laws from different disciplines to account for the same phenomenon, *local theoretical unification* a non-grand unificationist form of bridging theories at a local level of the domain, and *explanatory Integration* using multiple explanatory approaches to understand the same phenomenon (Mitchell 2004). An example of the first is to use both gravitational and electromagnetic laws to account for resultant motion. An example of the second is a form of unification of multiple theories of the evolution of the universe in cosmology to account for one particular phenomenon. Also, an example of the third one is to use different approaches to account for the ecosystem of a lake involving multiple organisms and multiple organismic interactions.

4- Interactive Pluralism: This approach to Pluralism is an intermediary position between the isolationist approach and the integrative approach since it also advocates for and motivates interactions between different theories (as opposed to the isolationist approach), but at the same time argues that satisfactory understanding can be achieved without the need of integration (as opposed to the integrative approach) (Van Bouwel 2014).

5- *Isolationist Pluralism (or Compartmentalized Pluralism)*: This is a form of Pluralism that recognizes the different theories explaining the same phenomenon, and recognizes their independence with little or no interaction between the fields (Mitchell 2004). This form of Pluralism is counter to reductionist unification accounts that try to reduce the explanation provided by one discipline into the explanation of another. An example here would be how different approaches such as psychoanalysis, behaviorism, evolutionary psychology, etc., can each provide different explanations for human behavior in psychology without interacting with one another.

6- *Epistemic Anarchism* (or *'Anything Goes' Pluralism*): This is an extreme form of Pluralism endorsing all forms of explanations as valid in the absence of foundational standards of justification in science (ibid).

In the case of EWE attribution studies, the two different approaches I have been analyzing, namely PBA and SA, have their own set of argumentative claims for why their presence in the field is justified, and both approaches provide us with different forms of climate information that can be of potential usage by different stakeholders. Given the fact that neither has there been any reduction at any level, nor any attempts to do so nor do their intrinsic differences provide potential for such an attempt (unless in the future they are heavily adjusted for such an attempt), *Reductionism*, the action of reducing one methodology into another, wouldn't be an option in this context. And even if such an adjustment were to be made, the methodologies would change so much that they would become two intrinsically new methodologies than the ones we are currently talking about, making it a discussion about other methodologies that are different from the ones we are currently investigating. Thus, I can argue here that Reductionism would not work here, and we can go on with establishing Pluralism in this context.

After establishing the basis for Pluralism in this context, it is also important to understand what form of Pluralism would best fit the following instance. Among the mentioned approaches to Pluralism, one can easily eliminate Explanatory Reductive Pluralism based on the same argument previously provided for eliminating Reductionism. The idea behind Explanatory Reductive Pluralism is a temporary coexistence of two methodologies until eventually one would be reduced to the other. But as I have mentioned earlier, the differences between these two approaches are intrinsic and they also provide different types of results about the phenomenon under question. Added to that is the idea that none of the approaches show any potential for reduction unless they are heavily adjusted which would turn them into two new distinct methodologies from the ones at hand. These two points are against the required conditions for Explanatory Reductive Pluralism. Thus, this option would not be the most compatible approach for our context.

The two options of Competitive Pluralism and Epistemic Anarchism can also be eliminated. The reason why the former does not work for this situation is that in Competitive Pluralism, the competing approaches need to provide different answers to the same question, like how wave theory and particle theory of light provided different answers to the same question regarding the nature of light. But, we do not have that situation here since these two approaches answer different questions regarding the phenomenon at hand. One approach, namely SA, answers questions like 'How did CC affect the severity of that particular EWE when all other factors are conditionalized?' while the other, namely PBA, answers questions about 'What is the risk of a specific class of EWE under current forcing compared to no forcing?' (Winsberg et al. 2020). Thus, with the questions being different from each other, and the framing and nature of results of the two approaches also being different from each other (see section 3-1 and 3-2), one can argue that the SA is not an alternative way of answering the same question pursued by the PBA, which is a fact that has also been iterated by the advocates of the SA (ibid). As for Epistemic Anarchism, the satisfactory condition for such a case would be when justificatory standards are absent. But as I argued in section 3-5-1, it is not the case here that we lack justificatory standards, but rather that the justificatory standards of both approaches are equally valid for them to be considered scientific. Thus, it is not the case that 'anything goes' in this situation since there need to be justificatory grounds for an approach to be considered valid, and therefore Epistemic Anarchism is not suited to describe the Pluralism employed here.

