# Regarding Holechek and Briske, and Rebuttals by Teague, Gill & Savory

## Correcting Misconceptions about the Supposed Discrediting of Savory's Approach

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## Abstract

This paper investigates the grazing management assessment reports authored by Briske (2008), and Holechek (2000) in light of their claims regarding methodologies for grassland restoration advocated by Allan Savory. Rebuttals to the Briske and Holechek conclusions are provided by Teague et al. (2008), Gill (2008, 2009a, 2009b, 2009c), and Savory (2000). The Briske and Holechek papers conclude that methodologies for grazing management that they attribute to Allan Savory are not advantageous. The refuting authors claim that Briske and Holechek, though correct in their assessment of the grazing systems evaluated (rotational and short duration), error in their association of those systems with Savory. The studies cited by Briske and Holechek, according to the refuters, represent rote grazing methodologies that trivialize the complexities of land and livestock interactions and are not representative of the Holistic Grazing strategy advocated by Savory (1999) in which planning and monitoring for grassland health are central tenets. In fact, contrary to discrediting Savory, the refuting authors claim, the Briske and Holechek studies actually advance Savory's theses. The studies prove exactly his claim that nonadaptive grazing systems will fail. Additionally, the refuters cite many cases of adaptive grazing management producing desirable environmental and economic results.

This paper finds the refuters' arguments have merit. The studies reviewed by Briske and Holechek were not evaluations of the Savory method of planned grazing and not reflective of its efficacy. Clarity on this matter is becoming increasingly germane within the environmental community where there is a growing interest in grassland restoration to mitigate global warming.

## Overview

Two papers, Briske (2008) and Holechek (2000), are often used as evidence to discredit the grazing approach advocated by Savory (1999). Although the Briske and Holechek papers are often cited, they are not without their critics. Strong refutation is provided by Teague et al. (2008), Gill (2008, 2009a, 2009b, 2009c), and Savory (2000) who present point-by-point counter arguments. Their principal claim is that the methodologies cited by Briske and Holechek are *not* those advocated by Savory, even if the wording has been borrowed, and that continued association of Savory with the practices followed in the studies is inexcusable. Most notably, the studies cited by Briske and Holechek are using stocking rates and grazing timings that are predetermined. Thus, by design, they cannot be adaptive to conditions on the ground, and would not be expected to enable land recovery.

Such grazing systems, claim Teague, Gill, and Savory, are the *antithesis* of what Savory advocates, which is management for maximum ecosystem health using a planning process tailored for that purpose. The Savory approach (1999) is a decision making process, not a regimen, that includes goals for the land, allows for adequate plant recovery, and moves livestock in a fashion that mimics the beneficial grazing and herding behavior of wild ruminants.

Typically, this *holistic* approach entails higher stocking densities, shorter grazing periods, and longer recovery times than are traditionally recommended, but, as nature dictates, they are different in each environment, season and year. Finding the proper impact for the particular ecosystem is the goal of the holistic rancher and the planned grazing methodology enables informed action toward that end. The whole point of this approach is to reverse the desertification that is exacerbated by conventional or “continuous” as well as rotational grazing (Savory 1999). As the refuting authors claim, without the proper planning and vigilant adaptation to conditions on the ground, deterioration from grazing is almost certain, and the actual herd densities or rotational schemes are immaterial.

Thus, contrary to proving Savory wrong, the studies of the type cited by Briske and Holechek, it is argued by their refuters, only prove what Savory is the first to predict, that grazing systems that don’t provide the animal impact grasslands evolved with, will degrade the land and contribute to biodiversity loss. Such studies, argue the refuters, offer no insight into how grasslands evolved nor how we can restore them to their natural conditions, which, not long ago, supported millions of mammals on soils meters deep.

Additionally, says Teague, the papers cited by Briske and Holechek are referring to small paddock studies that are not indicative of what happens on a ranch. Teague cites numerous studies, not mentioned by Brisk, that show an entirely different picture of ecosystem impact. In these cases, ranchers have ecological objectives and cattle interact with their environment in a more natural fashion. Teague makes the claim, echoed by the other refuters, that instead of extrapolating from unrepresentative paddock studies, we should direct our attention to the ranches around the world that are successfully managing for ecosystem restoration.

