




Ecological Origins of Freedom: Pathogens, Heat Stress, and Frontier Topography Predict More Vertical but Less Horizontal Governmental Restriction

Personality and Social
Psychology Bulletin
2017, Vol. 43(10) 1378–1398
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sagepub.com/journalsPermissions.nav
DOI: 10.1177/0146167217713192
journals.sagepub.com/home/pspb


Lucian Gideon Conway III¹, Kate Bongard¹, Victoria Plaut²,
Laura Janelle Gornick³, Daniel P. Dodds¹, Thomas Giresi¹,
Roger G. Tweed⁴, Meredith A. Repke¹, and Shannon C. Houck⁵

Abstract

What kinds of physical environments make for free societies? The present research investigates the effect of three different types of ecological stressors (climate stress, pathogen stress, and frontier topography) on two measurements of governmental restriction: *Vertical restriction* involves select persons imposing asymmetrical laws on others, while *horizontal restriction* involves laws that restrict most members of a society equally. Investigation 1 validates our measurements of vertical and horizontal restriction. Investigation 2 demonstrates that, across both U.S. states and a sample of nations, ecological stressors tend to cause *more* vertically restrictive societies but *less* horizontally restrictive societies. Investigation 3 demonstrates that *assortative sociality* partially mediates ecological stress→restriction relationships across nations, but not in U.S. states. Although some stressor-specific effects emerged (most notably, cold stress consistently showed effects in the opposite direction), these results in the main suggest that ecological stress simultaneously creates opposing pressures that push freedom in two different directions.

Keywords

legal restriction, individualism/collectivism, vertical, horizontal

Received December 31, 2016; revision accepted May 9, 2017

All known human cultures have a political system of some kind. Those political systems inevitably have to address the inherent tension in human society between serving the best interests of the group and the serving the best interests of the individual. This struggle is ultimately reflected in a society's approach to how much to restrict the freedoms of its citizens: For example, strict criminal laws often prohibit specific individual's freedoms, but they are intended to simultaneously reduce the overall level of crime society must endure. Some cultures lean toward the *common good* in this political balancing act, while others lean toward *individual freedom* (for discussions, see, for example, Conway, Sexton, & Tweed, 2006; Conway, Houck, & Gornick, 2014; Van de Vliert, 2013a).

Ecological Precursors of Governmental Restriction

What *causes* societies to focus less on the common good and more on individual freedom? Theories answering this question

can roughly be grouped into two macrolevel categories. Some theories focus on bottom-up processes, describing how communication between persons shapes the fate of cultural or political development (e.g., Conway et al., 2009; Conway & Schaller, 2005, 2007; Conway et al., 2006). Other theories focus on top-down processes, describing how preexisting ecological conditions might shape cultural or political milieus in systematic ways (e.g., Kitayama, Conway, Pietromonaco, Park, & Plaut, 2010; Kitayama, Ishii, Imada, Takemura, & Ramaswamy, 2006; Schaller & Murray, 2008; Van de Vliert,

¹University of Montana, Missoula, USA

²University of California, Berkeley, USA

³Roanoke College, Salem, VA, USA

⁴Kwantlen Polytechnic University, Surrey, British Columbia, Canada

⁵Syracuse University, NY, USA

Corresponding Author:

Lucian Gideon Conway, Department of Psychology, University of Montana, Skaggs Building 143, Missoula, MT 59812, USA.

Email: Luke.Conway@umontana.edu

2013a; Welzel, 2013). It is the second type of theory that we address in this article.

There has been a recent debate concerning the ecological origins of cultural and political systems related to freedom (see, for example, Murray, 2013; Van de Vliert, 2013a; de Oliveira Chen & Kitayama, 2013). In particular, three different ecological stress-focused theories of the emergence of political freedom have come to the forefront. One of those theories focuses on the stressors involved in *disease prevalence* (Beall, Hofer, & Schaller, 2016; Fincher & Thornhill, 2012; Fincher, Thornhill, Murray, & Schaller, 2008; Murray & Schaller, 2010, 2012, 2014; Murray, Schaller, & Suedfeld, 2013; Murray, Trudeau, & Schaller, 2011; Park & Schaller, 2009; Schaller & Murray, 2008; Schaller & Park, 2011). A second theory focuses on the interplay between *stressful climates and wealth* (Robbins, 2015; Van de Vliert, 2007, 2009, 2011, 2013a, 2013b; Van de Vliert, Matthiesen, Gangsøy, Landro, & Einarsen, 2010; Van de Vliert & Postmes, 2012; Van de Vliert & Yang, 2014). A third theory focuses on the stressors involved in the settlement of *harsh frontiers* (e.g., Conway et al., 2014; Kitayama et al., 2010, Kitayama et al., 2006; Varnum, 2012, 2013; Varnum & Kitayama, 2011).

Stressor-Specific Influences of Ecological Stress on Freedom

These different ecological stressors often push freedom in different, stressor-specific ways. For example, work on pathogen prevalence suggests that individual freedom is restricted in cultures with more pathogen stress (e.g., Murray, & Schaller, 2010, 2012, 2013; Murray et al., 2013; see also Murray, Fessler, Kerry, White, & Marin, 2016), and some work likewise suggests that harsh climates can sometimes lead to more restrictive governments (Van de Vliert, 2014). On the other hand, work with both harsh climates¹ and frontier topography suggests that those ecological stressors sometimes produce more individualism and less governmental restriction (Conway et al., 2014; Kitayama et al., 2010; Kitayama et al., 2006; Van de Vliert, 2007, 2009, 2011, 2013a, 2013b; Varnum, 2012, 2013; Varnum & Kitayama, 2011).

These results are largely consistent with their accompanying stressor-specific theories. For example, based on pathogen stress theories, pathogens produce societal authoritarianism in part because dealing with that ecological stressor requires authoritarian norms that restrict people's freedom in order to keep them from spreading disease. Due to coevolution between parasites and hosts, these norms often have a rigid focus that excludes outgroups (see Fincher & Thornhill, 2012). Other ecological stressors do not have properties such as coevolution with parasites and thus would not necessarily lead to the same outcome. However, based on theories of frontier topographies, people living in harsher frontier environments are more likely to require individual

ingenuity to survive. Furthermore, rugged topographies often tend to drive people apart, making communal solutions more difficult to achieve, thus leading to a more individual-focused ethos that would make individual freedom paramount and government restriction more limited (see Kitayama et al., 2010). All of these challenges do not apply consistently in the same way to other kinds of stressors, and thus, there is no necessary reason to expect other stressors to produce this same outcome.

Ecological Stress as a Common Factor Influencing Freedom

As the preceding section makes clear, different ecological stressors can produce potentially different cultural outcomes with respect to freedom. Thus, it might be tempting to dismiss "ecological stress" as a macrolevel construct that can help us understand the origins of freedom. If one kind of ecological stress creates more freedom and another kind of ecological stress creates less freedom, what is the point of considering the relationship of ecological stress (writ large) to freedom?

This is a reasonable question. And yet stressor specificity does not necessarily preclude directionally similar effects of ecological stress that apply across different kinds of stressors. As an analog from work on arousing emotions, consider that the emotions of disgust and anger often have different effects; however, this fact does not preclude the identification of similar underlying influences of the roughly equal levels of arousal produced by each emotion (see, for example, Gil & droit-volet, 2012). Thus, in much the same way that domain specificity in other areas of psychology does not preclude an underlying main effect (for a discussion in another research area, see, for example, Conway et al., 2015), the existence of theoretically interpretable stressor-specific effects does not preclude the possibility of a general effect of stress across multiple types of stressors (see also Welzel, 2013).

Are there reasons in this case to imagine that ecological stressors share common features? Yes. All ecological stressors can threaten survival, and they all generally make life less comfortable. That is true whether the ecological stress involves disease, extreme heat, extreme cold, or harsh topography. Although the specific nature of the stressors is different, a sick person and a person experiencing excessive heat both nonetheless face challenges to survival, and both feel less pleasant than if they were not sick or overheating.

In the present article, we examine both the stressor-specific effects of each ecological stressor *and* stressor-general effects that might apply to all stressors. To do so, we elaborate on a set of reasons that ecological stress might exert a general and consistent influence on governmental restriction of human freedom by discussing a dual-pressure model of ecological stress—a model that first requires us to take a step back and distinguish between different types of freedom.

Vertical and Horizontal Governmental Restriction

While there is a tendency to think of governmental restriction of freedom in a “one-size-fits-all” way, in reality, governmental restriction itself is not a unitary construct. Although all governmental restrictions involve taking away freedoms, they do not all do so in the same way or with the same breadth. One of the most important dimensions on which restrictions can differ is the degree to which restrictions are *vertical* or *horizontal* (see Conway et al., 2006; Conway et al., 2014). This directional metaphor is borrowed from work on vertical versus horizontal cultures: Some cultures appear to be more comfortable with a hierarchical structure where some persons or groups are at the “top” and others at the “bottom” (hence the term *vertical*), while other cultures emphasize the “sideways” equality of all persons (hence the term *horizontal*; see, for example, Gelfand, Triandis, & Chan, 1996; Hofstede, 1980; Singelis, Triandis, Bhawuk, & Gelfand, 1995; Smith, Dugans, & Trompenaars, 1996; Triandis & Gelfand, 1998). Governmental restrictions can be similarly classified as those that apply in a hierarchical fashion versus those that apply in a more equality-based fashion. Some restrictions involve one group of people imposing a hierarchical structure on a minority of other persons in a populace (and are thus more vertical in nature); other restrictions apply to the larger majority of persons (and are thus more horizontal).