What remains among the pluralistic approaches outlined above are Integrative Pluralism, Isolationist Pluralism, and Interactive Pluralism. Considering the intrinsic nature of the methodologies I mentioned in the previous chapter (section 3-1), one compelling pluralistic approach for these two methodologies seems to be Interactive Pluralism in which both methodologies contribute to the understanding of the phenomenon without the need for integrating one methodology into the other. While that might seem like a plausible pluralistic approach to pursue in this context, Shepherd (2016) has a paper in which he advocates for a common framework between these two approaches that might seem to make us reconsider whether Interactive Pluralism is the best approach to have here. But one option Shepherd's framework eliminates is the option of Isolationist Pluralism. This is because Shepherd argues that these two methodologies, while intrinsically different, are not mutually exclusive. Since a core requirement for Isolationist Pluralism is the total exclusivity of two methodologies, we can eliminate it as an option here and only concern ourselves with the two remaining options of Integrative Pluralism and Interactive Pluralism. In the next subsection, I will explore this approach by Shepherd and use it as a basis for choosing between the two pluralistic approaches that are fitting for our context.

4-2-1 Shepherd's Common Framework

Although PBA and SA are intrinsically different in the way they approach EWEs, Shepherd (2016) argues that they are not mutually exclusive and can be assigned into a common framework for analyzing certain EWEs. The way Shepherd argues for this common framework is through an analysis of change in the probability of an EWE given changes in the dynamic conditions. He provides the following equations for the common framework:

$$P(E) \;=\; P(E|D)\,P(D) \;+\; P(E|{\sim}D)\,P({\sim}D)$$

E means EWE, D being the presence of the dynamic condition, and ${\sim}D$ is its absence. If we were to assume small changes δ in the dynamic conditions, the change in the probability of the event would be:

$$\begin{aligned}\delta P(E) \;&=\; \delta[P(E|D)\,P(D)] \;+\; \delta[P(E|{\sim}D)\,P({\sim}D)]\\ &=\; P(D)\,\delta P(E|D) \;+\; P(E|D)\,\delta P(D) \;+\; \delta[P(E|{\sim}D)\,P({\sim}D)]\\ &=\; P(D)\,\delta P(E|D)\,[1 \;+\; \tfrac{\delta P(D)/P(D)}{\delta P(E|D)/P(E|D)}] \;+\; \delta[P(E|{\sim}D)\,P({\sim}D)]\end{aligned}$$

The P(D) δP(E|D) term refers to changes in the probability of the event assuming no changes in the dynamic factors which led to the event. What Shepherd argues here is that for cases where the dynamical changes ratio δP(D)/P(D), and the thermodynamic change ratio δP(E|D)/P(E|D), is small. Then this aspect can be negligible in those situations. He also argues that δP(D) is highly uncertain in small changes and better be assumed as zero in these small changes unless there is a ground for it. He also argues that if we were to assume that D was a necessary condition for the formation of the EWE, then P(E|~D) is impossible, therefore zero, and the whole term δ[P(E|~D) P(~D)] would be zero. Thus, what remains is the following equation:

$$\delta P(E) \;=\; P(D)\,\delta P(E|D)$$

What remains here is the same approach that is advocated by the SA approach which is the idea that we can conditionalize on the dynamic factors to achieve the results. What Shepherd tries to argue for here is that for dynamic-related EWEs where there are negligible dynamic changes, one can use the SA approach in the PBA to find credible estimates of δP(E|D) and thus credible estimates of the $\delta P(E)$ real-world probability conditions of an EWE which is the p_1 used in FAR and PR. But it cannot provide the same thing for its counterfactual counterpart, p_0.

This framework provided by Shepherd illustrates the possibility of integrating the SA, for limited types of cases, into the PBA models to provide more credible estimates and fewer uncertainties in the results. This form of integration very much aligns with the local theoretical unification aspect of Integrative Pluralism in which different theories are integrated and jointly modeled to better explain a complex process (Mitchell 2002). But, is this enough to establish Integrative Pluralism here?