## Briske and Holechek

The Briske and Holechek papers are seminal in that they are widely cited synthesis papers which both claim to be the final word in the refutation of the Savory philosophy of grazing management. In their own words, they claim to have done an exhaustive review of literature and that the findings are conclusive. They conclude that the body of scientific papers published refutes the claim that rotational grazing (which they erroneously attribute to Savory) is superior to continuous grazing along the environmental and productivity factors measured, yet, both acknowledge that many studies show minimal to no differences and that in some circumstances rotational was actually better.

For example, Briske reports that in regards to plant production, 83% of comparative studies (19 of 23) found “no differences” and that of those that did report a difference, the majority reported favorably for rotation grazing. Similarly, with Holechek, regarding forage production, he reports that “Several studies now show that there is little difference in forage production between short-duration and continuous grazing if stocking rates are the same”, and in regards to plant succession, he states, that when stocking rates are the same, the studies show the systems to be “similar.” He even goes on to state that although increasing stocking densities by 2.5 times the recommended rate caused mid-grasses to decline, “shortgrasses were not affected.” None-the-less, in spite of the mild difference in performance (which is not surprising, seeing as none of the studies were adaptive), both Briske and Holechek use uncharacteristically charged language in refuting Savory, revealing an undertone of contentiousness. Examples are below:

Briske et al:

> “In spite of overwhelming experimental evidence to the contrary, rotational grazing continues to be promoted and implemented as the only viable grazing strategy.... These experimental data demonstrate that a set of potentially effective grazing strategies exist, none of which have unique properties that set one apart from the other in terms of ecological effectiveness.” (Briske et al, 2008).

Holechek et al:

> “In our search of the literature we could find no studies that substantiate Savory's claims on the benefits of hoof action on range soils.... It was concluded that using high animal density and stocking rates with time-controlled grazing would result in range deterioration.“
>
> “During this study, deferred rotation, time-controlled (short-duration), and season-long grazing did not differ in their effects on either forage production or plant succession.”
>
> “The hypothesis that time-controlled grazing with high stocking rates and high stock densities will improve rangeland condition was strongly rejected by the authors.” (Holechek, 2000)

The general hypothesis from Briske and Holechek is not that rotational grazing is inferior, but that claims of its superiority over continuous grazing are unsubstantiated. This in itself is fine. Savory has claimed as much. The problem is that both Briske and Holechek misrepresent Savory and the grazing management approach he is advocating. They reduce it to a recipe of cattle density, which he refutes in the clearest possible terms, and then, none-the-less, claim to be testing his methods. They cite studies that provide no plan for ecosystem recovery, no attempt at simulating herd impact suitable for the region, and no allowance for adaptation. As a result of the predictable failure, the interpretations from Briske and Holechek perpetuate both the misconception that Savory methods don't work, and of equal tragedy, the belief that continuous grazing, with its litany of environmental damage, is the best we can achieve. Neither is true. As Teague, Gill, and Savory himself will show, the finding from Briske and Holechek misrepresent what Savory is advocating, undermine the potential of what is possible, and ignore the results of successful ranchers.

## Rebuttal from Teague

Richard Teague is a professor with Texas A&M and a co-author of the Briske paper. He disagrees with the conclusions, stating that both the Briske and Holechek ignored abundant evidence to the contrary and that the studies reviewed are not representative of what happens in an actual range ecosystem. He has subsequently written his own paper in reply, titled, *Benefits of Multi-Paddock Grazing Management on Rangelands: Limitations of Experimental Grazing Research and Knowledge Gaps*. The paper was published as the lead chapter in Grasslands: Ecology, Management and Restoration, Nova Science Publishers, 2008. As Teague states,

> "The benefits of multi-paddock rotational grazing on commercial livestock enterprises have been evident for many years in many countries.... Many ranchers who have practiced multi-paddock grazing management for decades are very satisfied with the economic results and improvement to the ecosystem, as well as the change in management lifestyle and social environment of their ranch businesses. Such ranchers regularly win conservation awards from the ranching industry and natural resource professional organizations. In contrast, many grazing researchers have concluded that multi-paddock grazing offers no significant benefit over continuous grazing (Holechek et al. 1999, 2000; Briske et al. 2008), but their studies have been largely small-scale trials focused on the technical questions of ecological impacts and livestock production conducted in a relatively limited scope of fairly resilient landscapes. In addition, research plots are designed to reduce or eliminate variability, while ranch managers must manage in the environment with all the inherent variability of the landscape." (Teague et al., 2008)

Teague (2011) also has a subsequent paper in Agriculture, Ecosystems and Environment, titled *Grazing management impacts on vegetation, soil biota and soil chemical, physical and hydrological properties in tall grass prairie.* In direct opposition to Briske and Holechek, this papers finds a wide range of benefits attributed to "adaptive management and multi-paddock" grazing (MP), relative to "continuous" and nonadaptive grazing typical on ranches. Benefits

include increased ground cover, increased soil organic carbon, and increased fungal/bacterial ratios, which, according to Teague, represents increased water holding capacity and nutrient availability.