Vertical and horizontal governmental restrictions are conceptually orthogonal constructs. Although, as we shall see, they tend to be modestly negatively related in our own data, there is no necessary reason it should always be so. It is easy to conceive of a society that is high on both forms (for example, a society could quite easily be high on vertical laws such as “three strikes” rules applying only to repeat criminal offenders, but also high on horizontal restrictions such as road safety laws that affect the majority of the populace) or low on both forms. From a practical point of view, there are forces that might push the two apart *and* forces that might bring them together. (a) For example, cultures differ in their propensity toward ingroup favoritism (see, for example, Fincher & Thornhill, 2012; Van de Vliert, 2011). Cultures high in ingroup favoritism might be particularly likely to produce laws that apply only to minority groups, but might be especially *unlikely* to produce laws that apply to all persons equally. This might make the two negatively related. (b) However, cultures also differ on their individualistic ethos more broadly (see, for example, Kitayama et al., 2010). Highly individualistic societies may have a bias toward opposing laws of any ilk. To an individualist, it may matter little whether a law restricts a great number of people equally or only some people—they may simply prefer more than less freedom. This would drive the two types of restriction together as individualistic societies produced less of both kinds of restriction and collectivistic societies produces more of both kinds.

Why Does the Distinction Between Vertical and Horizontal Restriction Matter? A Dual-Pressure Model of Ecological Stress

Both vertical and horizontal restrictions involve governments removing freedom from persons in the populace under consideration. The force of the restriction on the affected parties is the same whether the origins of the law are vertical or horizontal. Nonetheless, in spite of this similarity, vertical and horizontal restrictions have potentially very different ecological origins, and this may help us more fully understand the larger relationship between ecological stress and individual freedom. What makes vertically restrictive governments may not be the same as what makes horizontally restrictive ones, and it is possible that ecological stress consistently contributes differently to those two types of restriction.

How might this be? We discuss one possibility below.

Dual Pressures in Ecological Stress

At a broad level, it is worth noting that any kind of ecological stress—regardless of stressor-specific impacts—can potentially produce psychological effects that push persons in different directions with respect to freedom. Indeed, other researchers have pointed out that stress can produce multiple potential responses: For example, stress can sometimes produce close-minded avoidance of ambiguity, yet at other times can produce open-minded problem solving (Gelfand et al., 2011; LePine, LePine, & Jackson, 2004; Richter & Kruglanski, 2004; Van de Vliert, 2013a).

Drawing on both this work and research on dual-concern models in the conflict management and negotiation literatures (Blake & Mouton, 1970; McClintock & Moskowitz, 1976; Pruitt & Rubin, 1986; Van de Vliert, 1997), we suggest here that these sometimes-opposing psychological processes often operate *concurrently* in cultural development, and this fact might help us better understand the ecological origins of freedom. Specifically, the dual-pressure model proposes that there is a tension “built in” to ecological stress at a general level—a tension that occurs for essentially all kinds of ecological stress.

How might ecological stress produce concurrent opposing pressures? (a) On one hand, ecological stress may produce a need for ingroup affiliation or authoritarianism to solve specific problems. When a stressor is present, it may heighten one’s need for ingroup aid or strict rules because the stressor means persons are less likely to survive otherwise. (b) On the other hand, ecological stress may force people to compete for resources to survive, producing an individualistic ethos more psychologically conducive to political freedom than rigid rules. In this ethos, persons may especially abhor laws that give limited resources to those outside of their own local sociocultural circles, for fear that those

resources will simply not be available for them to compete for if they are spread *too* thin. Individual ingenuity would not be useful if there simply are not enough resources within which to use it.

Thus, while it is certainly the case that part of the variance across prior studies in the effects of ecological stress on freedom is due to stressor-specific differences, it is also possible that part of the variability across contexts may be attributable to the inherent opposing pressures involved in *ecological stress* as a large construct. Consider, for example, that although pathogens clearly can create pressure for authoritarian norms (e.g., Murray & Schaller, 2010), it is also conceptually possible that that same stressor could lead to a drive for individual ingenuity in much the same way as harsh topography.² On the flip side, consider that the stressors in harsh topography that might make people avoid collectively inspired limitations (e.g., Kitayama et al., 2010) might also make them more in need of top-down rules. If the terrain is harsh and it is hard to find food, I might be more likely to favor authoritarian laws that increase the likelihood of my family and others close to me of acquiring food from others—particularly if such a law comes at the expense of other groups.

As a result of these reflections, it is possible that ecological stress of any kind concurrently produces pressures in both directions. While there are many possible ways that these dual pressures might manifest themselves, we explore here the possibility that they manifest themselves differently in vertical versus horizontal legal restriction. For example, ecological stress may increase the desire for ingroup affiliation and outgroup denigration, a psychological backdrop wherein vertical forms of restriction are more likely to be implemented. However, while ecological stressors may make people more likely to implement laws restricting *others*, they may simultaneously make people feel concerned about spreading resources *too* thin, thus leading to reduced horizontal legal restriction (see Conway et al., 2014). Ecological stress may, in a sense, cause persons to batten down the psychological hatches: It may cause them to focus on ensuring their own survival and those around them whom they care about (or of ingroup members who would be more likely to reciprocate help as needed), but to avoid larger collective actions that are shared equally among all people within a larger cultural area (and thus risk spreading resources too thin).³

One Mechanism for Understanding Dual Pressures: Assortative Sociality

The above discussion suggests that concurrent dual pressures may partially result from ecological stressors' impact on ingroup/outgroup behavior and attitudes—what has sometimes been called assortative sociality (see Fincher & Thornhill, 2012, for a fuller discussion). Cultures high in assortative sociality tend to focus on local groups more so than larger collectives.

Consider the following links in a possible causal chain: (a) Ecological stress produces more assortative sociality

because it narrows survival focus to local ingroups (for one example, see Fincher & Thornhill, 2012). (b) Assortative sociality subsequently puts opposing pressures on vertical and horizontal legal restriction. Cultures with a localized view of groups that draw heavy distinctions between their own groups and others (high assortative sociality) may be particularly likely to show vertical forms of legal restriction because those forms involve hierarchical applications of political systems and laws. However, cultures high in assortative sociality may be *less* likely to form horizontal laws because broad-based laws that apply equally to everyone do not fit a hierarchical ethos.

To the degree that both (a) and (b) above prove true, increasing ecological stress would produce opposing pressures on different kinds of freedom. If ecological stress increases assortative sociality, it may cause an increase in vertical restriction while causing a decrease in horizontal restriction, and the effect of ecological stress on each kind of restriction would thus be mediated by assortative sociality.

Overview of the Present Studies

In Investigation 1, we develop and validate new indices that allow us to more clearly distinguish between horizontal and vertical legal restriction (both within the United States and across nations). In Investigation 2, we then proceed to our primary tests of the potential ecological origins of these different types of restrictions by simultaneously exploring markers of pathogen prevalence, climate stress, and frontier topography. In Investigation 3, we investigate directly the degree to which assortative sociality serves as a mechanism by which ecological stress affects vertical and horizontal restriction differently.

Throughout, we use the principle of triangulation (see, for example, Conway et al., 2014; Kitayama et al., 2006) by looking at parallel tests both within the United States and across a multinational sample. Within-nation and across-nation approaches have complementary strengths and weaknesses (see, for example, Van Lange, Rinderu, & Bushman, *in press*), and thus, this triangulation approach helps us better establish and understand the theoretical ties between ecological variables and governmental restriction (see Conway et al., 2014; Kitayama et al., 2006, for discussions).

Investigation 1: Comparing Vertical and Horizontal Legal Restriction

The goals of Investigation 1 were to (a) fill in measurement gaps in prior research by developing and updating governmental restriction measurements as necessary, and (b) validate the usefulness of those measurements of vertical and horizontal restriction. As we will see, Investigation 1 revealed theoretically interpretable differences between horizontal and vertical legal restriction measurements that give us more confidence to move on to Investigation 2.

Measurement Gaps From Prior Studies

Prior work on governmental restriction has shown a patchwork of evidence with many gaps to fill. That work has largely focused on one type of horizontal legal restriction measurement in the United States (that using laws), but has no parallel measurement in the United States for vertical restriction (see Conway et al., 2006; Conway et al., 2014). Contrarily, although that work has used a vertical measurement of political restriction at the nation level, it has included no measurement of horizontal restriction at the nation level and no parallel vertical legal restriction measurement.

In summary, as of yet no comprehensive measurements exist to compare the associations of vertical and horizontal legal restriction at the state and national level. Investigation 1 helps to fill in these gaps. For the United States, we aimed to develop an index of vertically restrictive laws and to compare this index with the predictive power of the horizontal restriction index used in previous research. We also aimed to develop a vertical legal restriction index for each nation and to compare the predictive validity of this index with a newly updated index of horizontal legal restriction. (See the online appendix for scores for each state and nation on all newly developed indices.)

Criteria for Validation in Study 1

Validation is a multifaceted construct. For example, researchers have distinguished between validation tests that are implied by the measurement and those that are specific to a particular theory or theoretical perspective (see Houck et al., 2014; Tetlock et al., 2014). We use both kinds of validity tests in Investigation 1. For example, one of our tests (described below) involves taking our developed indices of vertical and horizontal restriction and collecting data on the likelihood that the laws in each index could be considered vertical or horizontal in the populations described. This kind of validation is directly implied by the measurement.

We also provide tests of the second kind of validity in Investigation 1. The logic of this is as follows. Before proceeding to our primary research questions in Investigations 2 and 3, it is useful to demonstrate other meaningful theoretical relationships for these constructs.⁴ While these tests are less direct markers of validity because the distance between the constructs and the outcomes is greater, they are nonetheless useful ways to establish the validity of a construct (see Houck et al., 2014; Tetlock et al., 2014). We describe each kind of validity test in more detail below.

Investigation 1 Method

For the U.S.-focused inquiry, the unit of analysis was the U.S. state, which is in line with a growing body of work on U.S. states (e.g., Fincher & Thornhill, 2012; Plaut, Markus, & Lachman, 2002; Rentfrow, 2010; Rentfrow, Gosling, & Potter, 2008; Varnum, 2013). For across-nation analysis, the nation was the unit of analysis.

Vertical legal restriction

Development of the U.S. vertical legal restriction index. For the United States, we created a state-level “Vertical Legal Restriction Index.” See the online appendix for more detailed information on selection of laws, scoring of the laws, and the validation of this scale. We targeted three general areas of legal restriction conceptually associated with vertical restriction and for which information on laws was widely available: Abortion, Extreme Criminal Punishment, and Gay/Lesbian Rights. We obtained a range of 28 laws relevant to these issues (see online appendix for original complete list of laws). In each case, the law was scored by the source as either being present or not present, and we coded laws directly from sources as containing the presence of a restrictive law/absence of a law protecting freedom (= 1) or absence of a restrictive law/presence of a law protecting freedom (= 0). Thus, each variable was scored so that 1 = restriction and 0 = no restriction.