4-2-2 Integrative Pluralism or Interactive Pluralism?

In the previous section, I outlined the framework Shepherd provides in terms of integrating the two approaches in specific contexts. The main question of this subsection is related to the most plausible form of Pluralism between Integrative Pluralism and Interactive Pluralism considering this framework and the details I discussed in section 4-1.

Here, it is important to emphasize that Shepherd's framework has a limited scope of application which is for events where the change in the dynamic factors are low that conditionalizing on them will not make a difference, and there, we can integrate the SA to provide more credible results. Thus, this form of integration has a very local scope with limited applications (this is also emphasized by Otto et al. 2020).

Given that Shepherd's account allows for integration, even at a local level, such a condition needs to be considered when choosing between the two possible pluralistic approaches. While it is true that the two methodologies, the PBA and the SA, are intrinsically different but can interact at different levels, our first intuition was to argue for Interactive Pluralism in this context. But the issue with Interactive Pluralism is that while it allows interactions between two different methodologies, it is fitting for situations in which these methodologies have distinct non-mutual boundaries. Under such strict requirements for non-mutuality, it would be hard to consider this as a fitting form of Pluralism here considering Shepherd's provided framework.

On the other hand, if we were to consider Integrative Pluralism as a plausible pluralistic approach in this context, it does not have the isolationist requirements that Integrative Pluralism has for the methodologies. Thus, it can accommodate different forms of integration. This feature of Integrative Pluralism is very helpful in this context since it enables the accommodation of Shepherd's common framework given that it is a form of integration. And, while we cannot maintain Interactive Pluralism even if there is integration at a local level between the methodologies, a form of interaction within Integrative Pluralism can still be maintained since there is no requirement for mutually exclusive boundaries here.

Though Integrative Pluralism seems suitable here, another aspect of the debate is that this form of integration described by Shepherd is not a grand unificationist project aiming at ultimately reducing one approach into the other. I mentioned in the argument against Reductionism that these two methodologies are currently incompatible for such a procedure. And, given that the only possibility for such a unification here would be under drastic changes, ultimately turning them into intrinsically different methodologies than the ones we are describing here, such a project would not be suitable in this context. Thus, a version of Integrative Pluralism is needed here that accommodates the non-unificatory nature of the integration.

Based on the previous consideration, one can consider an altered version of Integrative Pluralism which I title as *Non-Unificatory Local Integrative Pluralism*. This form of Pluralism can be considered a subform of Integrative Pluralism. The idea behind it is that there can be integration between two approaches on limited levels while maintaining the general interactions between the two, but at the same time, this integration is not ultimately aiming for a unification project and maintains the boundaries of both approaches.

Before concluding this approach to Pluralism, certain questions need to be answered regarding the nature of this pluralistic approach to better understand the conditions, boundaries, and applicability scope of this approach. First and foremost to explore whether this account of Pluralism can satisfy all the conditions I have assigned for a pluralistic approach to be. In terms of these conditions, I argue that 1) it maintains and respects the intrinsic differences of both approaches, 2) the different forms of results provided by both approaches are accommodated here in its context, it respects the diversity of approaches leading to 3) more inclusion and 4) less bias, and 5) it accommodates both approaches since each has its argument basis to argue for its scientific credibility. Apart from the mentioned conditions, it does satisfy the other requirements that have been laid out, such as providing the opportunity for integrating these approaches in limited cases while maintaining each one's boundaries against grand unification. Thus, in terms of satisfying all the conditions, I can argue that it fits the current context we have in EWE Attribution Studies.

A second question to ask here is regarding the conditions under which Non-Unificatory Local Integrative Pluralism operates and the limitations of this approach. As I argued earlier, the approach is considered a suitable approach in our context. But there can be certain limitations of this approach. The first one is that it currently only works between the two main methodologies, the PBA and the SA, in the field of EWE Attribution Studies. So, if it was the case that new approaches to studying EWEs were to come up at later stages of development, it does not mean that they can also be considered under this pluralistic approach. Given the nature of the new methodology, a different form of Pluralism can be reconsidered in such circumstances. But as for now, Non-Unificatory Local Integrative Pluralism satisfies the requirements that were currently laid out as targets between the PBA and the SA.