> "This study documents the positive results for long-term maintenance of resources and economic viability by ranchers who use adaptive management and MP grazing relative to those who practice continuous season-long stocking." (Teague, 2011)

**Understanding "Adaptive Management"**

The operative term in Teague's statement above is "adaptive management", and therein lies the distinction between the studies cited by Briske and Holechek and the principles actually advocated by Alan Savory and practiced by the successful ranchers referred to by Teague, Howell (2008), Gill and others. Adaptive management means that the grazing regime is responsive to the conditions on the land. It is not preset. Stocking densities, paddock size, and movement schedules are subject to a daily assessment of the local environment, including plant, soil, and weather conditions. At the heart of all of this is adequate plant recovery. If plants are eaten before they are fully recovered, they are overgrazed, and root structure dies back. If they are not eaten in time, and left to brown, then they are undergrazed. Their biomass is not converted to dung and recycled into the soil with ungulate stomach bacteria. In either case, the soil degrades and carbon is lost. The Savory method is all about timing based on plant growth and recovery. Where grazing is "adaptive" to this parameter, it is consistent with Savory methods and simulates the conditions in which the grassland evolved. Where it is not, as most aren't, it is not consistent with the Savory approach, and any such claim is a misrepresentation. Whether such systems practice high or low stocking density, or rapid or continuous grazing, is immaterial. If they are nonadaptive, they will fail, and it not surprising when they do.

**Regimented Paddock Study versus Adaptive Ranch Management**

According to Teague, the studies reviewed by Briske and Holechek are fundamentally flawed in that they do not represent what actually happens on the range. The land is not a factory and animal interaction with it is not a clockwork mechanism. To the contrary, grasslands co-evolved with grazing mammals and depend on their impact in a wide variety of climate regimes and migration patterns. Achieving maximum grassland and animal health on a range depends on developing a grazing plan that mimics as closely as possible the original conditions for that particular location while being adaptive to weather, land, and plant variability. Any regimented grazing system, or field study, whether continuous or short duration, that is nonadaptive to conditions on the range, necessarily departs from the natural co-interaction of animals and grass, and invariably results in environmental degradation. There should be nothing surprising about this, and indeed this is precisely what the studies find. Nonetheless, this fundamental fact is absent in both the planning and assessment of the efficacy of the studies, their conclusions, and applicability to range management.

> "Grazing ungulates have an entirely different impact on the landscape than that implied by Briske et al. (2008), as is well documented by work at the landscape scale we have outlined earlier in this chapter. This points to an entirely different and more meaningful way of designing and interpreting grazing trials" (Teague et al., 2008).

It follows, therefore, according to Teague, that meaningful comparisons are not between nonadaptive paddock studies (whether continuous or short duration), but between any such grazing system and ones which are adaptive to plant and land conditions and which have express goals to achieve maximum ecosystem results. When "holistically managed" regimens like this are in place for the whole ranch, the results are conclusive: adaptively managed, multi-paddock grazing improves forage and ecosystem health while supporting larger stocking rates and improved revenues. Teague makes the point that researchers who are interested in studying ecological restoration through grazing management and documentation of "best results" should direct their attention to the ranchers who are doing just that. They should stop extrapolating from non-representative samples and instead define protocols to properly capture what is happening in the real world with successful ranchers. Such an approach, concludes Teague, would provide insights for ecosystem health and range management that could be of practical use worldwide.