Following procedures from previous research (Conway et al., 2006; Vandello & Cohen, 1999), we then narrowed this set of laws by removing both redundant items (e.g., if there were two highly correlated measurements of conceptually similar things, such as the laws pertaining to “Providers May Refuse to Participate—Individual Abortion” and “Providers May Refuse to Participate—Institutional Abortion”) and items with low interitem correlations. This process left eight laws spread across our three focal areas that had adequate reliability ($\alpha = .70$): three abortion laws (partial birth abortion ban, public funding of abortions limited to life endangerment and rape, and parental involvement required for minors), two extreme criminal punishment laws (death penalty, “three strikes” laws), and three gay/lesbian rights laws (language banning same sex marriage, absence of official recognition for same sex unions, and absence of domestic partner benefits). Scores for these laws were converted to *z*-scores and averaged into a “Vertical Legal Restriction Index” for each state.

Nation-level vertical restriction indices. For the nation-level index, we used two different markers of vertical governmental restriction.

First, we developed a legal restriction index designed to be parallel to that used for the U.S. states. This index contained laws pertinent to abortion; lesbian, gay, bisexual, and transgender (LGBT) rights; and the death penalty (see online appendix for more information on the construction of this index). We obtained laws for 191 nations on all three of those variables. We converted each variable to a *z*-score, reverse-scored when necessary so that higher scores meant more restriction, and averaged into a single *vertical legal restriction* index. As with the parallel state-level index, this index had good reliability (standardized $\alpha = .67$).

A separate-but-overlapping way to characterize vertical governmental restriction is by considering the larger political system in which laws are constructed. Restriction

produced in totalitarian governments is more vertical on average, because it involves a leader or group of leaders imposing laws on people who did not get to vote for those laws. As a result, totalitarianism is a conceptual proxy for vertical governmental restriction. Thus, as an additional marker of vertical restriction, we used the Freedom House Totalitarianism index: We averaged the Freedom House (2013) indices for political rights and civil liberties. This index has been used extensively in previous research as a measure of top-down authoritarian pressure that removes the freedom of its citizens (e.g., Clague, Gleason, & Knack, 2001; Conway et al., 2006; Dixon & Senese, 2002; Vanhanen, 2000) and thus is a good measurement of *vertical political restriction*.

These two markers were modestly positively correlated, $r(190) = .41, p < .001$, suggesting that while there is some tendency for totalitarian governments to produce more vertically restrictive laws, there is nonetheless a substantial amount of nonoverlap in these measurements. Thus, we opted to analyze them separately.

Horizontal legal restriction

U.S. index development. To measure horizontal legal restriction, we used the previously constructed and validated “Horizontal Legal Restriction Index” for each of the 50 states (Conway et al., 2006). The index measures laws related to taxes, road safety, and gun control which favor governmentally initiated restriction affecting a high proportion of the population. This index is (nonsignificantly) negatively related to the vertical legal restriction index, $r(50) = -.21, p = .140$.

Across-nation index development. To measure horizontal legal restriction, we created an updated version of a previously constructed “Horizontal Legal Restriction Index” (Conway et al., 2006; Conway et al., 2014; see online appendix for more information). The index parallels the U.S. horizontal restriction index and measures laws related to taxes, speed limit laws, and blood alcohol levels.

As in the original Conway et al. (2006) index, the three types of laws were not consistently positively correlated with each other, and as such do not form an index with good internal reliability. We nevertheless converted the score for each law to a *z*-score and averaged them to produce a single index of *horizontal legal restriction*. We did this for two reasons: (a) Conceptual overlap is often more important than correlation (see Welzel, 2013, for a discussion). Even if the amount of apples and the amount of bananas people eat every day were not correlated, it nonetheless would make sense to combine them into an additive measure of “fruit consumption.” In this case, the three law types are similarly conceptually related to each other in their overlap with horizontal legal restriction—more restrictive tax laws, more restrictive speed limit laws, and more restrictive blood alcohol levels are all laws that conceptually mean more horizontal legal

restriction. As such, an additive score meaningfully captures a range of laws relevant to horizontal restriction. (b) Relatedly, we wanted a summary index that was fairly comparable with the U.S. index for triangulation purposes. While we could potentially search for other laws that might artificially increase the internal reliability of the index, doing so would make our comparison between nation- and state-level data harder to interpret. Thus, there is value in keeping the indices as parallel as reasonably possible.

The resulting horizontal legal restriction index was modestly negatively correlated with both vertical legal restriction, $r(177) = -.48, p < .001$, and vertical political restriction, $r(176) = -.43, p < .001$.

Validity test implied by restriction measurements: Percentage of the U.S. population a law restricts. If the horizontal legal restriction index truly casts a more inclusive net than the vertical legal restriction index, it should apply to more people within the relevant population. On the surface, all laws apply to all people in a culture (e.g., if there is a law relevant to repeat offenders, that means that *any* person in society is potentially subject to that law). Practically and psychologically, however, laws vary greatly in the likely populations they affect. Thus, as a direct validity test, in each case, we used available data in the United States to estimate the likely percentage of the population that the law domain restricts (horizontal law domains = tax, road safety, and gun control; vertical law domains = abortion, extreme criminal punishment, and gay/lesbian rights). It is implied by the nature of the constructs themselves that the law domains comprising the two measurements should differ on this percentage, and thus, this is a direct validity test.

To accomplish this goal, we used known estimates of people who engage in the relevant behavior or adopt the relevant identity.⁵ While this approach likely underestimates in each case the percentage of people affected by the law (as, for example, some portion of the population may want to engage in the restricted behavior but do not because it is legally forbidden), it does so roughly equally across all the domains under scrutiny, and as such, it is still a reasonable marker of the relative size differences among the populations affected by the laws.

The specific details concerning our application of this approach are discussed in the online appendix. The net result is that we produced estimates of the likely percentage of the U.S. population that a given law directly restricts. We then used this to test the assumption that horizontal laws were in fact more horizontal than vertical laws (see “Investigation 1 Results and Discussion” section) in the United States.

Validity tests relevant to specific theoretical contexts: Correlates with common sociocultural variables. To further validate these measurements before proceeding to Investigations 2 and 3, we chose to focus on three primary sets of correlates. These were chosen partly because they have implications for our understanding of vertical and horizontal legal restriction and partly

because they are common touchstones in research on sociocultural variables: Collectivism (e.g., Conway et al., 2006; Conway et al., 2014; Van de Vliert, 2013a), Wealth (Hofstede, 1980; Schwartz, 1990 see, for example, Diamond, 1992; Lipset, 1959; see Ball, 2001; Clague et al., 2001, for reviews), and Urbanity/Pace of Life (Conway et al., 2006; Vandello & Cohen, 1999). To the degree that vertical and horizontal restriction show demonstrably different and theoretically interpretable relationships with these variables—especially if these relationships hold across different units of analyses—it provides additional evidence that they are valid measures for distinguishing different forms of legal restriction.

Collectivism. To measure cultural collectivism within the U.S. sample, we used Vandello and Cohen's collectivism index for each of the 50 states (Vandello & Cohen, 1999). This index has been validated in multiple ways for use as a state-level index (Conway, Ryder, Tweed, & Sokol, 2001; Conway et al., 2006; Kimmelmeier, Jambor, & Letner, 2006; Vandello & Cohen, 1999). For nation-level analysis, we used Hofstede's (1980) nation-level index of "individualism" (reverse-scored for ease of presentation) as an indicator of *cultural collectivism*.

Wealth. For U.S. analysis, three measures of wealth were included: GDP per capita, poverty, and personal income per capita. Unless otherwise noted, all statistics were obtained through the U.S. Bureau of the Census, either in Statistical Abstracts of the United States (United States Bureau of the Census, 1997) or via the Internet (see www.census.gov). For the nation-level analysis, we included a measure of GDP per capita (from Socioeconomic Data and Applications Center, 2007).

Urbanity/Pace of Life. For both the U.S. and nation-level analyses, percent living in urban areas and pace of life were included as measures related to urbanity. For the U.S. analysis only, population density was also included. The pace of life measure was taken from work by Levine and his colleagues (see Levine, Martinez, Brase, & Sorenson, 1994, for the United States.; see Levine, 1997; Levine, Norenzayan, & Philbrick, 2001, for international), where they measured, among other things, how quickly people walked in various locations.

Covariate for U.S. analysis: Democratic versus Republican vote. Because in the present work in the United States we hope to get at legal restriction that is above and beyond mere allegiance to a particular political party, we computed an index representing the average political party affiliation in each state: the average percentage of the vote cast for the Democratic presidential candidate from 1980 to 2004.

Investigation 1 Results and Discussion

Percentage of the population affected by the law in the United States. We first performed a *t* test with restriction type

(horizontal vs. vertical) as the independent variable and the percentage of the population each law affects as the dependent variable. For this procedure, "law domain" served as the unit of analysis ($n = 6$ domains, representing 3 horizontal and 3 vertical law domains). Despite this low power, the horizontal law domains were significantly more horizontal ($M = 47.4\%$) than the vertical law domains ($M = 8.4\%$), $t = -3.28$, one-tailed $p = .016$.⁶ This provides fairly direct evidence using available U.S. data that our horizontal laws are accurately viewed as more horizontal and the vertical laws more vertical in nature.

Correlations

U.S. analysis. To analyze the relationship between the two legal restriction indices and our correlates, both zero-order correlations and partial correlations (controlling for Democratic vote) were applied, using the state as the unit of analysis. These analyses are presented in Table 1.