Another limitation of this approach can be its sensitivity to any drastic changes in these two methodologies. What I mean by a *drastic change* in this context is either a) changes in the local aspect of integration into a *non-unificatory universal integration* where these methods are integrated into one another at all levels, but such integrations still enable the methodologies to maintain their respective boundaries, or b) changes in the non-unificatory aspect of the approach to have a *unificatory aim*, eventually fully integrating one approach into the other for grand unification. If any of these changes were to occur, the approach of Non-Unificatory Local Integrative Pluralism would not continue to be the most compatible form of Pluralism, and other approaches, such as other versions of Integrative Pluralism for a) and maybe an Explanatory Reductive Pluralism for b), could be the compatible approaches to have. But I can also defend the position against such criticism since these changes, labeled as drastic, are not changes in the accidental aspects of the approaches, but at the cores which affect the identity of the approach itself. For example, either one approach or both approaches need to make dire changes in order to have a common definition or framing that can be integrated. Another example is for either one or both approaches to change their mathematical tools to accommodate the usage of one in the other in a *universal* manner, i.e. not limited to only specific cases or circumstances. And, such changes are in the core aspects of the approaches that make them totally different approaches from the ones I am currently describing. Thus, in such a situation, one would be looking at finding a compatible form of Pluralism for new approaches different from those I am currently mentioning. So, it does not affect our stance that Non-Unificatory Local Integrative Pluralism holds under the circumstances I am currently describing. And, since it is a totally natural practice in science to change the pluralistic approach to adapt when drastic changes occur, such as the changes in the nature of the Competitive Pluralism between the wave theory and particle theory of light before and after the double-slit experiment and the foundation of Quantum Mechanics, I can argue in the same spirit that the stance here holds under the current conditions for the current identities of the PBA and the SA, and it can be abandoned when such conditions drastically change.

I have so far discussed the issue of changing the local integration into a universal integration between the two approaches. But another dimension to consider would be the opposite of that, i.e. what happens if this local integration were not to work? In other words, what would our pluralistic approach look like if Shepherd's framework would no longer be a part of it? While Shepherd's framework has been used in scientific works (for example Cheng et al. 2018) providing no reason for why it should not work, it is still worth considering the hypothetical since it would have implications on the way Pluralism would be in our context. As mentioned earlier, Non-Unificatory Local Integrative Pluralism is integrative at a local scale, and this local scale might not necessarily be limited to one circumstance under which they can be integrated. There can be multiple circumstances under which the two methodologies can be integrated. In such a scenario, if it was the case that under one circumstance integration would result in a failure, Non-Unificatory Local Integrative Pluralism can still be maintained since there are other circumstances for which this integration still holds. But in this case, there is only a certain circumstance under which this integration is considered as possible which is the one described by Shepherd (described in section 4-2-1). Thus, the failure of integration on that specific level would mean that the integrative part of the Non-Unificatory Local Integrative Pluralism cannot be satisfied. Thus, what remains between the two approaches is merely interactions, leaving us with the Interactive Pluralism option that we reached before discussing Shepherd's framework. Thus, having even one circumstance in which integration occurs is a necessary condition for Non-Unificatory Local Integrative Pluralism to hold.

So far, what I have discussed relates back to the most compatible pluralistic approach in our context given the conditions I have provided as requirements to be accommodated by the approach. But, another interesting question can be asked regarding our hopes for the future developments of EWE Attribution Studies, which is the question of whether Non-Unificatory Local Integrative Pluralism would still be the most compatible choice of Pluralism, under those futuristic considerations. Although making claims about the future developments of a field can be a hard task, and it is beyond my knowledge to do so, I believe that there is a potential aspect of the SA that, if developed, would prove to be a crucial development in the field of EWE Attribution Studies. It also has implications on the way Pluralism would be perceived in such a context, and that aspect is to also consider developing the SA to further integrate EWEs that involve more dynamic aspects. Under such a scenario, there would be the opportunity for comparison at multiple levels with the PBA, as well as more areas where integration can happen between the approaches. And, a further reason why this would be an important development is due to the fact that the SA relies on building plausible storylines for an EWE, and under such circumstances, there would be potential to better understand the physics and the development of these complex nature EWEs through having a causal storyline of how they developed and could develop in the future. Under such circumstances and considering no other changes, Non-Unificatory Local Integrative Pluralism might still be the most compatible approach to have, but the scope of its local scope might increase to accommodate more circumstances, and maybe reach a universal level where there can be integration at all the levels between the approaches. But, we still need to wait to see what the future of EWE Attribution Studies holds, and how it would affect the discussions about Pluralism in its context.