> "The numerous instances from research studies outlined in this document and evidence from scores of ranchers around the world provide solid reasons to modify the hypothesis expressed by Briske et al. (2008) that there is no reason to favor multi-paddock rotational grazing over continuous grazing and conservative stocking. Because hypotheses cannot be proved, only rejected, the role of science is to test alternative hypotheses or paradigms and specifically try to refute them." (Teague, et al., 2008)

> "Thinking in terms of grazing systems is far less important than understanding processes and determining how to achieve management goals using that knowledge...People are the glue that links soils, plants and herbivores in grazing systems, and if we really want to understand the innovation and integration essential to the successes of those relationships, we must understand what the best managers do." (Teague et al., 2008)

**Teague Criticism Summary**

Teague offers many criticisms of the Briske and Holechek papers. I am here grouping these into three main points.

1 **Small Paddocks not Representative of Actual Ranch Conditions:** Small paddocks with predetermined grazing schedules and stocking rates are not representative of what happens on a ranch ecosystem or farming enterprise, nor of natural animal and grassland interaction. They don't properly account for spatial and temporary variability in animal movement and impact. In fact, variability is intentionally removed.

> “Relatively small research paddocks grazed continuously to compare with rotational grazing do not mimic the continuous grazing of large paddocks...the conclusions of such research have been extrapolated to all pastoral situations, regardless of paddock size. Small-scale experiments are carried out as though paddock size doesn’t matter, and when the paddocks are only 20 ha or less, it doesn’t. Unless the issues of scale and spatial heterogeneity are included as treatments, experiments at only small scales do not represent what happens at the scales of commercial ranches.” (Teague et al., 2008)

2 **Nonadaptive. No goals:** The studies cited by Briske and Holechek, by definition, are nonadaptive, and thus cannot be sensitive to plant or soil conditions. They contain no goals for the land or enterprise. Ecological degradation is all but a forgone conclusion (from either overgrazing or over rest). In a natural environment, there would never be overgrazing nor over rest. There would be enough animals to prevent over rest, and enough pasture, and predators, to prevent overgrazing. Animals would herd in high densities, eat the best pickings, and move on. This is a naturally adaptive system in which grassland ecosystems achieve their maximum potential, both for forage production and ungulates populations. In such environments, pastures will have deep soils and riparian areas will have full cover. Grazing systems that circumvent adaptation to plant growth and soil health through a prescribed regimen (continuous or rotational) are destined to failure. Comparisons between them are meaningless. This is not planned grazing.

> “...researchers have often applied treatments that did not adequately consider physiological effects, complementary relationships among soils, plants, animal behavior, preferences and selectivity, and ecological processes like water and mineral cycles.… As we have indicated in this document, unless experiments have been conducted in a manner that aims at achieving the best plant and animal responses, the results will probably be misleading in defining the potential of an experimental treatment” (Teague et al., 2008).

3 **No Assessments of Successful Ranches:** As mentioned, the studies cited by Briske and Holechek are basically lab tests on paddocks running predetermined stockings. They are not assessments of grazing systems that are achieving breakthrough results on ranches, nor do they infer what could be possible or how the ecosystems existed in their natural state. According to Teague, there are examples worldwide of successfully managed ranches that are improving land health through planned, adaptive grazing, and have been doing so for decades. In these cases, there is an understanding of the land-animal interaction and grazing is managing accordingly. The small paddock and nonadaptive studies cited by Briske and Holechek do not assess these ranches and are thus misrepresentative.

> “In contrast to the conclusions of many researchers, numerous commercial livestock enterprises in many countries have used a basic knowledge of plant and animal

> physiology and ecology within an adaptive, goal-oriented management approach to implement successful planned grazing management programs.
>
> Managers need to know how to work adaptively within their operations to produce the best results and minimize inherent problems. Successful ranchers modify their management to achieve the best possible outcomes in terms of profitability and enhancing or maintaining ecosystem health. Researchers have much to learn by working with successful ranchers." (Teague et al., 2008)

## Gill Rebuttal

Chris Gill manages the 32,000-acre Circle Ranch in West Texas. He has been a rancher for 38 years, with management experience throughout Texas and Uruguay. According to Gill, under eight years of planned grazing in accordance with Savory's methods his ranch has increased forage production by more than 35 percent per year and almost tripped the animal days of grazing recommended by extension services. In other words, his land got healthier. He became aware of Holechek *et al* from a 2006 book titled Habitat Guidelines for Mule Deer, published by the Mule Deer Working Group of the Western Association of Fish and Wildlife Agencies. The book cited Holechek (and the papers he cites) as proof that Savory methods won't work and advises ranchers against using them. Seeing that his ranch was accomplishing exactly the results that Holechek said could not happen, he decided to read the scientific papers for himself. Gill's subsequent 24-page, point-by-point rebuttal was sent to all authors and sponsors of the mule deer book. He also published the full report on his Circle Range website and wrote summary articles that appeared in both *Range Magazine* and *In Practice.* There is no excuse for a serious researcher of the Savory vs. Holechek debate to overlook Gill's contribution.