As can be seen there, when using a zero-order correlation, the horizontal legal restriction index was significantly positively correlated with U.S. collectivism, $r = .47, p < .05$ (95% confidence interval *beta* lower = 5.0, upper = 16.9). The vertical legal restriction index was also positively correlated, although this correlation was not significant, $r = .10, p > .05$. However, controlling for the percentage of Democratic vote suggested that party affiliation was suppressing a real effect for vertical legal restriction: When controlling for party affiliation, both the horizontal ($r = .41, p < .05$; 95% CI *beta* lower = 3.8, upper = 17.0) and vertical ($r = .30, p < .05$; 95% CI *beta* lower = 1.2, upper = 32.9) legal restriction indices were significantly positively related to U.S. collectivism.⁷

Although similar with respect to collectivism, additional analyses revealed that horizontal and vertical forms of legal restriction nonetheless showed differential predictive validity for other factors. As can be seen in Table 1, while horizontal legal restriction was positively correlated with measures of wealth, the opposite was true for vertical legal restriction (Steiger's *Z* tests of correlated correlations suggested that this differential predictive power of the two indices was significant). A similarly significant difference emerged for measures of urbanity, with states higher in horizontal legal restriction showing more urbanity, population density, and a quicker pace of life, while the opposite was true for the vertical legal restriction index (Steiger's *Z*'s all $< .05$). While controlling for the percentage of Democratic vote reduced the size of some of these effects, in general the pattern of results remained largely unchanged (see Table 1).

Nation-level analysis. Nation-level results are presented in Tables 2 and 3. As can be seen, the pattern of results showed some striking similarities to the results from the U.S. analysis. Whether using vertical legal restriction (Table 2) or vertical political restriction (Table 3), vertical forms of restriction were significantly negatively related to measurements of wealth and urbanity, while horizontal restriction was

Table 1. Investigation I: Validity Tests for Vertical Versus Horizontal Legal Restriction Across U.S. States.

	Zero-order			Controlling for democratic vote		
	Vertical restriction	Horizontal restriction	Steiger's Z difference	Vertical restriction	Horizontal restriction	Steiger's Z difference
Collectivism						
U.S. collectivism	.10	.47**	ns	.30*	.41*	ns
Wealth						
GDP per capita	-.39**	.10	-2.22*	-.37*	.03	1.96 [†]
Personal income	-.50**	.32*	-3.78**	-.33*	.15	2.31*
Poverty	.27 [†]	-.03	1.34	.37*	-.07	-2.14*
Wealth total	-.47**	.18	-2.98**	-.42**	.10	2.55*
Urbanity						
Population density	-.43*	.35*	-3.55**	-.18	.16	1.62
Urban percent	-.18	.32*	-2.23*	-.10	.28 [†]	-1.82 [†]
Pace of life	-.27	.28 [†]	-1.98*	-.23	.24 [†]	-1.70 [†]
Urbanity total	-.35*	.40**	-3.39**	-.16	.27 [†]	-2.03*

Note. Descriptive metric = *r*; Difference = Steiger's Z for computing the probability that different overlapping correlations would have emerged due to chance alone; *n* = 48, except for U.S. Individualism (*n* = 50 for this measure); ns = *p* > .05. *n* = 50 except "Pace of life" (*n* = 36).
[†]*p* < .10. **p* < .05. ***p* < .01. (two-tailed).

Table 2. Investigation I: Validity Tests for Vertical Political Restriction Versus Horizontal Restriction Across Nations.

	Vertical restriction	Horizontal restriction	Steiger's Z difference
Collectivism	.59*** (97)	-.49** (98)	-7.01*
Wealth			
GDP per capita	-.51*** (190)	.25*** (177)	-6.26*
Urbanity			
Urban percent	-.45*** (151)	.33** (148)	-5.88*
Pace of life	-.74*** (30)	.33 [†] (31)	-4.12*

Note. *n* for each comparison is indicated in parentheses; descriptive metric = *r*; Difference = Steiger's Z for computing the probability that different overlapping correlations would have emerged due to chance alone.
[†]*p* < .10. **p* < .05. ***p* < .01. ****p* < .001.

Table 3. Investigation I: Validity Tests for Vertical Legal Restriction Versus Horizontal Restriction Across Nations.

	Vertical restriction	Horizontal restriction	Steiger's Z difference
Collectivism (Hof)	.58*** (97)	-.49** (98)	-6.69*
Vertical collectivism	.48*** (42)	-.26 [†] (42)	-2.50*
Horizontal collectivism	.54*** (42)	-.48*** (42)	-3.81*
Wealth			
GDP per capita	-.43*** (224)	.25*** (177)	-5.71*
Urbanity			
Urban percent	-.36*** (160)	.33** (148)	-5.01*
Pace of life	-.47** (31)	.33 (31)	-2.49*

Note. *n* for each comparison is indicated in parentheses; descriptive metric = *r*; Hof = Hofstede's collectivism measure; Difference = Steiger's Z for computing the probability that different overlapping correlations would have emerged due to chance alone.
[†]*p* < .10. **p* < .05. ***p* < .01. ****p* < .001.

positively related to both types of measures. Furthermore, as with the U.S. analysis, both vertical forms of legal restriction were positively correlated (in most cases, significantly) with cultural collectivism.

The key divergence from the U.S. pattern involved the relationship between horizontal restriction and collectivism. While in the U.S. analysis horizontal forms of collectivism were associated with greater cultural collectivism, in the

nation-level analysis, the opposite pattern occurred: Horizontal legal restriction was negatively related to collectivism. This suggests that at the nation level, more collectivistic countries are *less* likely to have horizontally restrictive laws. This pattern converges with results from Investigation 3 on assortative sociality—an issue we return to later in the article.

Transitional Discussion

These results provide some initial evidence that the measurements of vertical and horizontal governmental restriction—to be used in Investigations 2 and 3 to understand the origins of cultural freedom—are valid. It is indeed noteworthy that measurements across such very different units of analysis (within one nation versus across multiple nations) and that sometimes contain different instantiations of restriction (legal versus political) show a strikingly similar pattern of results overall. This pattern suggests that our measurements of vertical and horizontal restriction provide a valid backdrop to study the origins of freedom.

It is beyond our present purpose to pursue in depth the specific meaning and contribution of all the correlations used in Investigation 1. However, we do pause to consider one of these correlations sets in particular—wealth—with the primary goal of establishing that these correlations demonstrate the conceptual value of our measurements.

The present results demonstrate a similar pattern within the United States and across nations for wealth. In particular, these results suggest that horizontal legal restriction is positively related to wealth, while vertical legal restriction is negatively related to wealth. These results are consistent with other theory from political science suggesting that restrictive forms of government are not necessarily less likely to produce wealth (see Ball, 2001). Indeed, our work could help reconcile this theory with work from other areas suggesting that an individualistic ethos (not generally viewed as conducive to legal restriction) ought to be associated with greater wealth (see, for example, Diamond, 1992; Lipset, 1959; Schwartz, 1990; see Clague et al., 2001, for a review). In particular, a society of people viewed as equals can be a very powerful force for financial success, and thus, laws reflecting that horizontal outlook may encourage financial gain (see Ball, 2001); however, it may be that vertical legal restriction has the opposite effect. Consistent with this idea, Acemoglu and Robinson (2013) argue that inclusive economic institutions (that closely embody a horizontal philosophy) foster wealth and prosperity, whereas extractive economic institutions (that have adopted an approach most closely resembling vertical restriction) are comparatively less incentivizing and thus hinder the accumulation of wealth. Thus, considering our dual measurements of legal restriction helps us understand both aspects of the wealth–restriction relationship in a way that fits with theory from different sides of the debate.

We now turn to our primary purpose: to use these measurements to investigate ecological origins of freedom.

Investigation 2: Ecological Precursors of Governmental Restriction

Investigation 2 Method

Vertical and horizontal restriction. For Investigation 2, we used the same vertical and horizontal restriction measurements from Investigation 1 for U.S. states and across nations, respectively.

Stress precursor measures

Pathogen prevalence. To measure the amount of preexisting communicable disease prevalence for both U.S. states and nation-level analyses, we used Fincher and Thornhill's (2012) pathogen prevalence indices (see Fincher & Thornhill, 2012, for computational details and description of each final index).⁸

Climate stress. To measure the amount of stress induced by extreme climates, we used Van de Vliert's measurement of climate stress for both the U.S. states and across nations (Van de Vliert, 2011, 2013a). Because researchers have noted that hot and cold stressors can have different effects (e.g., Murray, 2013; Van de Vliert, 2013b; Van de Vliert & Tol, 2014), we here separate hot versus cold climate extremes.

Frontier topography. Frontier stress, like climate stress, is a multifaceted construct. As such, it can be operationalized in many ways. In the present study, we opted to use topographical physical terrain markers of frontier stress that have been used in past research: mountain topography and inland topography (from Conway et al., 2014). The measurements used in the present study are correlated with other markers of frontier terrain (see Conway et al., 2014). Because frontier terrains are harsher (see Kitayama et al., 2010), these measurements were thus useful for our present purpose (see Conway et al., 2014, for a specific defense of these two measurements as related to frontier stress).

Constructing an overall ecological stress measurement. These five stressor measurements were not always highly correlated with each other (and sometimes negatively correlated, as with cold stress and pathogens). However, because our theoretical focus on the dual pressure model aims to tease out cumulative stressor-general from stressor-specific effects, it is conceptually important that we create an aggregate measurement of ecological stress from the five measurements (one pathogens, two climate, two frontier topography) used in the present study. The logic behind this is as follows: It is useful to create a composite measurement out of uncorrelated—or negatively correlated—things, when the sum total of that measurement is *conceptually* meaningful (for a discussion, see Welzel, 2013). In this case, as discussed in the introduction, there is reason to suspect that there might be an additive effect of multiple stressors that would be predictive of political freedom. Thus,

Table 4. Investigation 2: Stressors’ Zero-Order Relationship to Vertical and Horizontal Restriction Across Both Nations and States.

