Re: Slope Compensation and Vodka

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From: terry (*the_domes_at_xtra.co.nz*)
Date: 08/11/04

Date: Wed, 11 Aug 2004 15:15:58 +1200

"Genome" <genome@nothere.com> wrote in message
news:SogSc.663$To5.429@newsfe2−gui.ntli.net...
>
> *"terry" <the_domes@xtra.co.nz> wrote in message*
> *news:4ReSc.11758$N77.520748@news.xtra.co.nz...*
> | *"Genome" <genome@nothere.com> wrote in message*
> | *news:HhfSc.620$Qs4.75@newsfe5−gui.ntli.net...*
> | >
> | > *"colin" <no.spam.for.me@ntlworld.com> wrote in message*
> | > *news:mBeSc.2139$K84.1781@newsfe3−gui.ntli.net...*
> | > |
> | > | *"Genome" <genome@nothere.com> wrote in message*
> | > | *news:PieSc.594$Qs4.137@newsfe5−gui.ntli.net...*
> | > | > *I'm trying to gain a bit of insight into things to do with slope*
> | > | > *compensation in peak current mode control.*
> | > | >
> | > | > *I model it, I sit and look at it.... I drink Vodka, actually I*
> *mix*
> | > *the*
> | > | > *Vodka with Cider.*
> | > | >
> | > | > *I wonder.*
> | > | >
> | > | > *Now, I've seen various explanations about the whys and*
> *wherefores*
> | > *and I*
> | > | > *don't understand them. However, as part of my website I need to*
> | > *explain*
> | > | > *it to others.*
> | > | >
> | > | > *Is there a simpler way?*
> | > | >
> | > | > *What I see is that when subharmonic oscillation occurs there are*
> | > | > *multiple solutions to the sum that's being solved by the system.*
> | > | >
> | > | > *With slope compensation those sums are reduced to one unique*
> | > *solution.*

> | > | >
> | > | > That's my take on things.... is there a way of proving it
> | > | > mathematically?
> | > | >
> | > | > DNA
> |
> | IIRC its associated with the sampled−data nature of the overall
> control
> | loop, which the standard linear analysis conveniently ignores. Ray
> Ridley
> | did his PhD thesis on basically including the effects of
> discretisation in
> | the analysis. Others have approached the subject from slightly
> different
> | avenues, but when you include this sampled−data behaviour the
> duty−cycle
> | dependant subharmonic oscillation pops out the wash. And (kinda
> obviously)
> | it doesnt miraculously disappear when D = 0.4999999, rather the
> effects
> | gradually reduce, disappearing at around D=35%. Normally the
> explanation is
> | a nice hand−wavey description of what takes place DURING this
> oscillation,
> | and how to fix it, rather than saying WHY it takes place.
> |
> | I went to one of his seminars once, and found it to be, overall, not
> much
> | practical use − it seemed to be mostly a pitch for his FRA's. The
> maths was
> | interesting, but like everyone else he didnt give usd all the
> equations we
> | need to close the loop on the half−dozen or so different converters
> that
> | people in practice build. A shame really, cos it probably all fits
> onto a
> | single page....
> |
> | Cheers
> | Terry
> |
> |
>
> That's a bum. I can't say why it takes place but I can believe why it
> happens.
>
> I don't believe in an analysis that talks about 'stability'..... or dare
> I say sampled data behaviour.

well if you do a simulation including all the relevant behaviours, the
problem occurs. When you build a real one, it occurs. If you then "simplify"

the real circuit for a control loop analysis, the problem doesnt "appear". therefore the "simplifications" are the problem. Discretisation MUST affect the control loop, for the entirety of (1−D) the error amp cant do anything at all. It all seems to boil down to the "choice" of modulator transfer function

There is a nice (but old) paper looking at a leading−edge modulated converter (a real space app) that in practice was stable, but the conventional analysis showed to be unstable so some folks at VPEC asked why. turns out that modulator (in the middle of the loop) has an effect.....(oops Ridley was involved there too).

I have read quite a few of his papers, many of which are shit (some of which are even wrong).

>
> *I do believe in multiple solutions without slope compensation that get*
> *reduced to a unique solution with slope compensation. Its just the*
> *method required to prove it.*
>
> *But I wouldn't tout that one around.*

The real question is: is subharmonic instability a function of the power stage or the controller. There are indeed topological changes that occur within the power stage (eg DCM/CCM) but these arent involved in subharmonic oscillation. lets assume it aint chaos (which might not be a good assumption), then the controller really looks like the culprit. And the fact that Fsmps is used to cure it is IMO the last nail in the coffin of the controller. We know real accurately how the error amp performs, and it aint got anything to do with Fsmps, but the flip−flop sure as hell does.

Moving the crossover frequency of the closed loop doesnt affect it, so it aint got anything to do with nyquist. Changing Fsmps itself doesnt help either − it happens at 10kHz just as readily as at 500kHz. But change the modulator from leading to trailing edge and it DOES have an effect. Ergo, my hand−wavey argument "proves" the modulator is "at fault" − or at least the models used are the problem. And what do all the papers studying this do? why they pick different (so−called "better") models for the modulator. I cant be bothered looking, but some guys in South America have written some great papers on this in the last decade.

>
> *Smidley's in it for the bucks, has to roll his shirt sleeve up to take a*
> *shit and someone else washes his trousers for him.*

that much was clear from the "course" − I gleaned a few useful facts but mostly it was like the discovery channel − lots of waffle, but fuck all hard data. science for sissys. I didnt pay for it though, and ended up having a couple of wild nights out on the piss, a−la expense account :)

>
> *DNA*

Cheers
Terry