Gill's rebuttal (2008) is comprehensive. He takes to task every paper cited, as well as the mule deer book editors, explaining in detail where they either misrepresent Savory, ignore the co-evolution of ungulates and grasslands (and the need for adequate plant recovery), make inapplicable extrapolations from paddock studies, and, most disturbingly, undervalue the experience of real ranchers. Addressing the last point, he gives numerous examples from his own ranch and provides access to meticulous records from a multi-year period. He also finds sloppy scholarship in the mule deer book itself.

Gill's format is to address a statement and then make a rebuttal. These statement-rebuttal pairs are grouped into categories germane to the debate, for example: herbivory (forage production), grazing (paddock size, soil health), and infiltration (hoof action). Below are examples:

**Regarding Herbivory**

> **"**Excessive Herbivory, page 11 (of the Mule Deer book), states in part: '(Allan) Savory… claimed that by grazing pastures intensively and moving stock frequently the range could actually be improved while simultaneously increasing the stocking rate. On some ranches

it was even claimed that stocking rate could be doubled or tripled with improvements to range and livestock productivity. Researchers during the last few decades have shown these claims to be invalid (Heffelfinger et al. 2006; Holechek et al. 2000).'" (Gill 2008)

**"To the contrary, this is precisely the outcome we have experienced** (emphasis added). After eight years under planned grazing at our 32,000-acre high-desert mountain Circle Ranch in Hudspeth County, far-West Texas, we take almost triple the animal days of grazing (AD's) possible from conventional stocking rates recommended by NRCS and Texas Parks & Wildlife. Conventional practice dictates a herd of 250 head for 365 days: 250 X 365 = 91,800 AD's. This year we are running 1000 head for 240 days: 1000 X 240 = 240,000 AD's; plus, 50 head for 180 days: 50 X 180 = 9,000. This totals 249,000 AD's, 270% of conventional results, virtually the **very result you say has been proven invalid** (emphasis added)." (Gill 2008)

**Regarding Grazing Tests**

"Jung examines a 105-day growing season program incorporating five grazing/rest cycles through 8 pastures (paddocks), with moves according to a pre-determined schedule. Grazing periods were 2-1/2 days and recovery periods 18-1/2 days long (Jung et al. 1985). **Rotations were not determined by actual plant conditions** (emphasis added)**.** Recovery periods are inadequate: in West Texas, weather warms in late April but rains seldom arrive before July. Since weather cools again in mid-September, the effective growing season is 75 days. At our Chihuahuan Desert ranch two full seasons are usually necessary for complete recovery and sometimes on parts of the ranch three to five years may pass without adequate rain for plant recovery. So cattle moves and grazing according to monitoring actual plant conditions is critical. By definition (see page 12) any spring grazing causes over-grazing. Even if, and after, rains begin, an assumption of full plant recovery after only 18-1/2 days, let alone that there will be five such recoveries annually, postulates the impossible. This system overgrazes for all three reasons contained in the definition of over-grazing found at page 12. To avoid repetition, consider the foregoing comments as implicit in any repetition of the next statement: 'This was not a test of planned grazing.'" (Gill 2008)

**Regarding Hoof Action and Infiltration Studies**

Holechek (2000) states that Savory's claims regarding hoof action and infiltration are the most researched, and begins his attack on this front, citing seven papers which reject the benefits of Short Duration Grazing. Gill (2008), thus, responds in kind, dissecting each study, even studies not cited by Holechek, but relied on by the respective authors. His refutation is robust and quoted below is only a partial sample.

"Holechek concludes that several studies prove that Savory's claims that short duration grazing will increase water infiltration into the soil compared to continuous grazing are

disproved... and that several studies have been quite consistent in showing that hoof action from having a large number of animals on a smaller area for short time periods reduce rather than increase infiltration (McCalla et al. 1984; Thurow et al. 1986; Weltz et al. 1986; Warren et al. 1986; Pluhar et al. 1987). These same studies have also been consistent in showing short-duration grazing increase erosion compared to continuous or season long grazing (Holechek et al. February 2000)".