Stressor type	Parasite	Climate		Frontier geography	
	Pathogens	Hot	Cold	Mountains	Inland
Across U.S. states					
Vertical legal restriction	.34*	.16	-.16	-.32*	.38**
Horizontal legal restriction	.23 ^{††}	-.46***	-.07	-.29*	-.47***
Across nations					
Vertical legal restriction	.52***	.51***	-.54***	.05	.09
Vertical political restriction	.54***	.31***	-.18*	.30***	.36***
Horizontal legal restriction	-.50***	-.39**	.41***	-.16*	-.10

Note. Descriptive metric = *r*. For U.S. states, *N* = 50. For Across Nations Vertical Legal–Pathogens *r*, *N* = 191. For Across Nations Vertical Legal–Hot/Cold *r*, *N* = 224. For Across Nations Vertical Legal–Mountains *r*, *N* = 234. For Across Nations Vertical Legal–Inland *r*, *N* = 141. For Across Nations Vertical Political–Pathogens *r*, *N* = 188. For Across Nations Vertical Political–Hot/Cold *r*, *N* = 189. For Across Nations Vertical Political–Mountains *r*, *N* = 189. For Across Nations Vertical Political–inland *r*, *N* = 141. For Across Nations Horizontal–Pathogens *r*, *N* = 175. For Across Nations Horizontal–Hot/Cold *r*, *N* = 177. For Across Nations Horizontal–Mountains *r*, *N* = 176. For Across Nations Horizontal–Inland *r*, *N* = 137.
^{††}*p* < .15. **p* ≤ .05. ***p* ≤ .01. ****p* ≤ .001.

while acknowledging the pitfalls of aggregating across different stressors, we think the benefits of a cumulative aggregate score outweigh these pitfalls in the present study. This aggregate measure, both for the U.S. states and across nations, involved standardizing each stressor and then averaging each stressor into an overall index. As a result, each individual stressor contributed equally to the overall stress measurement.⁹

Wealth as a modifier. One theory of the effect of cold stress suggests that its impact on freedom is modified by wealth (see Van de Vliert, 2013a). In particular, when financial resources are available, cold stress produces more individualism because individuals have enough resources to handle the stressor “on their own.” But when resources are not available, cold stress forces people together and makes them more likely to sacrifice their individual freedoms out of necessity to survive. A vast array of evidence exists for this expected Wealth × Stressor interaction for cold stress (Van de Vliert, 2007, 2009, 2011, 2013a, 2013b; Van de Vliert et al., 2010; Van de Vliert & Postmes, 2012; Van de Vliert & Yang, 2014).

Conceptually, it is possible that the same kind of Wealth × Stressor interaction exists for the other kinds of ecological stress. However, there is far less evidence testing this idea for other stressors. Some evidence suggests the interaction does not apply for heat stress in the same way as it does for cold stress (Van de Vliert & Tol, 2014). Research examining Wealth × Topography interactions is very scarce, and what evidence exists seems mixed (see Conway et al., 2014; de Oliveira Chen & Kitayama, 2013). And while wealth has been controlled for in pathogens research as a covariate (e.g., Murray et al., 2011), we know of no direct tests of Wealth × Pathogens interactions on individual freedom measurements.

The present study provides an opportunity to comprehensively test for Wealth × Stressor interactions on all three stressor domains simultaneously, using the same outcome measurements on the same dataset. We use the same wealth measurements as in Investigation 1 to accomplish this task.

Table 5. Investigation 2: Competitively Comparing the Ecological Origins of Vertical Versus Horizontal Governmental Restriction Using Steiger’s Z Tests.

Stressor	Vertical legal with horizontal	Vertical political with horizontal
Across U.S. states		
Pathogens	0.52	
Hot	2.84**	
Cold	-0.40	
Mountains	-0.10	
Inland	3.94***	
Cumulative ecological stress	3.04**	
Across nations		
Pathogens	5.53***	6.13***
Hot	4.66***	3.78***
Cold	-5.08***	-3.09**
Mountains	1.61 ^{††}	2.29*
Inland	1.48 ^{††}	2.07*
Cumulative ecological stress	3.94**	4.68***

Note. Please see footnote to Table 4 for more information on measurements.
^{††}*p* < .15. **p* ≤ .05. ***p* ≤ .01. ****p* ≤ .001.

Investigation 2 Results and Discussion

Primary results are presented in Tables 4 and 5. As can be seen there, there is an overall tendency, both across U.S. states and across nations, for ecological stressors to be positively associated with vertical forms of governmental restriction (both legal and political), but negatively associated with horizontal forms of governmental restriction.¹⁰ The primary exception to this was cold stress, which typically showed the opposite pattern.¹¹

To more directly test the idea that the two types of governmental restrictions have different ecological origins, we computed Steiger’s Z test for comparing correlated correlations

Table 6. Investigation 2: Cumulative Ecological Stressor Measure Predicting Legal Restriction and the Modifying Effect of Wealth.

Stressor type	Whole sample		Only nations with all five measurements	
	<i>r</i>	Interaction	<i>r</i>	Interaction
Across U.S. states				
Vertical legal restriction	.18	.14*		
Horizontal legal restriction	-.48***	-.15		
Across nations				
Vertical legal restriction	.19**	-.16 ^{††}	.36***	-.16
Vertical political restriction	.54***	-.51*	.48***	-.34
Horizontal legal restriction	-.31**	.05	-.34***	.04

Note. *r* = zero-order relationship between legal restriction and overall stressor measure. For nations, “whole sample” is an average of all five measures, with missing measurements given the mean of the other measures; the “only nations with all five measurements” excludes all nations for which all five measurements are not present. For interactions, positive beta-weight interaction scores mean that the stressor–DV relationship is more positive at higher levels of wealth than at lower levels of wealth. Negative beta-weight interaction scores mean that the stressor–DV relationship is more negative at higher levels of wealth than at lower levels of wealth. For U.S. states, *N* = 50. For Across Nations, whole sample *N*s range from 177 to 236; smaller sample *N*s range from 137 to 140.

^{††}*p* < .15. **p* ≤ .05. ***p* ≤ .01. ****p* ≤ .001.

(see, for example, Conway et al., 2008). As can be seen in Table 5, these tests in the main confirmed that the origins of vertical restriction significantly differed from the origins of horizontal restriction. It is noteworthy that in all three tests of the overall ecological stress measurements both within the United States and across nations, ecological stress was significantly more likely to increase vertical restriction and decrease horizontal restriction (all *ps* < .01).

We further computed all analyses in Table 5 while controlling for the other type of governmental restriction (e.g., for the vertical legal restriction, ecological stress correlation, we controlled for horizontal legal restriction). These analyses revealed that all significant *Z* tests in Table 5 remained significant when accounting for the overlap between vertical and horizontal restriction. This demonstrates that the different reported origins of vertical and horizontal legal restriction are independent.

Indeed, although imperfect (as is the case in most large archival datasets), the overall similarity across multiple stressors is nonetheless noteworthy. Table 6 captures this overall pattern: Both within the United States and across nations, the cumulative measure of ecological stress was positively correlated with vertical measurements of restriction, but negatively correlated with horizontal measurements of restriction (individual measurements were significant in all but one case, and all Steiger’s difference tests were significant at *p* < .01).

Do the stressors independently predict legal restriction? Given the current debate surrounding the independent contributions of pathogens, climate, and frontier topography to the development of freedom (e.g., Murray, 2013; Van de Vliert, 2013a; de Oliveira Chen & Kitayama, 2013), it is not surprising that researchers have called for more tests of the predictive validity of these different approaches (see, for example, Murray,

2013; Van de Vliert, 2013a, 2013b). Thus, to help fill in this gap, we ran additional regression analyses where we entered all five ecological stressors into a series of simultaneous regressions with the governmental restriction indices as dependent measures (both within the United States and across nations). Results of these analyses are presented in Table 7. As can be seen there, while the strength of the effects of each stressor generally (and predictably) went down, many significant effects remained. In terms of the strength of effects, (a) frontier topography measures were the best predictors for vertical restriction across U.S. states, followed closely by climate measures; (b) climate measures were the best predictors of vertical legal restriction across nations; (c) inland topography, heat stress, and pathogens were the best predictors of vertical political restriction across nations; and (d) cold climate was the best predictor of horizontal legal restriction across nations.

It is also worth noting that significant zero-order pathogens measurements became essentially zero in the regressions for the U.S. states, and that the two frontier topography measurements did not consistently show the same pattern in these analyses.

Does wealth modify the effects? We computed Ecological Stress × Wealth interactions on governmental restriction measures (for each ecological stressor separately). Results are presented in Tables 6 and 8. As can be seen, we replicated the effect of Van de Vliert (2013a) for cold stress on vertical political restriction—cold stress leads to more restriction in poor countries, but more freedom in wealthy countries. More importantly, we further corroborated Van de Vliert’s theory with entirely new data: Although weaker, the same significant Cold Climate × Wealth interaction held for our new measurement of vertical legal restriction across nations.

Table 7. Investigation 2: Stressors' Independent Contributions to Vertical and Horizontal Restriction Across Both Nations and States While Controlling for the Other Stressors.

Stressor type	Parasite	Climate		Frontier geography	
	Pathogens	Hot	Cold	Mountains	Inland
Across U.S. states					
Vertical legal restriction	.19	.41**	-.28*	-.65***	.48***
Horizontal legal restriction	.11	-.33††	.08	.05	-.32*
Across nations					
Vertical legal restriction	.24*	.23**	-.35**	.06	.06
Vertical political restriction	.25††	.19*	-.12	-.01	.31**
Horizontal legal restriction	-.16	-.17††	.38**	-.07	-.11

Note. Descriptive metric = standardized *beta*. For U.S. states, $N = 50$. For Across Nations, Vertical Legal $n = 139$; Vertical Political $n = 139$; Horizontal Legal $n = 136$.

†† $p < .15$. * $p \leq .05$. ** $p \leq .01$. *** $p \leq .001$.

Furthermore, our results revealed an additional novel finding that a similar interaction holds for frontier topography measures, an effect that was particularly strong for mountain terrain. For vertical legal restriction, this is corroborated by the overall ecological stressor interactions presented in Table 6.

However, no significant interaction effects emerged for horizontal restriction across nations, nor did any significant interactions emerge for any individual measurement across U.S. states (and the only significant interaction for U.S. states—that for the cumulative ecological stress measurement with vertical restriction—was in the opposite direction).

Investigation 3: What Accounts for the Ecological Stress–Freedom Relationship?

