RMC Ranching For Profit
Healthy Land, Happy Families & Profitable Businesses

By Dave Pratt

***Picking A Fight Over Rotational Grazing***

Several months ago Steve Oswald brought a paper published in the *Journal of Ecology and Rangeland Management* to my attention. The title was "*Rotational Grazing on Rangelands: Reconciliation of Perception and Experimental Evidence."* Steve told me that the premise of the paper was that rotational grazing did not produce beneficial results on rangelands. I told him I didn't think I'd waste time reading it. My inclination was to ignore it. There is a lot of noise out in academia and trying to respond to everyone who wants to pick a fight will wear you out. I told him that the people who are using cell grazing on rangelands know differently.

A couple of days later I asked Kirk Gadzia, who delivered our continuing education program at the Colorado Executive Link meeting, if he knew about the paper. He did and said it was disturbing. When I expressed my tendency to ignore it he got pretty animated and said that bureaucrats and anti-grazing activists were likely to use this to influence policy. Of course he's right. Kirk was kind enough to send me a copy of the paper.

Even after talking to Steve and Kirk, I didn't think I'd have much of a reaction to it. I was wrong. I was surprised at how agitated I got. It brought back a flood of emotion for me. The paper encapsulated a lot of the reasons I left the University. A colleague of mine and I who were working together built 26 paddocks with several water points in a watershed at one of the University's field stations. We conducted demonstrations and research in the watershed. We didn't want to get into an "us v. them" fight comparing the health and productivity of the pastures in our project to those on the rest of the field station. The watershed wasn't big enough and we didn't have the time or resources that would have been required to do the statistically valid replicated trials on a study like that.

Our premise was that a growing number of ranchers were using cell grazing, and that there wasn't any good research on impacts of various stock densities, timing of herd effect, impacts of 1 v. 2 v. 3 day graze periods on animal performance, etc. We wanted to do replicated trials within the cell, comparing those sorts of things.

We also changed the calving season from the conventional October-November season used in the California foothills to April, the period of peak forage growth at the field station. Our calves were small compared to the calves produced from the conventional calving season and would have brought us $50-100 less than if we were fall calving. But we lowered production costs by nearly $300/cow by eliminating the hay feeding and cow depreciation. (By calving out of season we could use other people's late calving and open cows they were culling as our replacements). Not making a profit would have been like trying to fall out of a boat and not hit water.

Every year we had to fight to continue our cell grazing research. The Field Station's research committee wanted us to do replicated trials comparing cell grazing to conventional management. They ultimately said that we could either continue the grazing (but would have to replicate the cell 4 times and compare it to 4 replicates of a set stocking treatment *AND* we would have to change the calving season back) OR we would have to get rid of the paddocks, and just compare the fall and spring calving seasons, but we would have to keep the supplement program constant. They either didn't understand that it was all one treatment, or they understood and just wanted to set rules that would have made our work meaningless.


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I relate this experience simply to explain some of the problems with getting useful cell grazing research done and create a context for my opinions about **"The Paper."**

***The Paper*** *(The paper can be viewed or printed from the on-line edition of our newsletter.)*
The 9 authors (so many authors you'd think it was a congressional bill) synthesize 47 studies done on rotational grazing. The authors conclude, incorrectly, that there is no scientific evidence that rotational grazing is a beneficial practice on rangelands. They apparently decided to ignore several studies showing a significant impact. For example, a study the authors failed to mention, conducted by researchers in the Department of Botany at the University of New England in Armidale, Australia, reported that desirable plant species and ground cover increased and undesirable species decreased significantly under cell grazing. They reported that continuous grazing had the opposite effect.

In spite of what the paper says, those who read it and understand cell grazing, will realize that the paper is *not* an indictment of cell grazing. It is an indictment of the inadequacy of traditional research models.

Their conclusion that rotational grazing doesn't work is right, although not for the reasons they cite. Most rotations involve fixed graze and rest periods and a limited number of paddocks. More importantly, *cell grazing* is not *rotational grazing*. However, the authors called anything that wasn't set stocking in one paddock "rotational grazing." According to them it doesn't make a difference if there are 2 paddocks (the fewest possible in a rotation) or 24 (the most paddocks in the studies they examined). That's a little like saying bowling, NASCAR, ice hockey and marathon running are all the same because they are all sports. It defies common sense.

None of the papers the authors reviewed were valid examples of cell grazing. We summarize the cell grazing principles at the Ranching For Profit School by saying you typically need 8-10 paddocks to stop overgrazing, 14-16 to achieve good animal performance and 25 or more to see rapid range improvement. Of the 47 papers they looked at only 4 had more than 10 paddocks and none had as many as 25. In fact more than half the studies had less than 8 paddocks, many with only 3 or 4 paddocks. So most of the studies actually researched "rotational overgrazing." One study did have 25 paddocks. The study was done in "desert grassland" and the average paddock size in that study was less than 1/100 of an acre (47 square yards)!

I was bewildered by several things the authors wrote. For example, they made a comment about "poorly managed continuous grazing," which, as far as I'm concerned, is redundant. The insinuation of the paragraph is that since all of the good managers are using rotational grazing and the bad ones were using continuous grazing it wasn't possible to make a fair comparison. Does it really take a rocket scientist to figure out why the good managers stopped continuous grazing?

They did acknowledge the complexity of studying grazing systems. They said that they managed the complexity by ignoring it and holding the variables constant, even though they admitted that doesn't represent the real world. Then they had the temerity to question why people, who live, work and have to make money in the real world, didn't follow the advice of researchers who don't know how to cope with real world variability and don't have to make a living from resource management. And the coup de grace: while concluding that rotational grazing was no better than continuous grazing, they admitted that continuous grazing at low stocking rates, their recommended practice, might not be profitable.

Near the end of the article the authors say, *"The direct application of research results obtained in small-scale experiments may not be entirely appropriate because the ecological processes of interest often do not scale in a linear fashion."* They also write, *"Failure to consider the time required for ecosystems to adjust to changes in management regimes may potentially mask ecosystem responses..."* Finally they say, *"Research protocol requires that grazing experiments be structured in a manner that minimizes both ecological and managerial variability to effectively test hypotheses..... These research requirements do not allow grazing experiments to necessarily mimic management activities targeting production of conservation goals at the ranch enterprise.... treatments are often applied on a more rigid schedule to ensure experimental integrity."* That's a long winded way of admitting that there are some big limitations on

being able to research grazing systems. It would have been more correct to end the last sentence with "which renders the experiments nearly meaningless."

***The Final Straw***

There's a lot in the article that got, as Stan Parsons might say, "My knickers in a twist," but the big kicker for me was this statement *"...circumstantial evidence from successful grazing managers cannot be elevated to the status of experimental data..."*

OK, let me make sure I have this right. The experience of someone who makes a living from the land and is in touch with that land and the animals on a daily basis has less credibility than research which doesn't have the scale, longevity or flexibility to "mimic" the real world? I think they got it backwards; we ought not elevate this so-called range research to the status of experience. We aren't supposed to believe our eyes, our bank accounts or the way we are feeling? It reminds me of the scene in the Wizard of Oz where Toto draws back the curtain revealing the Wizard. The Wizard replies, "Just ignore the springs that are flowing again and the increased carrying capacity, the return of perennials, the reduction in invasive weeds, the reduced input costs, the improved profitability, the overall satisfaction of the operator and the happiness of this family. The wizard of range research has spoken!"

It's been interesting to me how riled up the article made me. Can you imagine the tremendous impact it would have if researchers chose to focus on real issues in a meaningful way?

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