"Short-duration grazing (SDG) is not planned grazing (Savory 1983; Savory et al. 1999; Butterfield et al. 2006). Let's look at each nevertheless."

"McCalla tested SDG's. McCalla found that 'livestock grazing can alter infiltration rates of rangelands soils by removing protective plant cover and by trampling.... Reduced infiltration rates as a result of livestock grazing have been attributed to (1) loss of vegetation cover, (2) decreased mulch cover, (3) decreased amounts of vegetation standing crop and mulch, (4) increased bare ground and (5) increased bulk density as a result of trampling' (McCalla et al., 1984). These observations are correct and predicted by planned grazing protocol. SDG causes over-grazing and degradation of plants."

"The runoff plots were pre-wet until they would not take any more water, then allowed to rest for 24 hours. Then the moist soil was tested for relative infiltration (McCalla et al. 1984). McCalla's methodology fails with respect to the most important aspect of planned grazing and soil trampling – the breaking of the crust that otherwise keeps rainfall from initially soaking into the top layer of soil (Thurow et al. 1986). Observers of rains in deserts know these generally fall on dry soils where capping, if present, sheds water. Once capped soils become wet in the desert, water soaks in more. **Testing uncapped wet soils to evaluate how capped dry soils absorb sudden rains is not a test of real conditions** (emphasis added)**.** Nor is it a test of the most important aspect of hoof action which is breaking the soil crust. **Planned graziers will stipulate to the self-evident: infiltration varies directly with plant community health** (emphasis added). It is the foliated plant, its root mass, litter cover, the associated insect tunnels, microorganisms, organic soil content, and lack of capping that slows, traps, absorbs, and allows water to soak into the soil instead of running off (Savory 1983; Savory et al. 1999; Butterfield et al. 2006; Heffelfinger et al. 2006; Thurow et al. 1986)."

…

"Warren et al studied a site devoid of vegetation, following periodic trampling of intensive grazing systems. They found that the deleterious impact of livestock trampling generally increased as stocking rate increased. Damage was augmented when the soil was moist at time of trampling. **Thirty days of rest was insufficient to allow hydrological recovery** (emphasis added). This study consisted strictly of bare soil inquiries. The trampling of the bare soil involved large animal numbers five times at 30-day intervals through the growing season with the soil wet before the trampling (Warren et al. 1986). **Planned graziers would not return large herds to bare soil five times during the same growing season**, nor in the desert could heavy rains be expected to coincide with such animal presence

(emphasis added). This does not test planned grazing. What was found is consistent with what planned grazing principles and common sense would predict."

"Pluhar conducted experiments in different scenarios and found that what helps plants helps infiltration. Infiltration rates were least in the spring, and most in the fall, because in the spring soil was dry and plant cover minimal compared to conditions at the end of the growing season (Pluhar et al. 1987). This is what planned graziers would expect: whatever system is tested that tends to bring animal numbers up for the shortest time possible to achieve the desired amount of grazing will be the one that works best. Not a test of planned grazing." (Gill 2008)

**Regarding the Misrepresentation of Savory by Holechek.**

Gill's strongest criticism appears to be reserved for Holechek, whom Gill says blatantly misrepresents Savory and continues the mythology that Savory is somehow a proponent of any grazing system that is predetermined, be it called Short Duration, Rotational, or anything else. Gill counters Holechek's characterization of what Savory implies with Savory's actual, and unambiguous words to the contrary. Particularly, here, being the matter of Short Duration Grazing (SDG) for which Holechek defines with specific parameters for paddock size and grazing schedule (such as 10 full cycles per year), without a word about planning for adequate plant recovery (Savory's central theme). He attributes this (non-adaptive) system to Savory, apparently even citing from works in which Savory explicitly refutes the very notion that such a plan could ever work. In Gill's words, this is a "re-definition", and "mischaracterization." As Savory's words to this extent have been in journals for decades prior to Holechek's paper, including Rangelands (which Holechek himself is published in) it's implied that Holechek was negligent in his scholarship.

"The Holechek study defines planned grazing as a SDG wagon wheel system, with shared water, no fewer than eight paddocks, 5-days- or-less grazing with 4-weeks-or more-nonuse: about 10 full cycles per year using a schedule of predetermined moves (Holechek et al. 2000)."…

"Here is what Allan Savory said about such SDG systems in the August 1983 issue of Rangelands.