Investigation 2 showed results largely consistent with a dual-pressure model of ecological stress: Ecological stress increased vertical restriction while decreasing horizontal restriction.

However, this demonstration did not identify *why* these dual pressures might exist. This is important. The dual-pressure model focuses on commonalities across ecological stressors that produce similar opposing pressures in the case of each stressor. However, it is possible that the effects from Investigation 2 represent stressor-specific differences across each type of stressor and not the general effects suggested by a dual-pressure approach. If a common factor can be found that mediates the effects of each ecological stressor on both vertical and horizontal restriction—in other words, if the same factor mediates the opposing pressures for each stressor—this would provide more direct evidence that a factor common among stressors is driving the effects of Investigation 2. Such a common factor is what the dual pressure model would expect.

Investigation 3 focused on one such possible factor: assortative sociality. This factor is particularly useful to the

present investigation because, both conceptually and in its specific measurement operations, it is not directly a type of freedom (thus, it is not surprising that it has been considered separately from political freedom by researchers; see Fincher & Thornhill, 2012). From a phenomenological point of view, family ties (for example) are not directly related to political restriction—and yet, psychologically and conceptually, there are reasons to believe that they may be the mechanism by which ecological stressors produce dual pressures on freedom.

Thus, in Investigation 3, we explored the possibility that ecological stress operates on political restriction via assortative sociality. Because the primary conceptual mediator has far fewer measured nations, Investigation 3 had a lower N for across-nation analyses (ranging from 86 to 93; see Table 9).

Investigation 3 Method

Restriction and ecological stress measurements. For Investigation 3, we used the same governmental restriction and ecological stress measurements from Investigations 1 and 2 for U.S. states and across nations, respectively.

Assortative sociality measures

Across nations. Fincher and Thornhill (2012) produced four nation-level indices of assortative sociality: religiosity, family ties, national identity, and a synthetic combination variable they referred to as *ingroup assortativeness*. These four measures contain many missing values and many non-overlapping cases. As a result, they first were analyzed separately. However, these four assortative sociality measures show (a) extremely high correlations with each other (range = .66 to .94; average $r = .80$) and (b) similar predictive validity as mediators. As a result, we averaged them into a single score representing each nation's total *assortative sociality*. To maximize the number of nations we could use, missing values were replaced with the average of the other variables. This approach is appropriate when values are very highly correlated (see, for example, Simonton, 2006).

Table 8. Investigation 2: Interaction of Key Ecological Stressors With Wealth.

Stressor type	Parasite	Climate		Frontier geography	
	Pathogens	Hot	Cold	Mountains	Inland
Across U.S. states					
Vertical legal restriction	.03	.06 ^{††}	.01	.04	.05 ^{††}
Horizontal legal restriction	.05	-.05	-.10	-.02	-.07
Across nations					
Vertical legal restriction	.12*	.08 ^{††}	-.09*	-.07 ^{††}	-.08 ^{††}
Vertical political restriction	.26 ^{††}	.30 ^{***}	-.49 ^{***}	-.45 ^{**}	-.21 ^{††}
Horizontal legal restriction	-.07	-.11 ^{††}	.05	.07	.01

Note. Positive (unstandardized) beta-weight interaction scores mean that the stressor–DV relationship is more positive at higher levels of wealth than at lower levels of wealth. Negative beta-weight interaction scores mean that the stressor–DV relationship is more negative at higher levels of wealth than at lower levels of wealth. For U.S. states, $N = 50$. For Across Nations Vertical Legal–Pathogens $r, N = 191$. For Across Nations Vertical Legal–Hot/Cold $r, N = 224$. For Across Nations Vertical Political–Pathogens $r, N = 188$. For Across Nations Vertical Political–Hot/Cold $r, N = 189$. For Across Nations Horizontal–Pathogens $r, N = 175$. For Across Nations Horizontal–Hot/Cold $r, N = 177$.

^{††} $p < .15$. * $p \leq .05$. ** $p \leq .01$. *** $p \leq .001$.

U.S. states. Fincher and Thornhill (2012) produced three state-level indices of assortative sociality: religiosity, family ties, and a synthetic variable which they refer to as *ingroup assortativeness*. These three measures are generally highly correlated and thus were standardized and averaged into a single measure of *assortative sociality* (standardized $\alpha = .87$).¹²

Investigation 3 Results and Discussion

Assortative sociality was correlated in the expected direction with our restriction measurements across nations: It was positively correlated with both vertical restriction measurements ($r_s > .47, p_s < .001$) and negatively related to horizontal restriction ($r = -.57, p < .001$). This pattern was also in the expected comparative direction for the U.S. states; however, the relationship for horizontal legal restriction was not negative as predicted: There was a significantly positive correlation between assortative sociality and vertical restriction ($r = .48, p < .001$) and a lower correlation for horizontal restriction ($r = .21, p = .127$).

To test for the mediating effect of assortative sociality, we computed indirect effects of assortative sociality on each ecological stressor→restriction relationship via both bootstrapping (1,000 samples) and Sobel tests (Hayes, 2013; Sobel, 1982). These results are presented comprehensively in Table 9, with bootstrapped effect sizes for the indirect effects and inferential tests based on Sobel tests.

As can be seen there, the results are mixed. Across nations, there are consistently significant indirect effects of assortative sociality on the ecological stress→restriction relationships. Evidence from the cumulative ecological stress measures across nations suggests that ecological stress (a) increases vertical restriction in part by increasing assortative sociality (indirect effect $p_s < .05$) while (b) decreasing horizontal restriction in part by increasing assortative sociality (indirect effect $p < .001$). This pattern is especially important because a single factor common to all of these stressors not

only mediated ecological stressors' increasing one kind of restriction; it also mediated (in ways consistent with the dual pressure model) ecological stressors *decreasing* another kind of restriction. This effect thus suggests a common underlying mediator that applies across multiple stressors for dual opposing effects.

As with the overall pattern from Investigation 2, cold stress continued to be an exception to the overall trend. Cold stress showed significant indirect effects in the opposite direction, such that cold stress lowered vertical restriction by decreasing assortative sociality, but the opposite pattern occurred for horizontal restriction (indirect effect $p_s < .001$).

However, for U.S. states, very little evidence exists of a consistent pattern for assortative sociality mediating the ecological stress→restriction relationships. For the overall ecological stress variable, there were essentially no indirect effects for either restriction measurement, and almost none of the individual stress measures showed consistent or strong effects. The primary exception was cold stress, which showed a significant indirect effect ($p < .001$) for the cold climate→vertical restriction relationship, in a direction consistent with that across nations. However, unlike for across-nations analyses, this indirect effect was in the same negative direction (albeit only approaching conventional levels of significance using normal tests) for the cold climate→horizontal restriction relationship.

What are we to make of the general mediational differences across U.S. states and nations? It is clearly too early to know for sure. It is possible that it represents a cultural difference in the meaning of the variables; variables do not always “translate” the same across cultures as they do within culture (see, for example, Conway et al., 2006, for discussion). It is also possibly a result of having a larger and more diverse sample across nations than within the United States. While there are reasons to think that ecological variables should have stronger effects at smaller units of analysis (see Conway et al., 2014), in the present case, this might be

Table 9. Investigation 3: Testing Mediation Models of the Ecological Stress–Governmental Restriction Relationship via Bootstrapped Indirect Effects.

Stressor	Vertical		Horizontal
	Vertical legal restriction	Vertical political restriction	Horizontal restriction
Across U.S. states			
Pathogens	.07 [−.01, .19]*		.04 [−.19, .21]
Hot	−.02 [−.06, .00]		−.01 [−.08, .00]
Cold	−.11 [−.22, −.05]***		−.11 [−.24, −.00]††
Mountains	−.01 [−.05, .02]		−.01 [−.05, .01]
Inland	.00 [−.02, .05]		.00 [−.02, .06]
Cumulative stress	−.03 [−.09, .05]		−.02 [−.17, .01]
Across nations			
Pathogens	.23 [.14, .35]***	.23 [−.10, .67]	−.16 [−.27, −.06]**
Hot	.24 [.15, .35]***	.49 [.20, .89]**	−.19 [−.32, −.1]***
Cold	−.25 [−.40, −.12]***	−.73 [−1.22, −.32]***	.20 [.09, .36]***
Mountains	.18 [.07, .31]**	.33 [.12, .62]**	−.13 [−.25, −.06]**
Inland	.11 [−.03, .24]††	.16 [−.01, .38]††	−.06 [−.14, .01]††
Cumulative stress	.46 [.25, .72]***	.61 [.21, 1.26]*	−.30 [−.59, −.06]***

Note. Descriptive metric = bootstrapped unstandardized beta. Positive scores mean inclusion of the M variable reduced the effect of X→Y; negative scores mean inclusion of the M variable increased the effect of X→Y. Numbers in brackets represent the lower and upper confidence intervals for the reported effect using bootstrapping methods. For U.S. states, N = 50. For Across Nations Vertical Legal–Pathogens, N = 90. For Across Nations Vertical Legal–Hot/Cold, N = 93. For Across Nations Vertical Legal–Mountains, N = 93. For Across Nations Vertical Legal–Inland, N = 86. For Across Nations Vertical Political–Pathogens, N = 89. For Across Nations Vertical Political–Hot/Cold, N = 90. For Across Nations Vertical Political–Mountains, N = 90. For Across Nations Vertical Political–Inland, N = 86. For Across Nations Horizontal–Pathogens, N = 87. For Across Nations Horizontal–Hot/Cold, N = 89. For Across Nations Horizontal–Mountains, N = 89. For Across Nations Horizontal–Inland, N = 84. For Across Nations Vertical Legal–Total Stress, N = 93. For Across Nations Vertical Political–Total Stress, N = 90. For Across Nations Horizontal–Total Stress, N = 89. ††p < .15. *p ≤ .05. **p ≤ .01. ***p ≤ .001.

overwhelmed by the lower variability in the U.S. sample (see online appendix). Whatever the cause, however, it is worth noting that (a) the lack of a mediational effect in the United States should not invalidate the mediational test across nations, which showed clear and consistent evidence of the mediational effect of assortative sociality on the ecological stress→restriction relationship. (b) Furthermore, it should not invalidate the ecological stress→restriction relationship within U.S. states from Investigation 2. Instead, it suggests only that we do not yet have a clear partial explanation for it—but all psychological and behavioral phenomena have multiple causes, and it may be that the differences observed in Investigation 2 in the United States are explained through similar dual pressures on help-seeking versus ingenuity (or other similar opposing pressures).