To sum up, based on the conditions drawn from the comparisons of the earlier chapters, the other conditions that have been laid out in this chapter, as well as considering Shepherd's Common Framework, Non-Unificatory Local Integrative Pluralism is the most compatible form of Pluralism that can be established between the PBA and the SA of EWE Attribution Studies. While the field might still further develop in such a way that this pluralistic approach might not be valid anymore, this approach accommodates many of those changes unless they are drastic under which the methodologies themselves change into new ones, making it a different consideration for different methodologies than those currently discussed, and still being the most compatible methodology under its circumstances.

Chapter Five: Conclusion

The main question of this thesis was to investigate the possibility of Pluralism between the two main methodologies of Extreme Weather Event Attribution Studies which are the Probability-Based Approach and the Storyline Approach, given the conditions we provide as a result of a multi-dimensional comparison between the approaches, and also to arrive at a form of Pluralism satisfying the conditions mentioned. The main conclusion of the thesis is that Pluralism should be the approach to take in this context based on the indispensable pragmatic aspects of Pluralism, and the form of Pluralism that is suggested here is a subform of Integrative Pluralism that I title as *Non-Unificatory Local Integrative Pluralism* which is the idea that there can be integration between two approaches on limited levels while maintaining the general interactions between the two. And, the form of integration in this approach is not ultimately aiming for a unification project and maintains the boundaries of both approaches.

The way this conclusion was reached is in the following order:

In *Chapter 2*, I briefly explained the field of Extreme Weather Event Attribution Studies and the way it operates in the field of climate science. Later, I outlined the main distinction between the thermodynamic and dynamic factors of the atmosphere and talked about the basis for this distinction as well as the uncertainties involved with each. Afterward, I described the two main methodologies in the field, namely the Probability-Based Approach and the Storyline Approach, and talked about the way each one approaches and studies Extreme Weather Events.

In *Chapter 3*, I provided a comparison between the two approaches on multiple levels. First, I distinguished between the way each methodology defines and frames Extreme Weather Events and the limitations of the framing. Next, I explained the differences between the attributional methodologies used by each approach, namely the Fraction of Attributable Risk and Probability

Ratio for the Probability-Based Approach and Causal Networks for the Storyline Approach, and talked about some of the problematic aspects regarding their usage. Then, I proceeded to explore the amount of reliance of each approach on available historical data and argue that one approach, namely the Storyline Approach, is less reliant on available historical data than the alternative, namely the Probability-Based Approach, and how this would lead to more inclusive climate science. Afterward, I examined the proneness of each of the two approaches to different types of selection bias and argued how both approaches are prone in different ways to different biases, and how the diversity of methodologies helps in terms of mitigating the effects of these biases on the methodologies. Finally, I described how each methodology has different arguments supporting their position regarding the preference between overestimating the effects of Climate Change and underestimating it, and how this affects the way the results are communicated given different value preference considerations. I also argued how these different forms of results are pragmatically beneficial in the way that each fulfills different stakeholder needs.

In *Chapter 4*, I used the results of the comparison made in the previous chapter to serve as requirements to be considered in answering the two main questions of the thesis. Given those considerations, I investigated the potentiality of Pluralism in our context and argued for the compatibility of Pluralism based on the previous considerations through the pragmatic argument of Pluralism, and the idea that Pluralism should be considered when two approaches are pragmatically indispensable. Then, I explored some possible forms of Pluralism and investigated their compatibility with our context and considerations. This was done by eliminating the incompatible approaches based on different arguments and considering a case study, which is Shepherd's Common Framework for Approaches to Extreme Weather Event Attribution, to arrive at the approach that we proposed, titled as Non-Unificatory Local Integrative Pluralism. Finally, we argue how this approach satisfies the conditions we have laid out and also considered some criticisms and limitations of our proposed approach.

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