> *'As a general rule... the government prescribed stocking rates can safely be doubled in the first year of operation* ***as long as adequate time control is bought into the grazing handling*** (emphasis added by Gill).
>
> ***(Planned grazing) is not a grazing system. Anyone describing it as a grazing system is merely indicating that he has not yet understood the holistic approach to the management of all resources simultaneously, with constant monitoring and adjustment to achieve a goal*** *(emphasis added)**).***

> *Some say it (planned grazing) is a 'cell system' or the 'wagon wheel system'. Again this is totally erroneous and can only lead ranchers to costly error if they believe it and apply it as such.'*

"The foregoing statements of Savory's are unambiguous**.** Yet, in 2000, citing Savory's 1983 paper while dismissing its objections (which are consistent with hundreds of Savory's later writings and observations), including Savory's statement that he had both developed, and later rejected SDG's as unworkable, Holechek equates planned grazing with SDG systems anyway. According to him that is the 'common conception' (Holechek et al. 2000). Proceeding from that re-definition he tests not Savory's actual principles, but a mischaracterization" (Gill, 2008).

## Savory Rebuttal

Savory replies to Holechek in the June, 2000 issue of Rangelands magazine. Here Savory is unequivocal in his condemnation of their misrepresentation of his methods, as well as steadfast in his advocacy for Holistically Planned Grazing as essential to grassland restoration and the diversion of flooding, and carbon loss.

> "**The work Holechek et al. describe is unlike any range management practice I have ever advocated** (emphasis added). They claim an exhaustive research of the literature and refer to the first edition of my book. **In my writings there is nothing advocating the short duration grazing they researched** (emphasis added)**.** In fact I have consistently stated that all grazing systems and rotations, including short duration grazing, will fail....
>
> "The criticisms of Holechek et al. would be easy to ignore were it not for the serious desertification of the U.S. and appalling state of our public lands and many ranches. ...This makes the urgency of providing corrective management, too great to permit inadequately researched papers to misguide policy makers. The author's paper would have benefited from more extensive peer review." (Savory 2000)

Savory discusses the work done by himself in Africa that first proved the efficacy of restorative grasslands management through planned grazing. The study administered by the Minister for Agriculture in Rhodesia had him manage 4000 acres of former grassland that was considered depleted beyond repair. Surrounding this was 200,000 acres of similar land that was under traditional management. As he writes, the success of his methods was apparent by the second year, with perennial grasslands reemerging on the trial plots, and with only cattle as treatments. Over an eight year period they had five times as much meat per acre and three times the stocking rate compared to the surrounding acres. After trial, the land was put back to rotational grazing without a planning process, at which point the land quickly degraded, and thus was no longer able to support livestock.

> “On this land we spent a total of $1.80 per acre, trebled the stocking rate within the first year, and produced five times as much meat per acre over the next eight years through all manner of seasons. The higher meat production came from three times the stocking rate combined with higher individual performance of animals compared with all herds on the surrounding 200,000-acre control. By the second year perennial grassland had reappeared with no action other than using the livestock impact with the planned grazing I continue to advocate.”
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> “Following the first eight years this same project was then subjected to four years of rotational short duration grazing without the planning process. The land reverted to bare ground with all livestock having to be removed—a useful lesson confirming the findings of Holechek et al.” (Savory 2000)

Savory exposes what he believes to be Holechek’s lack of proper research, or simply outright bias, by showing that although Holechek must have known about the eight year trial in Africa, he chooses not to cite the report on its success, but instead only cites the study that looked at the subsequent four years, after the planning process had ended, and the land started to degrade.

> “Holechek et al., claiming to present the facts did not mention that trial, conducted on the planned grazing I advocate, although available in university libraries in the U.S.. They were aware of the unofficial trial as they refer to that last four year period after the planned grazing had been stopped (Skovlin 1987)”.
>
> “Another significant paper Holechek et al. did not refer to is '"Biodiversity As an Organizing Principle in Agroecosystem Management: Case Studies of Holistic Resource Management Practitioners in the USA" *(Agriculture, Ecosystems and Environment,* vol. 62:199-213) by Debora H. Stinner, Benjamin R. Stinner and Edward Martsolf of Ohio State University. In this study of 25 farms and ranches across the U.S. all but one reported improvement in the land and the average increase in profit was over 300%.” (Savory 2000)

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