General Discussion

Political freedom is not a unitary construct (Conway et al., 2006; Guss, 2013; Pemstein, Meserve, & Melton, 2010; Van de Vliert, 2013b). Yet, in spite of this fact, very little research attempts to empirically assess different types of political freedom and why those types of freedom matter.¹³ The present three investigations show that, not only are different types of political freedom viable constructs with different properties, evaluating those different types of freedom might

help us understand at a broad level the farther-back ecological origins of freedom.

Ecological Origins of Freedom

Investigations 2 and 3 are unique in that they simultaneously examine two conceptual factors that are important to the ecological stress→political freedom relationship: Type of stressor (pathogens, climate, and frontier topography) and type of legal restriction (horizontal vs. vertical). As such, they allow us to more closely examine the contributions of specific stressors versus the dual-pressure effects of general ecological stress.

Below, we first elaborate on the contributions to stressor-specific theories, and then discuss how we can reconcile stressor-specific effects with a general dual-pressure model of ecological stress.

Novel Contributions to Prior Stressor-Specific Theories

Independent of the generalized effects of ecological stress (discussed below), this research is an important contribution to the literature in several regards. First, researchers have called for more comprehensive tests of various ecological theories (see, for example, Murray, 2013; Van de Vliert,

2013a, 2013b). The present study is the first to our knowledge to test pathogens, climate, and frontier topography theories simultaneously, and to do so using two freedom-based constructs that were developed independently. Thus, this is the most comprehensive test to date of the predictive power of ecological stress—it tests three separate theoretical markers (pathogens, climate, frontier topography) on two separate conceptual types of restriction (vertical, horizontal) on two different units of analysis (within a nation, across nations). It further tests (a) the modifying power of wealth and (b) the mediating power of assortative sociality on each of these relationships.¹⁴ Combined, these investigations provide an important contribution to a literature in need of independent and comprehensive simultaneous comparisons.

While predictably somewhat messy, in the main, these analyses suggest that these different ecological stressors are independently contributing to aspects of legal restriction—even in a very conservative test of that idea that includes multiple overlapping indicators at once—in ways generally consistent with preexisting stressor-specific theories. However, the present work also showed that each of these stressors' effects on freedom—at the very least—needs to be qualified by the type of freedom under consideration. Thus, the present work provides an important contribution to each stressor literature independently. We first briefly consider the contribution of the current work to each stressor in turn by focusing discussion on the available data. We reserve most of our general theory-focused interpretations for our discussion of the dual-pressure model.

Pathogens. A large array of work suggests that pathogens increase authoritarian tendencies that are consistent with vertical restriction (e.g., Murray & Schaller, 2010). However, while Varnum (2014) demonstrated that pathogens decrease generalized trust, no work to our knowledge has demonstrated that pathogen prevalence decreases horizontal forms of legal restriction. Although the comparative difference between vertical and horizontal legal restriction was larger across nations than for the U.S. states, the overall pattern is consistent with the notion that while pathogens increase the likelihood of vertical restriction (consistent with prior work), that effect is not uniformly applicable to all forms of restriction.

We suggest below that this reversal of the normal pathogens effect is best understood in light of a dual-pressure model. But it is worth noting that, whatever its origins, it provides an important qualifier to prior work on pathogens. Pathogens do indeed decrease freedoms—of a certain kind. However, they actually *increase* other kinds of freedoms. When the freedoms in consideration are those that apply widely across large groups of people in a horizontal fashion, pathogens are precursors to *more*, and not less, freedom.

Climate. When considering the effect of climate on culture, there has been a tendency to focus on cold stress and/or to lump extreme hot and cold climates together (see Van de

Vliert & Tol, 2014, for a discussion). However, more recently, there is an increasing realization that extremely hot and extremely cold climates may share unique challenges with unique impacts on cultural development. For example, Van de Vliert and Tol (2014) demonstrated that heat stress predicted increased autocratic governance, but cold stress did not.

Our present data both support and extend the general conclusion that heat stress and cold stress have different effects on the development of freedom. We first provide a conceptual replication of Van de Vliert and Tol's (2014) finding that extremely hot environments tend to produce more autocratic governments and that extremely cold environments do not. In our work, hot environments are an ecological precursor to vertical restriction, and cold environments are either unrelated to vertical restriction or are a negative predictor.

However, we also demonstrate the entirely conceptually novel finding that this effect depends starkly on the type of restriction under the microscope. Indeed, this pattern exactly reversed for horizontal forms of legal restriction—heat stress predicts reduced horizontal restriction, while cold stress predicts increased horizontal restriction. The predictive validity difference is true for both heat stress and cold stress when controlling for the relationship between the two types of legal restriction, suggesting independent climatic origins of these two types of legal restriction. We explore possible reasons for the unique pattern of cold stress below—but however construed, these results importantly qualify prior results based on vertical approaches to governmental restriction.

Frontier topography. Prior work on frontier topography shows that it is associated with an independent ethos that causes it to be negatively related to both cultural collectivism and governmental restriction (Conway et al., 2014). Indeed, much prior theory and research on frontier psychology suggests that frontier ecologies should increase independence and freedom due to such harsh ecologies necessitating more independence for survival (Kitayama et al., 2010; Kitayama et al., 2006; Plaut et al., 2002; Plaut, Markus, Treadway, & Fu, 2012). Consider this representative summary from Kitayama et al. (2010), who commented on the psychological impact of the U.S. frontier:

Accordingly, settlers on the U.S. frontier often faced harsh conditions without the benefit of social infrastructures to promote their safety and survival (e.g., Stegner, 1953; Stewart, 1963; Turner, 1920). In all likelihood, to be successful and to survive, the settlers had to develop strong psychological propensities toward self-promotion, self-initiative, and self-determination and autonomy . . . (p. 567)

The present work suggests that when the psychological effects of the frontier are translated to political freedom, this expected frontier→freedom path is indeed partially accurate—but in need of qualification. Specifically, it demonstrates that this pattern occurs only for horizontal forms of

restriction. Frontier topography did lead to more freedoms from horizontal forms of restriction, but (like pathogens and heat stress) frontier topography leads to less freedom from vertical governmental restrictions. We discuss below one theoretical framework for understanding this effect—but irrespective of that particular model, this novel finding helps provide important qualification to prior work on this specific stressor. Because frontiers produce more vertical governmental restriction (in direct contrast to the prediction of Kitayama et al., 2010, who suggested it should lead to less vertical restriction), our theorizing about this specific stressor needs to be revised.

The Dual-Pressure Model

How are we to explain and understand these new findings? While it is possible to construe them each as stressor specific, here we discuss the possibility that ecological stress shares general properties common across stressors, and those general properties help account for the pattern observed here. Although they may do so in very different ways, pathogens, heat stress, and frontiers all nonetheless provide challenges to resource acquisition and to survival. All make it more difficult to engage in day-to-day activities in a pleasant fashion. All provide some need for order, cognitive resources for planning, the need for help from close others, and the tightening of resources across populations.

It is thus possible that the specific patterns observed for those stressors result from *commonalities* among these various stressors exerting differential pressure on vertical and horizontal legal restriction. The present results provide partial support for this idea. Indeed, given the different levels of analyses involved, the overall similarity in pattern is impressive and suggests real connection between commonly shared factors in ecological stress and different forms of governmental restriction in a manner consistent with a dual-pressure model. It may be that ecological stress puts pressure to have rigid rules protecting one's self and one's own local group, while putting pressure to avoid horizontal restrictions that might spread resources too thin across too many people.

Reconciling the Dual-Pressure Model With Stressor-Specific Cold Climate Effects

Although in the main our results supported the dual-pressure model of ecological stress, there were some stressor-specific effects as well, most notably, that for cold stress. Indeed, in the context of all the stressors studied in the present work, cold stress produces a puzzle. Why might cold stress markedly differ from other stressors in its relative effect on vertical versus horizontal restriction, and can (and if so, *how* can) this be reconciled to the dual-pressure model? We here pursue two possibilities: (a) that the dual-pressure model is wrong, or (2) that the dual-pressure model is largely right but cold stress provides an exception to the general rule—an

exception that is anticipated by climato-economic theory (e.g., Van de Vliert, 2013a).

It is certainly possible that the dual-pressure model is simply wrong and that our results represent nothing but stressor-specific effects. This possibility would entail having at least three separate stressor-specific theories (pathogens, heat stress, and frontier topography) that would lead to the same high-vertical/low-horizontal outcome for reasons other than factors that could be construed as common among generalized stress.

There is no way to definitively rule out this possibility, but a few things give us reason to suspect that more than stressor-specific effects (effects that would only apply to those specific stressors) are occurring. (a) Parsimony makes the stressor-specific approach somewhat less plausible—it would require separate illumination of stressor-specific effects that would *only* apply to those specific stressors, and not to the other stressors. (b) In our larger and more robust sample of nations, we in fact find a factor (assortative sociality) common to all four stressors in question (pathogens, heat, mountains, inland) that accounts for quite a bit of the variance in each ecological stress→restriction relationship (see Table 9). Although we failed to find such a factor in the U.S. sample, this at least suggests for one of our samples an empirically plausible mechanism that can sometimes operate for multiple stressors in the same way. (c) Although not comprehensive, the present work provides a large simultaneous test of three of the major groups of ecological stressors—and the combined measure of ecological stress (including cold stress) suggests a general tendency for ecological stress to increase vertical but decrease horizontal forms of governmental restriction. Thus, it is perhaps better to construe the relationship between stressor-specific and general effects as one in which a main effect exists for overall ecological stress that is qualified by stressor-specific effects some of the time—in this case, for cold stress. Future data can better address this question.

Whichever possibility turns out to be true, it is still worth exploring why cold stress differed from the other stressors. It is important to note that, while cold stress is fairly strongly negatively related to heat (in our sample, $r = -.57, p < .001$) and pathogens ($r = -.52, p < .001$)—and thus it would be natural to expect a different pattern—cold is actually positively related to both markers of frontier topography (mountains $r = .30, p < .001$; inland $r = .21, p = .014$). This suggests that it is more than just the conceptual and practical overlap between cold and harsh frontier environments that is driving the effect—because when frontier topography is separated from cold stress, it shows the exact opposite pattern.

So what is driving the difference? Both prior theorizing about climate stress (Van de Vliert, 2013a) and our own data offer clues. As a starting point, Investigation 3 suggests that cold stress relates differently to assortative sociality than most other stressors. Unlike the other stressors, cold climate stress produces *less* assortative sociality. Although this

assortative sociality difference could occur for many reasons, one might involve differences in wealth: Wealth was negatively correlated in our work with assortative sociality both across nations and in U.S. samples ($r_s = -.60$ and $-.43$, respectively). As others have pointed out (e.g., Van de Vliert, 2013a), cold nations tend to be disproportionately wealthy—and indeed, in our sample of nations, harsh cold was the only ecological stressor positively correlated to wealth across nations (cold–wealth $r = .38$, all other stressors $r_s < -.15$) and was one of only two positively correlated in the U.S. sample (and was by far the most positively related, cold–wealth $r = .26$, all other stressors $r_s < .06$). Thus, the increased wealth of cold nations/states may produce a pattern of assortative sociality that causes it to differ from other stressors.

It may more generally be the case—as the stressor-specific climato-economic theory predicts (see Van de Vliert, 2013a)—that wealth helps offset ecological stress in a somewhat unique manner for cold stress. Indeed, consistent with this, Van de Vliert and Tol (2014) have shown that cold stress effects are more affected by wealth than heat stress effects. Van de Vliert (2013a) summed up the argument that wealth operates differently for cold stress than for other stressors:

Different hardships typically require distinct psychobehavioral adaptations (e.g., Cottrell & Neuberg, 2005) and different uses of monetary resources as adaptational tools (Parker, 2000; Sachs, 2000; Sen, 1999; Van de Vliert, 2009). Money is more of a sine qua non for heating and eating in colder regions and months, but it is more for preventing and recovering from diseases produced by substances, germs, bacteria, and insects in hotter regions and months. (p. 467)

How might this fit in with the dual pressure model? One possibility is that both models complement each other and that the effects of dual pressures for cold stress would be most likely seen under conditions of low wealth. To the degree that (a) dual pressures operate for all ecological stressors and (b) wealth interacts differently with cold stress than other types of stressors (as climato-economic theory suggests), this suggests the corollary that (c) in poorer countries, the cold climate→governmental restriction effects may be closer to the ecological stress patterns found for the other ecological stressors. In other words, dual pressures may be occurring for cold stress just as they do for other stressors, but (for that stressor only) wealth may uniquely offset and redirect the force of the stress.

And, in fact, as shown in Table 8, that is precisely what our data show. As an exemplar, for one interaction—that for vertical political restriction across nations—our data reveal an inferentially more compelling positive relationship for the poorest third of nations (cold climate→vertical restriction $beta = .50$, $p = .002$) than for the negative relationship among the richest third (cold climate→vertical restriction $beta = -.40$, $p = .021$). While this pattern is not nearly as compelling for the other measures, it is in the same comparative direction.

Although not definitive, this is consistent with the possibility that, while the effects of ecological stress may be reversed for cold stress, it nonetheless shows a pattern of relationships consistent with the dual pressure model when wealth is lower. In other words, one possibility is that dual pressures of ecological stress are always present for all kinds of ecological stress, but wealth may play a more important role in offsetting those pressures for cold stress than for other kinds of ecological stress. This perspective validates the importance of both models. It suggests that cold stress may show the same pattern as other stressors when wealth is not present, thus highlighting both the value of the dual pressure model and the importance of climato-economic theorizing.

Concluding Thoughts

Archival data are often messy. And indeed, there are many reasons that the present set of studies would not have yielded a consistent pattern of results. We used two completely different units of analysis (across-nations vs. within-nation). We used measurements of the constructs that had no direct overlap except as conceptually related to the construct (e.g., vertical legal vs. vertical horizontal restriction across nations). Even for methodologically similar measurements, we did not always have the exact same set of laws (e.g., horizontal restriction measurements for nations vs. U.S. states).

Yet, in spite of these potential pitfalls, a remarkably similar pattern emerged. That pattern suggested that, for all but cold stress, ecological stressors cause an increase in vertical forms of restriction, but simultaneously cause a decrease in horizontal forms. Although our data only partially support an explanation based on assortative sociality, they nonetheless do more fully suggest that, whatever the exact mechanism, there is clearly a general tendency for ecological stress to differentially affect different forms of freedom.

This is no small gap to fill in the literature. It is important to remember that although horizontal restrictions may seem “milder” subjectively, they nonetheless provide governmental restrictions of human freedom. Indeed, there is one sense in which horizontal restrictions are actually the more pervasive form of restriction: By definition, horizontal legal restrictions apply to *more* of the populace than do vertical restrictions. Thus, when considering where freedom comes from, it is not insignificant that pervasive restrictions such as those in the horizontal restriction category show very different consequences of ecological stressors than do vertical restrictions. This work thus highlights, at the very least, the importance of considering the ecological origins of freedom in a less monolithic way.

Acknowledgments

The authors would like to thank Evert Van de Vliert, Mark Schaller, Corey Fincher, and Michael Varnum for sharing data and ideas relevant to the present article.

Declaration of Conflicting Interests

The author(s) declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Funding

The author(s) received no financial support for the research, authorship, and/or publication of this article.

Supplemental Material

Supplementary material is available online with this article.

Notes

1. This is a brief big-picture summary that oversimplifies theory and research on climate stress (e.g., Van de Vliert, 2014)—we discuss the theory's specific predictions later in this article.
2. We recognize that some stressor-specific differences might explain part of the variance. For example, it is possible that pathogens produce less ingenuity because they have historically not been understood and are invisible. However, our larger point is about the psychological and conceptual possibilities: Whether understood or not, pathogens nonetheless produce sick people—a very visible marker that something is wrong—and an increase in the number of sick people might drive medical advances and ingenuity (and the exploration of novel approaches to medicine) that might not exist where disease is less prevalent.
3. These reflections were a part of an iterative process that involved exploring the data presented in this study (and other data from other studies) and using that exploration to consider the origins of ecological freedom. We present them in the introduction for the sake of narrative coherency and not to imply firm predictions that were generated entirely in advance of looking at the data.
4. For this second kind of validity, the line between what is considered a “substantive finding” and a “validity test” is determined in part by the theoretical context. Our theoretical context is studying ecological precursors to freedom. As such, for our purposes, establishing theoretically meaningful relationships between our restriction measurements and other cultural constructs serves as a validity test.
5. These estimates were developed entirely independently from the development of the indices themselves. They are not direct markers of the laws but of the population the relevant laws would most likely affect.
6. Correcting for unequal variances changed this result very little, $t = -3.28$, one-tailed $p = .030$.
7. The U.S. Horizontal Legal Restriction and Collectivism relationship has been reported before (Conway et al., 2006). Indeed, throughout, some of the findings reported in this article have been directly reported before; some of them are essentially independent replications of prior findings on nearly identical datasets, while the majority are entirely new findings. When the findings have been reported before, we have attempted to make this clear. Most of the findings reported in the article are nonetheless novel, and we have opted to report the nonnovel findings alongside the novel ones for ease of comparison, presentation, and understanding. Part of the purpose of this article is a side-by-side comparison of vertical and horizontal measurements, and that includes some existing data.
8. For nation-level results, we also performed analyses using the index generated by Schaller and Murray (2008). These analyses were largely identical to those reported in the “Investigation 2 Results and Discussion” section.
9. In the online appendix, we present more information on the cumulative stress index, including dispersion measurements within the United States and across nations, correlations between individual stressors and the overall measurement, and the individual ecological stress score for each state or nation.
10. Note that the pathogens-vertical political restriction (Murray & Schaller, 2010), climate-vertical political restriction (Van de Vliert, 2013a), and frontier topography-horizontal legal restriction (Conway et al., 2014) findings in the table have been reported before in some capacity, although the latter finding here is reported for a substantially updated measurement. All other findings in the table are, to our knowledge, novel.
11. We also computed all correlations in Table 4, for both U.S. states and across nations, while controlling for wealth. The pattern remained identical to that reported, and all currently significant correlations remained significant.
12. We used this method of aggregation for both states and nations (instead of only applying Fincher & Thornhill's synthetic measure) because it maximized the N we could include in analyses. Our summary scores were almost perfectly correlated ($r_s = .97$ and $.99$) with the synthetic variable, and initial analyses using their synthetic variable revealed a pattern identical to that using our summary variable.
13. Pemstein et al. (2010) provide a possible exception—they investigated 10 different measurements of democracy. However, although they acknowledge the possibility of specific conceptual attributes, they do not formally test any ideas related to the dimensions of freedom. That is in line with their focus on combining measurements and not conceptual dimensions. This highlights our point here: Even when the complexity of freedom is recognized by researchers, little formal work empirically distinguishes different forms of freedom in a conceptually focused manner.
14. It is possible to view assortative sociality as a type of freedom restriction. While we recognize the potential overlap, the measurements of assortative sociality used in the present article are not directly related to political freedom, but are instead measurements of religiosity, family ties, and national identity. In our view, there is no necessary connection between such measurements and political freedom. Furthermore, at a functional level, these measurements bear little direct phenomenological overlap with the structural political restriction measurements used in the present study. As such, we feel it makes sense to consider assortative sociality measurements as intermediaries in the relationship between ecology and freedom, and not as another kind of freedom (in line with Fincher & Thornhill, 2012, who separate political freedom from assortative sociality). It is further worth noting that assortative sociality, in line with the dual pressure model, predicts freedom in opposite directions, depending on the type of freedom—as such, it does not clearly map onto a sociality = less freedom model (but instead conforms to the dual pressure model's more flexible approach).

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